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AMERICAN SOCIOLOGICAL REVIEW
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Laslett
Social Change and the Family

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Gartrell
Status, Inequality and Innovation

Wright & Perrow
Insurgency of the Powerless

Hamblin, Hout, Miller & Pitcher
Arms Races

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nneman & Pampel
American Perception
of Class and Status

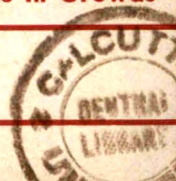
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Scientific Productivity
and Reward Structure

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Democracy and Equality
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Social Exchange



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Gan & Leon
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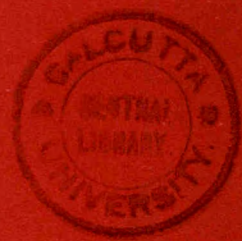
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Schiffrin
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Presidential Address: Countercultures and Social Change

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"Situation" versus "Frame"

Marks

Multiple Roles and Role Strain

Zerubavel

The Sociology of Time

Stolzenberg & D'Amico

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Hamilton

Chinese Consumption
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Changes in U.S. Occupational
Structure

Light

Numbers Gambling

Sørensen

Inequality
and the Process of Attainment

Denzin

The American Liquor Industry

Comment and Reply

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AMERICAN SOCIOLOGICAL REVIEW

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ITEMS

February, 1977

■ ANTHONY R. HARRIS (*Sex and Theories of Deviance*) is Assistant Professor of Sociology and Director of the Joint Laboratory in Social Interaction and Communications Studies at the University of Massachusetts at Amherst. He is presently analyzing data on caste and class differences in self-esteem with Anthony Conto. His continuing interest is in the effects of gender on behavior. Harris and Gary Hill have completed a study of changes in the gender-patterning of crime in the U.S. during the last several decades;

he is working with Gary Hill and Lenore Kligman on an experimental study of gender differences in the impact of equity norms on decision making; and he is writing a monograph on the roles of gender, caste and "type-scripting" in deviance.

■ VICTORIA LYNN SWIGERT (*Normal Homicides*) is Assistant Professor of Sociology at the College of the Holy Cross, Worcester, Massachusetts. The present paper is part of an ongoing study of differential treatment in the legal process

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AMERICAN SOCIOLOGICAL REVIEW

February 1977

Volume 42 Number 1

SEX AND THEORIES OF DEVIANCE: TOWARD A FUNCTIONAL THEORY OF DEVIANT TYPE-SCRIPTS *

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ANTHONY R. HARRIS

University of Massachusetts, Amherst

American Sociological Review 1977, Vol. 42 (February):3-16

Given available evidence on official and hidden criminality, conceptual neglect of the sex variable, not simply of women, is seen as having dire consequences for most contemporary criminal deviance paradigms and, consequently, as providing the major failure of deviance theorizing in this century. The outlines of a new paradigm are introduced, "the functional theory of deviant type-scripts," which suggest that everyday expectancies for deviance—which link types of actors to types of roles and which serve dominant social interests—at once account for our professional failure to include the sex variable in theories of criminal deviance and, more importantly, provide the single strongest theoretical account of the very striking cross-cultural difference in male and female criminality.

INTRODUCTION

The failure to study noncriminals proved a disaster to early work on criminal deviance. Putatively, the failure was a methodological one; in looking for a relationship between cranial shape and criminal propensity, phrenologists looked only at prisoners, thereby begging the question of how this purported relationship was distributed in the population at large (cf. Vold, 1958). The phrenologists' error, however, was almost certainly an artifact of a more fundamental conceptual blindness—the inability to imagine that rates of the "lower propensities" might be higher in the noncriminal population at large than among prisoners. Curiously, modern deviance theorists have applied the lessons learned from this sort of mistake only selectively. As a consequence, the general power of contemporary criminal deviance theory is only marginally greater than it was in the phrenologists' day. I am referring to a critical weakness resulting from the continuing failure to

consider women and, consequently, the sex variable in such theory.

This failure is more than merely methodological, precisely because it means that purportedly general theories of criminal deviance are now no more than special theories of male deviance. This is no small or easily correctable problem. Its solution does not depend simply on more studies of female deviants, on adding a corollary here or a specification there in the current special theories of male criminal deviance and then applying them to women, or on simply reevaluating the current evidence. The solution, or at least its beginnings, as I shall try to point out, most probably lie in the realization that our present conceptual blindness, like the phrenologist's, is deeply rooted in everyday assumptions about *who does what, including deviance*, in a society. Ironically, it is the distribution of these background norms about deviance which, I believe, at once accounts for our professional failure to include women in theories of criminal deviance and, more importantly, provides the single strongest causal account of the empirical differences in male and female criminality.

Such differences are striking indeed: sex appears to explain more variance in crime

* The author wishes to thank Gary D. Hill especially for his advice and encouragement. Thanks are also due other colleagues at the University of Massachusetts at Amherst, several anonymous ASR readers and Morris Zelditch, Jr. for their useful comments on this manuscript.

across cultures than any other variable. This appears so *regardless* of whether officially known or hidden ("true") rates of crime are indexed (Harris, 1976).¹ It is also worth stressing that, with the exception of age, the major social-structural predictors hitherto built into various theories of criminal deviance—class, race/ethnicity, area, and intact family background—are all strongly multicollinear. Sorting out their independent and interactive effects on crime has proved extremely difficult. This empirical morass has been paralleled by a theoretical one involving variations on the theme of structural disadvantage. However, the sex variable, to which virtually no attention has been paid, is only a trivial covariate of these major social-structural variables. The potential theoretical and empirical strategic advantages of this observation clearly have been neglected.

The sex variable potentially provides the clearest of all sociological beginnings in criminal, if not more general deviance theory (that the absolute, though perhaps not relative importance of the sex variable in predicting crime has varied and will continue to vary from time to time and place to place is an issue to which I will return). Unfortunately, however, deviance theorists have, *for all practical purposes*, focused on that part of the social landscape producing the highest criminal yield—males. Similarly, as the Secret Service agents guarding the American president's life traditionally have been tied to monitoring potential male assassins (and only through recent events have had to perhaps literally double their efforts by looking for potential female assassins as well), so too have sociologists been tied to monitoring and theorizing only about male criminal deviance.

For practical purposes akin to those of Mills' (1942) "professional social pathologists," this tie is one thing. For theoretical

purposes, however, it is quite another. Commitment to males only has meant the unwitting trade of general criminal deviance theory for the gain of practical special theories tied to practical everyday interests and expectations. For theoretical purposes and, if we are to believe in the importance of theories, rather ironically for *effective* practical purposes as well, the omission of the sex variable in deviance theorizing is tantamount to the guarantee that we can see neither the forest *nor* the trees.

In what follows, I shall examine what are commonly taken to be the major paradigms in criminal deviance and argue that the inclusion of the sex variable has dire effects on some of these paradigms, but less dire effects on others. Regardless of these consequences, it will be stressed that existing theories ought no longer be considered general theories unless and until they not merely "take account" of the sex variable but, instead, start with it. This issue will be addressed in the particular form: how, theoretically, can the sex variable be brought to bear as a potential distributor of the major conceptual elements in contemporary criminal deviance paradigms? One of the major goals of the analysis will thus involve identifying a parsimonious set of conceptual elements to describe such theories. These elements will be used to develop a propositional notation both specifying the theories and providing the basis for examining them once the sex variable is introduced.

A second major goal will be to locate what I consider the primary conceptual target of labeling theory in "*true*" and not, as presently conceived, in "*hidden*" rates of deviance. This viewpoint will be clarified through developing the outlines of what I will call "a functional theory of deviant type-scripts"; toward this goal, it will be argued that the theoretical notion of greatest potential use in the necessary reconstruction of deviance theory is not one involving the idea of means or of ends or of perceived opportunity but, rather, is one involving the idea of self-attribution. This claim will be seen as consonant with the general labeling perspective on deviance on the condition that the perspective

¹ This point is made with respect to American data. When data from less industrialized societies are considered, the point is more striking. Prior to modernization, data from third world societies yield sex ratio ranging from 900:1 to more than 20,000:1 (Nettler, 1974).

be revised and refined in several major respects.

THE MAJOR ELEMENTS IN CRIMINAL DEVIANCE THEORY AND THE CONSEQUENCES OF INTRODUCING THE SEX VARIABLE

A major problem in evaluating sociological theories often lies in the lack of specificity of the key conceptual terms in the theories. In order to specify the major criminal deviance paradigms for present purposes, I identify what I believe to be a theoretically parsimonious set of four conceptual elements, the social distributions of which are seen as providing the basic concerns of criminal deviance theory. These elements or their variants are used with great regularity in contemporary social psychology and together are held to be basically sufficient for developing a propositional notation by which the paradigms in question may be specified, potentially operationalized and empirically assessed.

The first such element is *Goal Utility*: the subjective reward characteristics or value of a particular type of behavior choice *if* it pays off. As such, goal utility (GU) is the contingent or possible (extrinsic) payoff value of choice—in frequent sociological usage, an “end.” Usually, but not necessarily, this kind of utility has extremely high social communality and may be easily traded. High communality utilities, such as money or material goods, may be thought of in Weber’s (1947) terms as having “primary value” in a society.

The second element is *Behavior Utility*: the subjective reward characteristics or value of a particular type of behavioral choice (intrinsically) received *when* the behavior is performed—in frequent sociological usage, a value inherent in behavior as an “end in itself.” As such, behavior utility (BU) necessarily is pegged to one type of behavior rather than to another. Usually, but not necessarily, this kind of utility has weaker social communality than goal utility and is not easily traded. Relatively high behavior utility, such as that attached to “good sportsmanship” or “getting high,” may be thought of in Weber’s

terms as having “secondary value” in a society.

The third element is *Probability*: the subjectively perceived chance of attaining utility (GU or BU). As such, probability (P) reflects the perceived chance that a particular type of behavioral choice will lead to gratification. The last element is *Self-Attribution*: an actor’s subjective inference that self is a member of a class of actors “likely to perform behavior Y.” As such, self-attributions (SA) are meant to reflect the subjective sense that because one is a member of the class of actors X, one expects oneself to choose a particular type of behavior at a higher rate than members of the class non-X.²

Having defined these terms, we turn to two questions: (1) how are the terms distributed in contemporary criminal deviance paradigms and (2) what happens theoretically when the sex variable is introduced in the analysis? It should be remembered that *the theories we will examine are claimed to be general. As such, their scrutiny cannot therefore be vitiated by the counterclaim that they apply only to men.*

Probability

The probability element represents perhaps the major conceptual unit in criminal deviance theorizing. It is usually tied to notions of goal utility or primary value, to consequent assumptions that goal utility is a fixed sum in a society and, thus, to theorizing about economic or property-related crimes. Two basic variants on this theme are encountered in theories of structured strain and differential opportunity. In each case, one or both of two kinds of probability, the perceived probability of obtaining goal utility given legitimate (L) means (i.e., $P(GU|L)$) and the perceived probability of obtaining goal utility given criminal (C) means (i.e., $P(GU|C)$), are seen as distributed by social-structural factors.

Structured strain. In this basic variant,

² Alternatively, this notion may be expressed in terms of subjective probability (P). Thus, given the newly acquired self-attribution that one is “criminal” as opposed to “straight,” P of criminal behavior would be expected to increase.

the probability of attaining goal utility by legitimate means is hypothesized as distributed by such social-structural factors as class or caste (cf. Merton, 1968). From this viewpoint, actors with objectively lower chances of attaining goal utility via legitimate means are very likely to perceive these low chances realistically. It is also characteristically assumed from this viewpoint that such actors have relatively higher perceived access to goal utility via criminal behavior. In themselves, these two conditions are not seen as sufficient to produce criminal behavior in structured strain theory. A second condition must be added—that actor find little or no intrinsic utility in the choice of legitimate means (i.e., maintain low $BU|L$). Consequently, it is reasoned that:

when

$$(1) P(GU|C) > P(GU|L)$$

(and where $P(GU|L)$ is distributed by social structure)

and

$$(2) BU|L \text{ is low}$$

then

$$(2) \text{Probability of } C > \text{Probability of } L.^3$$

It is extremely important to observe that, in this paradigm, it is only the probability of goal utility via legitimate means which is seen as systematically distributed by social-structure, and *neither* the probability of goal utility via criminal means *nor* the behavioral utility attached to legitimate means. It is tacitly assumed, that is, that *everyone* in a society has equal access to GU via C, and that little can be predicted about the structural distribution of commitment to legitimate behavior utility.

In order to maintain its logical structure once the sex variable is considered, structured strain theory would seem to require that its major element, $P(GU|L)$, be distributed by sex. From this viewpoint, how-

ever, the theory becomes counterfactual: since women appear to have lesser access to goal utility via the array of legitimate opportunities available than men do, women should show a higher crime rate (if only for economically related crimes) than men. Clearly they do not.

Differential opportunity. In this variant, often confused with structured strain (but actually overlapping it), *both* the probability of attaining GU by legitimate behavior *and* that of attaining GU by illegitimate behavior are seen as distributed by such social-structural factors as class, caste, ethnicity and area (cf. Cloward and Ohlin, 1960). From this viewpoint, as that of structured strain, actors with low objective access to GU via L are assumed to have realistic perceptions. As Short et al. (1965) have pointed out, once "low access" actors review their chances for goal utility via legitimate behavior, differential opportunity theory predicts a next step involving the cognitive review of perceived chances for goal utility via criminal means. Where this latter perception was invariantly distributed for structured strain theory, for differential opportunity theory it is seen as distributed by such variables as class, caste and neighborhood. Put simply, the perceived availability of primary values through criminality is now understood to vary by members' locations in social space. Consequently, it is reasoned that:

when

$$(1) P(GU|C) > P(GU|L)$$

(and where both terms are distributed by social structure)

then

$$(2) \text{Prob } C > \text{Prob } L.$$

Since now the probability of GU via criminal behavior cannot be assumed to be uniformly high for everyone, a more reasonable accommodation of the sex variable is possible. To make the strongest case for this viewpoint, where women have traditionally had lower access to GU via legitimate behavior than men, it would seem they have also been traditionally locked into the home and, consequently, out of access to criminal means. Thus, while women may generally maintain lower lev-

³ The terms "Probability of C" and "Probability of L" refer to objective or performance probabilities (henceforth, Prob C and Prob L) rather than to subjective probabilities (P). Though throughout the rest of the paper I will rely on deductive statements (Prob C > Prob L), propositions might alternatively be expressed as: the greater the X the greater the Y (e.g., Prob C).

els of $P(GU|L)$ than men, they also maintain even lower levels of $P(GU|C)$ than men. Given this, the differential opportunity approach would seem capable of accommodating the sex variable.⁴ Two major caveats, however, suggest otherwise.

First, for women the probability of goal utility or primary value through criminal means may well be lower than that for men because of women's traditional location in the home. This limitation, however, should *not* mitigate substantially the probability of committing violent crimes. As official and hidden data reveal, the opposite is true. Where we would on the basis of differential opportunity theory expect a lower sex ratio for crimes of violence than for economic crimes, the sex ratio for violent crimes is approximately *double* that for property crimes. Clearly, in its own right, the differential perception of criminal opportunity cannot account for this.

Second, even if economic crimes are considered the only type of crime within the theoretical purview of the differential opportunity perspective, there are other hurdles the perspective must face. In recent years, the sex ratio has decreased markedly for property crime arrests in general, and for white-collar economic crimes in particular. Although this change has, in effect, been interpreted as support for differential opportunity theory (e.g., Simon, 1975), the sex ratio for these crimes has dropped more quickly in the past few decades for people under 18 than for those over 18 (Harris and Hill, 1976). This observation would seem to suggest quite strongly that the opportunity to commit economic (including white-collar) crimes, assuredly greater for those over 18 rather than under 18,⁵ does not successfully account for age variance in the changing sex

differential. For, if the differential opportunity approach were correct, we should clearly expect a more marked decrease in that differential for those over 18 and in the labor force than for those under 18.

Behavior Utility

Theories built on the distribution of behavior utility are, in effect, concerned with social variance in secondary or special values intrinsically attached to specific classes of behavior. In the pursuit of desired goal utility or simply as an end in itself, behavior utility becomes a potential discriminator in the choice of one type of behavior over another. From this viewpoint, and following Cancian's (1975) line of argument about norms, behavior utility provides the affective core of "normative commitment" and the consequent gratificational basis for acts which document social identities. The social communality of behavior utility, especially as it concerns the binary choices of legitimate and criminal behavior, is of particular interest to deviance theory. How much intrinsic satisfaction, for example, is gained by different classes of actors for this particular act of conventional behavior, such as returning excess change at the cash register, or for that particular act of criminal behavior, such as stealing a car? Three basic paradigms which focus on the distribution of behavior utility are differential association, subcultural difference and control theory.

Differential association. A major feature distinguishing differential association from subcultural and control theory is its absolute neutrality with respect to probability considerations. Here, concern lies purely with utility. Though the differential association approach potentially has much to do with goal as opposed to behavior utility, I include it here because of its ability to illumine issues involving the relatively lower social communality of behavior utility than goal utility. In this variant of BU theory, the focal concern rests on "definitions" favorable to crime. Because this idea is not specified, it is both problematic and of broad potential significance. I will interpret the notion to mean subjectively favorable perceptions of utilities as opposed to

⁴ Simon (1975) appears to make this argument, for example, in explaining changing patterns in female arrest rates.

⁵ This rests on the assumption that there are more opportunities for occupation-related economic crimes for women over 18 and on the additional assumption that the opportunity to commit larcenies (such as shoplifting) is not significantly different for older females tied to households as opposed to younger females not so tied.

costs or disutilities (D) tied to criminal behavior. Thus, when Sutherland and Cressey (1966:81) speak of the "excess of definitions favorable" to crime "over definitions unfavorable" to crime, I understand this to mean the ratio of (a) goal+behavior utility via criminal behavior to (b) goal+behavior disutility via criminal behavior. Thus, it is tacitly reasoned:

when

$$(1) (GU|C+BU|C) > (GD|C+BD|C)$$

(where both terms are distributed by "association")

then

$$(2) \text{Prob } C > \text{Prob } L.$$

Differential association theory thus argues a simple cost-benefit analysis of criminal choice, maintaining that on the individual level the outcome will vary by actor's "association" with people and/or ideas on one end of the utility/disutility calculus or the other. In reality, this position is that of classical utilitarian criminology, with few conceptual gains and a large loss of precision. Differential association theory, moreover, leaves unspecified the social-structural distribution of its theoretical elements, except for the sociologically uninformative notion of "association," and thus bears little risk of falsification. Precisely because of its unrestricted character, the ability of differential association to stand once the sex variable is brought in cannot be questioned. From this viewpoint, the issues are: (a) do men have higher ratios of favorable to unfavorable definitions of crime than women and (b) if so, how and why? My suspicion is that the answer to the first question is "yes, men do." But this assumption neither locates one in any particular theoretical camp, nor helps in keeping the etiological question from being begged.

Subcultural differences. In this variant on the theme of behavior utility, the strength of BU tied to criminal behavior (though occasionally the strength of BU tied to legitimate behavior as well) is seen as distributed by such social-structural factors as caste, class and age (cf. Cohen, 1955; Sykes and Matza, 1957; Miller,

1958).⁶ From this viewpoint, subcultures formed on the basis of salient social identities have the power to construct realities incorporating, disavowing or neutralizing mainstream behavior utilities (the affective core of "normative commitment"). One critical factor separating subcultural difference from differential association theory is the former's implicit focus on comparative levels of BU via C and BU via L for individuals. The second, more important factor lies in subcultural theory's concern for probability. In these terms, a strong point of the subcultural approach rests in the functionalist assumption that low perceived access to legitimate goal utility (i.e., low $P(GU|L)$) leads classes of structurally disadvantaged actors to generate higher levels of behavior utility for criminal choice and/or lower levels of behavior utility for legitimate choice. Consequently, it is reasoned that:

when

$$(1) P(GU|C) > P(GU|L)$$

(where $P(GU|L)$ is distributed by social structure)

then

$$(2) BU|C > BU|L$$

(where $BU|C$, and possibly $BU|L$, is distributed by subcultural access)

thus

$$(3) \text{Prob } C > \text{Prob } L.$$

In order to maintain its coherence once the sex variable is considered, subcultural difference theory would need to argue: (a) there is a vast female and/or male

⁶ Cohen's (1955) theory really represents a special case of subcultural difference theory. The psychodynamic mechanism of "reaction formation" is relied on to argue that those with low access to GU via L will cognitively transform that utility, along with conventional utility tied to legitimate behavior (BU via L), to something actively *disvalued*. Those undergoing this transformation should be expected to try to act anti-thetically to GU and BU via L—to engage in behavior somehow negating that utility which cannot be had. Artificially, this behavior is defined as criminal (e.g., vandalism). The theory is unusual, especially in that it posits systematic cognitive relations between probability and utility.

subculture (or a vast set of such subcultures) and (b) this subcultural variance distributes the major element of $BU|C$ and, possibly, the minor element of $BU|L$. Since from this viewpoint, however, subcultures represent the responses of the disadvantaged to blockage from goal utility via legitimate means, we should expect women to develop higher levels of criminal, relative to legitimate, behavioral utility than men and, consequently, higher levels of criminality than men. Clearly, the data indicate the exact opposite.⁷

Control. In this last variant of BU theory to be considered, preference for behavior utility via legitimate behavior is always assumed to be unequally distributed, but not usually by social structure. In the version of control theory advanced by Nye (1958), it is implicitly understood that it is *always easier* to attain goal utility via criminal than legitimate means (the idea of the "fast buck"). This assumption forms a crucial backdrop for the question: why, then, do people ever choose the straight and narrow? For control theory the answer lies in the intrinsic behavior utility of legitimate choice. Hirschi's (1969) version of control theory postulates that commitment to "conventional" activities (such as sports) and "conventional" others (such as parents) provides the major social distributor of $BU|L$. Though one is not sure of how such commitment, in turn, is distributed by social-structure, it is consequently and tacitly reasoned that:

when

(1) $P(GU|C) > BU|L$

(where $BU|L$ is distributed by conventional "commitment")

(2) regardless of $P(GU|C)$ versus $P(GU|L)$ comparisons
and/or

$GU|C$ versus $GU|L$ comparisons,

then

(3) $\text{Prob } C > \text{Prob } L$.

⁷ If subcultural difference theory were to drop the functionalist claim that probability considerations involving low access to GU via L lead to the formation of deviant subcultures, it might stand in a better theoretical position with respect to the inclusion of the sex variable.

The major theoretical tension in the control paradigm is *across* the *metrics* of BU via L and of $P(GU|C)$: if the pull toward criminality is strong (which is always so because the probability of goal utility via crime is always assumed to be high), then the only possible counterforce resides in the extra-goal utility and extra-probability considerations inherent in commitment to legitimate behavior utility. It is worth noting that, in addition to having little to say about the social-structural distribution of behavior utility for legitimate behavior, control theory also has little to say about the social-structural distribution of behavior utility for criminal behavior (which term does not even count as an element in control theory). For this paradigm to claim generality and handle the inclusion of the sex variable, it would have to be argued that for juveniles *and* adults, sex distributes higher levels of commitment to conventional activities and others for females than males *and*, as a consequent function of this, higher levels of legitimate behavior utility for females than males. Though, perhaps not contraindicated by much existing research or theory, the assumption that females are more committed to conventional activities than males is not, I think, a strong one, especially when adult males and females are considered.⁸ Be this as it may, while control theory potentially provides a reasonable account of the sex difference in criminality, as differential association theory, it is still incomplete for it begs the questions, how and why?

Self-Attribution

Theories built on the distribution of self-attribution can be clearly separated from the preceding ones. Where all the preceding theories rely on some notion of subjective rationality or calculation in the production of criminal choice, theories built on the notion of self-attribution need not do so. What is at stake in such theories,

⁸ In a study of college students, Silberman (1976) reports *no* significant difference between males and females on levels of "moral commitment"—despite the simultaneous finding that the males report significantly higher rates of criminal offenses than the females.

instead, is the question of whether—through rational or extra-rational considerations—an actor perceives herself or himself as “the type of person expected to do L or C.” From this viewpoint, self-attribution which produces deviance has the propositional logical form, “since I am a member of the class of people ‘X,’ then I expect myself to produce more of behavior ‘Y’ than nonmembers of ‘X.’” This group of theories we identify by the variants “containment” (or self concept as insulator) and “labeling.”

Containment. In this version of self-attribution theory, actor's perceptions of self as (a) a person likely to get into trouble with the law and (b) a person not likely to get into trouble with the law are seen as distributed by socialization practices (Reckless et al., 1956). In a major respect, containment theory parallels control theory. Both assume an *a priori* pressure towards deviance which, if not checked, would result in a visibly high choice of criminal behavior. Where control theory, however, finds the source of the individual pressure toward criminal deviance in invariant probability considerations (see above), containment theory finds such pressure distributed by neighborhood or area.

Containment theory originally was introduced in order to close an apparent gap in probability theorizing, that is, to explain why some slum boys rather than others became criminal. As such, containment theory implicitly holds that the “criminal impulse” (either P or U, one cannot be sure) is distributed by neighborhood. But, as would also be the case for control theory, explaining the impulse to deviance is insufficient in accounting for the nondelinquent in the high delinquent area. One also needs to account for the checking of the impulse. Thus, in an equally important comparison, where control theory finds the “check” on the criminal impulse in the individual's intrinsic commitment to legitimate behavior utility, containment theory finds a parallel “check” in self-attributions which vary by socialization practices. Such attributions have the form $S \supset L$ (“self (S) implies legitimate behavior”) or $S \supset \sim C$

(“self implies noncriminal behavior”). Thus, it is reasoned:

when

- (1) $P(GU|L)$ or $GU|L$ is low
(where either term is distributed by neighborhood)

and

- (2) $S \supset C (=SA)$
(where SA is distributed by socialization)

then

- (3) $\text{Prob } C > \text{Prob } L$.

In a (social-psychologically) more rigorous form and given a more fully articulated approach to the social-structural distribution of its theoretical elements, containment theory would seem to provide a potentially powerful basis for handling the inclusion of the sex variable. Self-attributions are very clearly tied to and distributed by sex in this, if not in all societies. One such attribution with extremely high social communality may very well be the belief, held by men and women, that men are likely to get into trouble with the law; another may well be the belief that women are not. This approach, consonant with the reasoning that behavioral choice involves the extra-rational alignment of behaviors with self-perceptions (cf. Bem, 1967), would provide a “motivational” account of sex differences in criminal deviance free of the problems encountered in probability theories, fully in line with existing data and theoretically capable of subsuming control theory's necessary assumption that women maintain higher levels of legitimate behavior utility than men. (If, that is, women maintain higher levels of $BU|L$ than men, this is an affective artifact of self-attribution rather than its cause.) On the other hand, while containment theory may well offer the best theoretical elements so far for understanding the possible “how” of the etiology of sex variance in crime, it has little to say about the “why.”

Labeling. In this variant on the theme of self-attribution, actor's perception of self as a person likely to commit deviant behavior is seen—unlike the view in containment theory—as distributed by social structure, though in complex fashion. Two

kinds of interrelated social-structural phenomena form this distributive basis for labeling theory: (1) actor's relative social power or status and (2) contingent on this, actor's ability to counteract potential contact with official agents of social control. Crucial here is the assumption that what is distributed by such processes is *not* what Lemert (1951) has called "primary deviation"—in which *everybody* engages—but, rather, is "secondary deviation"—a form of deviance providing the organizing principle of behavior and in which only *some* engage. From this viewpoint, the proper criterion variable in the analysis, secondary deviance, is seen as a direct function of contact with, and labeling by agents and agencies of control. The power to resist such contact is argued by labeling theorists to vary by correlates of social stratification, including caste, class and age. The intervening process between official contact and secondary deviance is to be found, from the labeling viewpoint, in the processually induced self-attribution that self (S) is a member of the class of people called deviants and, since this is the case, in the consequent alignment by S of deviant actions with deviant self-attributions. With respect to the criterion phenomenon of secondary (and *not* primary) deviance, it is thus reasoned:

when

(1) $S \supset C (=SA)$

(where the term is distributed by the interaction of S's social status and contact with social control)

then

(2) $\text{Prob } C \supset \text{Prob } L$.

Containment theory locates the source of self-attributions which hinder or facilitate deviance in a rather amorphously identified area of "socialization" orthogonal to social structure; labeling theory locates the source of the deviant self-attribution in the interaction between actor's social power and contact with agents of social control. From this viewpoint, conventional labeling theory might seem capable of accommodating the sex variable through the assumption that women have less contact with agents of social control than men,

consequently weaker deviant self-attributions and, finally, lower rates of criminal behavior. The problem, however, is that, in the conventional sense, men have greater social power than women. Labeling theory would therefore seem obligated to the expectation that men ought to have greater ease in negotiating official labeling and, thus, to the counterfactual prediction that men have lower rates of criminal deviance than women. As I shall try to argue directly, I believe this otherwise fatal problem for labeling theory is tied to its usual and unnecessary insistence that only secondary, rather than primary deviance is of proper theoretical concern, and that actor's contact with social control is best predicted by actor's social power rather than "type."

LABELING THEORY REVISED: TOWARD A GENERAL FUNCTIONAL THEORY OF DEVIANT TYPE-SCRIPTS

I contend that any powerful general theory of criminal deviance, if not deviance more broadly, is a theory involving the stratification of behavior and identities. From this viewpoint, the functional preservation of social dominance may be seen as resting on the assignment of readily identifiable social "types" to classes of social locales and locale-specific roles. In American society, the assignment of blacks to the ghetto, women to the home and poor to the factory represent examples which have met the function of preserving white, male, middle-class dominance. Homologous legal, educational and occupational structures support such assignments and, at the level of group cognition, provide powerful expectancies by which actors come to assign themselves and others to limited classes of behavior according to social "type." Such expectancies—which specify broad behavioral sequences as well as type-to-role linkages—are referred to here as *type-scripts*. It is maintained that these scripts have important extra-logical implications, held with high social communality with respect to the perception of classes of behavior it is *likely*, *possible*, *unlikely* and *impossible* for particular types of actors to perform.



Unlike scripts for the theatre, which very precisely delimit word and action for a role but which do not specify who is to play it, type-scripts are seen not as delimiting the specific content of social roles but as specifying the types of actors who are to play them. This is not to suggest that type-scripts align a type of role with only one type of actor, or a type of actor with only one role. Rather, it is to suggest that such background expectancies align particular sets of actor types with particular sets of social roles.⁹

From the objective viewpoint, type-scripts are "normative": regardless of the social distribution of affect held for them, they would appear to have relatively high cognitive communality across both dominant and dominated groups.¹⁰ From the subjective viewpoint, type-scripts are informative since they provide not only a limited array of role choices but also the most salient bases for a limited set of possible social identities. In these terms, be-

sides aligning types of actors with possible classes of legitimate behaviors and identities, type-scripts also are seen as aligning types of actors with possible classes of deviant behavior and identities. One dominant order-maintaining function of type-scripts, then, involves specification of what types of actors are to commit what types of deviance in a society and—perhaps more illuminating—what types of deviance are seen as unlikely or "impossible" for other types of actors to commit.

Several examples illustrate the point. It is strongly type-scripted in American society that street crimes represent the preserve of blacks and the poor. From a somewhat different slant, it is (or was) scripted that it is nearly "impossible" for the highest officials in the land to act criminally. Similarly, and perhaps more relevant to the present analysis, it is (still) type-scripted that it is unlikely or "impossible" for women to attempt assassination, robbery or rape.¹¹

Based on the assumption of role assignment in the interests of the socially dominant, a functional theory of deviant type-scripts would maintain that, over time, deviant type-scripts which preserved dominant interests in particular role arrangements also would be preserved. From this perspective, in order to be functionally effective, deviant type-scripting must meet a special condition: *the existence or filling of*

⁹ It is proposed that this linking function is best seen as probabilistic, continuous and multi-conditional. In these terms, it would be possible to observe the conditional probability level a group held *vis à vis* the chances that a particular type of actor X—such as a woman—would hold a particular type of role Y—such as a bank presidency (i.e., the conditional "front" probability, $P(Y|X)$); or that the role Y would be held by the actor X (i.e., the conditional "back" probability, $P(X|Y)$). As suggested, such perceived probabilities are best approached as multi-conditional: while most members of this society undoubtedly hold a very low probability that a bank president would be female, there is undoubtedly a still lower perceived probability assigned the type-script of a bank president being both female and black. (In combination, however, conditional probabilities may be interactive rather than simply additive.)

¹⁰ It is important to recognize that the aligning linkages provided by type-scripting may be at considerable variance with empirical probabilities. As anonymous reports on criminality imply, "true" rates of deviance for given types of actors are often out of line with public representations of these rates. This may be as true for public under-representations of white-collar crime as it appears to be for white, middle-class delinquency. Incongruity between alignment probabilities in type-scripts and actual empirical probabilities is not seen as a random phenomenon but, rather, as systematically related to the maintenance of dominant interests by agencies of social control.

¹¹ As recent attempts on the life of the American president have pointed out, it is possible if not easier for women to engage in assassination. Similarly, it is clearly as possible for women to commit robbery. Though most people are aware of the "equalizing" effects of handguns, the countervailing script against female robbery has it that females would be overpowered in robbery attempts, even if they carried guns. The power of this deviant script or, rather, of its absence is strong—so strong as to preclude the realization that, even if strength were the issue, women can rob other women. Perhaps the strongest illustration of deviant scripting (or its absence), however, involves the notion of rape. Present scripting holds that rape involves vaginal penetration. On this assumption, male rape is ruled out by definition. Clearly, alternative sexual violations consonant with the more general moral principle of rape cannot be ruled out as impossible for females to commit against males or, for that matter, against females.

legitimate roles vacated by those actually assigned deviant status must not threaten the institutional hegemony of the socially dominant. Should this role-replacement condition not be met and the threat arise otherwise, the potential for broad societal script disarray would be great and the status and power advantages of the dominant would be severely challenged.

In these terms, ghetto males lost from legitimate roles (e.g., fatherhood) to prisons through crime—though a real loss to the black family and a potential loss to the abstract interests of the commonweal—are by no means a threat to the ongoing economic, political and symbolic interests of the socially dominant. (As literate whites can now testify, family structure in the ghetto is “matriarchal anyway”—the vacated role will presumably be filled easily by the black woman.) Similarly, given high rates of blue-collar unemployment, social members lost from blue-collar roles to prisons through crime are by no means a threat to the economic, political and symbolic interests of the socially dominant. (As many literate social scientists can testify, work in blue-collar and intensive labor occupations is to a high degree “functionally interchangeable.”) In neither of these cases does the existence or filling of the vacated role pose a threat to dominant interests.

The reassignment of women from homes to prisons, however, does *not* meet the role-replacement condition. Male dominance in occupational, educational, political and legal institutions is not served by allowing the development of type-scripts which lead to putting women in jail. Rather, the prime structural mainstay of male institutional hegemony has been the continued assignment of females to the home and to the role of homemaker. Assignment of females to this locale, a modal empirical feature of every class and ethnic group within this and almost all societies has had, among other structural implications, the major virtues of eliminating half the competition, of keeping labor supplies shorter and (male) wages higher than they would otherwise be, and of allowing males the full-time opportunity to manage extra-

familial institutional control. The regular and routine assignment of females to prisons, on the other hand, would appear to challenge this advantage severely through the development of increased rates of one or more major disequilibrating outcomes: (1) the breakup of the nuclear family, (2) the maintenance of the nuclear family only with grave financial burdens entailed in hiring a full-time role replacement for the female and/or (3) the partial or full-time withdrawal from the occupational sector of males so affected.¹² It is thus argued that it is not in the interests of male dominance to allow the development of deviant type-scripts for women, both among secondary agents of social control (such as police and courts) and, at a more fundamental level, among primary agents of social control (such as parents and teachers).

As outlined in these terms, the functional regulation of deviant role incumbency through the regulation of type-scripts is seen as providing a basis for distinguishing, in the most ironic sense, deviant behaviors and identities which are expected, functional and socially articulated for certain actor types from behaviors and identities which are anomalous, dysfunctional and socially inarticulate for other actor types. (Thus, where blacks and poor may be said to “become criminal,” types for whom deviant scripts are unwritten may be said to “become anomalous.”)

The theory outlined here extends labeling theory in several major ways. First, it offers a social-structural account of who and what is to be considered deviant and why.¹³ Second, it argues that the back-

¹² The threat to male hegemony posed by reassigning females from the home to prisons is not argued as varying by caste or class. It should be expected, however, that on the basis of caste and class power differentials in resisting detection, incarceration, etc.: (1) black and/or poor females will be assigned to prisons more regularly than white and/or middle-class females and (2) the hegemony of the black and/or poor male will be more threatened by such assignment than that of the white and/or middle-class male.

¹³ It may be objected that the development of type-scripts, though given an explanation here in terms of “why,” is not explained in terms of “how.” Though this apparent gap is often seen as

ground expectancies for deviance contained in type-scripts have social communality extending across the boundary between the dominant *and* the dominated. Third, in assuming this "normative" distribution, the theory maintains an etiological scenario at the individual level. In these terms, scripts for who and what is to be considered deviant become the direct basis for deviant self-attributions. Thus, it is important to realize that the proper major target of a functional theory of deviant type-scripts is *not* secondary deviance as labeling theory would have it but, rather, is *primary deviance per se*. From this viewpoint, deviant self-attributions actualized through the perceived linkage between social "type" and deviant type-scripting become the "motivational" basis for engaging in deviant behavior *independently* of direct contact with official agents of social control.¹⁴ Finally, in hypothesizing the direct impact of deviant self-attributions on (primary) deviant behavior, a functional theory of deviant type-scripts need not forego attention to the role of official social control. Rather, contact with

official agents may be seen as homologous to previous type-scripting and, consequently, as providing: epistemological reinforcement for deviant self-typing at the individual level, stronger attendant deviant self-attributions and further organization of personality through pressure to accept the master trait of "deviance" (secondary deviance).

The theory outlined here would seem capable of starting with the sex variable and accounting for the very high amount of empirical variance it "explains" in official *and* hidden criminality. Since the theory need not be restricted by the specification of concrete socially-dominant groups, it is historically and cross-culturally flexible.¹⁵ In these terms, despite the ubiquity of male dominance historically and cross-culturally, the degree of such dominance may easily be measured independently of sex ratios in criminal deviance and, at a minimum, the two expected to covary. Thus, even though the marked sex ratio in criminality may disappear in time, and more quickly so in post-industrial societies such as ours, the power of a general functional theory of deviant type-scripts would not be contingent upon such an occurrence.

a problem indigenous to functionalist arguments, the reader is cautioned to realize that this is properly a question of how *any* norms consonant with a given social order emerge. This general question is beyond the scope of the present paper.

¹⁴ It is not argued here that all members of a social type assigned deviant type-scripts (e.g., males, blacks, adolescents) engage in primary deviance at the same rate. On the one hand, it is not maintained that a given type is scripted for only one role. On the other hand, the issue is more properly one of explaining deviant *rate* variance across social types. Attempts to explain rate variance within types—analogue to the attempts by Reckless et al. (1956:16) to explain the existence of the nondelinquent slum boy—may, in part, be dealt with in the present context in terms of self-attributions which cross-cut and mitigate deviant type-scripts. The bulk of this mitigating effect is seen as a function of cross-cutting role alignments—and consequent self-attributions—based on such social-structural typologies as caste and class. From this viewpoint, and as opposed to the position of Reckless et al. (1956), the overt message, "you are a good boy, and good boys don't get into trouble with the law," is not nearly so subjectively important and sociologically significant as the covert message, "you are a white boy, and white boys don't get into trouble with the law."

CONCLUSIONS

That the sex variable in some form has not provided the starting point of all theories of criminal deviance has been the major failure of deviance theorizing in this century. In all, it appears to provide the single most powerful predictor of officially and unofficially known criminal deviance in this society and almost certainly in all others. Had the phrenologists realized this, and had they measured waists instead of skulls, they too might have discovered the need to begin their theorizing on the basis of gender.

As a variable, however, sex is categorical and, consequently, primitive. Whatever

¹⁵ Though our critique has been of theories of criminal deviance, the functional theory of deviant type-scripts outlined here need not be restricted to criminal deviance.

the theoretically useful correlates of this term and whatever their etiology, I have argued that understanding the role of gender in producing deviance remains the point toward which all deviance theorizing needs to be directed. This is not simply a theoretical problem. Strategies geared to the reduction of crime which do not face this issue squarely either must assume a fixed genetic basis for the sex difference in criminality (which, among other reasons, makes little sense given the simple observation of sharp drops in sex ratios as societies industrialize) or must continue to face the absurdity of developing, by analogy, a medical art based solely on the study of the "ill," never looking at the "well."

Yet this is not an easy thing to see. Shared representations of social reality which have to do with crime and typifications of deviance generally have at once precluded sociological awareness of the necessity of including sex in deviance theorizing and, at the same time, undoubtedly played a major sex-specific etiological role in the creation of deviance itself. Put simply, dominant typifications about what kinds of actors "do" criminal behavior—typifications which have served dominant male interests and have been held by both sexes—have played a crucial dual role in, on the one hand, keeping sociologists from seeing the sex variable in criminal deviance and, on the other, keeping men in crime and women out of it. The functional theory of deviant typescripts presented here in outline would, in principle, account for these parallel outcomes.

Though I have argued that it is unlikely or unreasonable to expect that the bulk of contemporary criminal deviance paradigms are capable of accommodating the sex variable as the starting point of the analysis, it has not been my intention to claim that this has been demonstrated. Of necessity, the critical argument has been developed with respect to crime generally and not to particular kinds of crime, has carried its own theoretical specification, and has been made speculatively when in the end, such issues are an empirical mat-

ter. All these issues lend themselves to ready debate. Yet, however valuable the critique and theory outlined here may be, given the present state of affairs in deviance theory, it is crucial that attempts be made to address the issues raised. It is my strong conviction that historically consuming questions in deviance theory—especially those involving caste and class—will be best addressed only *after* such attempts are made.

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NORMAL HOMICIDES AND THE LAW *

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This work focuses on the effects of a popular conception of criminality in the adjudication of homicide defendants. Data from a sample of persons arrested for murder suggest that the stereotype of the violent offender, the "normal primitive," constitutes an official imagery within which legal decisions are made. Path analytic techniques indicate that conformity to the criminal conception, along with the social class of the defendant, has significant consequences for the assignment of public counsel, denial of bail and a plea of guilt before the judge. Lack of access to legal resources, in turn, produces the more severe convictions awarded by the court.

The differential legal treatment of criminal offenders constitutes one of the most controversial areas of study in contemporary criminology. Allegations of racial discrimination or class bias in the judicial system have prompted continued debate and investigation of the issue. Yet the search for patterns of discretionary treatment has generated contradictory findings. Studies of police administration, bail procedures, prosecution and defense, jury selection, and final disposition, while prolific, have failed to surface a definitive statement concerning equality of justice in the United States.¹

The use of class and race as the primary criteria against which legal treatment has been evaluated may be responsible for the disparity of findings evident in the litera-

ture. The criminal justice system is the institutional representation of the "American Creed" (Myrdal, 1944) of equality for all. With many stages of the judicial process open to public inspection and legal sanction, violation of this principle through manifest class or racial discrimination would seriously undermine the collective myth.

Given a society characterized by an official commitment to equality before the law, the selective use of discretion in the legal system might better be examined along alternative lines. In this regard, it may be noted that law enforcement and adjudication are interpretive processes. At each stage, legal authorities must assess the offender and the offense for evidence that official sanction is warranted. Such evaluation and interpretation may be guided, in part, by popular stereotypes of criminality.

Evidence that stereotypes exist in the general population is abundant. People do, in fact, tend to utilize highly stereotypical characterizations in describing deviants.

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¹ Discussions of the discrepant findings evident in the literature may be found in Hindelang (1969) and Swigert and Farrell (1976: ch. 2).

Furthermore, individual descriptions of specific deviations are remarkably similar in content (Simmons, 1965:226). Thus, "the marijuana smoker stereotype emerges as an insecure escapist, lacking self-control and looking for kicks; the adulterer is immoral; promiscuous, and insecure; the homosexual is perverted and mentally ill; the political radical is ambitious, aggressive, stubborn, and dangerous" (Simmons, 1965:228). Such conceptions are perpetuated in everyday interaction and in the mass media characterizations of comic strips, television, newspapers, books, songs and advertising. In this manner, even the very young become familiar with stereotypes, images that continue to be reaffirmed throughout adulthood (Cain, 1964:278-89; Scheff, 1966:64).

Stereotypes not only shape public attitudes and behavior toward deviants, but guide the very choice of individuals who are to be so defined and processed. Persons possessing characteristics associated with the stereotype of a particular deviation are more likely to be identified and reacted to as such (Simmons, 1965:226). Furthermore, since minority groups, lower class persons, and males more closely approximate stereotypic images, these groups are especially susceptible to their application (Simmons, 1965:226; Hess and Mariner, 1975:255).

Such a pattern also may be observed at the organizational level. Thus, popular beliefs about deviants within social control agencies have been found to influence the application of formal labels regarding criminality. It has been noted, for example, that among legal authorities there is evident a belief that certain groups in the population are inherently criminal. This anticipation of law-violating behavior underlies much of the discretionary treatment accorded the black and the poor. The use of violence against blacks and slum dwellers (Westley, 1953), the cursory disposition of urban vagrants (Foote, 1956) and the differential selection of juveniles for treatment (Goldman, 1963; Piliavin and Briar, 1964) illustrate not so much racism or class bias in the law, but a response to a belief that blacks and the poor require rough treatment (Westley, 1953:163), that

obvious destitution implies future criminality (Foote, 1956:310) and that black children are more likely to engage in crime than white children (Goldman, 1963:288).

Given the suspicion concerning the behavior of these groups, encounters of legal authorities with black and lower class individuals are potentially problematic. In such a situation, the decision to invoke legal sanction is a process of determining the extent to which the individuals involved conform to the criminal stereotype. Thus, the arrests and prosecution of juvenile delinquents (Goldman, 1963; Piliavin and Briar, 1964), delinquent bail cases (Skolnik, 1966), shoplifters (Cameron, 1964; Steffensmeier and Terry, 1973) and homosexuals (Farrell and Hardin, 1974) have been shown to be motivated more by the stereotypic appearance of the offender than by evidence of actual criminal behavior; while facial stereotypes of the murderer, robber and traitor have been found to influence jury evaluations of guilt for each of these criminal categories (Shoemaker, 1973).

Evidence does exist, then, to suggest that stereotypes operate as guiding imageries for action in the treatment of deviance. Applying Goffman's theoretical discussion of the stigma to the adjudication process, it may be observed that the stereotype becomes the "means for categorizing persons and the complement of attributes felt to be ordinary and natural for members of each of these categories," the product of which is to "allow us to deal with anticipated others without special attention or thought" (Goffman, 1963:2). A *legally* problematic situation would seem to be particularly prone to the use of such categorizations. Where constraints of time, personnel, and the sheer number of individuals who must be processed preclude full enforcement or extensive investigation, officials are likely to depend on shorthand methods in administrative decisions. Such methods include inspection of the offender and the offense for conformity to the popular conception of criminality.²

² Quinney (1970) is to be credited with the development of the notion of criminal stereotypes. In his own work, the stereotype is the medium through which powerful segments of

That many of the cues perceived by legal authorities are manifestations of class and race cultures has led a number of researchers to conclude that the legal system discriminates against these groups. It is possible to speculate, however, that more than class and race, cultural stereotypes of criminality determine the decisions of legal authorities. While these stereotypes include class- and race-related characteristics, they are not restricted to, nor do they include all members of these groups.

THE DATA

In order to examine this issue, a study of the legal treatment of individuals arrested on general charges of murder was conducted. The records maintained by a diagnostic and evaluation clinic attached to the court in a large urban jurisdiction in the northeastern United States and the indictment files secured from the Office of the Clerk of Courts provided the data used in the research.

Established in the mid-1930s, the court clinic has been charged with the diagnosis and evaluation of selected offenders as part of pre-sentence investigation. While most of its long-term efforts have involved the evaluation of sex offenders, in 1955 the clinic established a routine policy of examining cases of criminal homicide. Consequently, all persons arrested for murder in this jurisdiction are seen at the clinic, where an exhaustive study is done of their psychological condition and social background. If the findings indicate that the defendant is insane, the clinic notifies the court and proceedings are initiated for commitment to a mental hospital. Otherwise, the information is summarized and forwarded to the presiding judge for use in sentencing, in the event that the defendant is found or pleads guilty. If the defendant is found not guilty or if the case is dismissed, the sealed envelope is returned to the clinic unopened. In either event, upon its completion, the court notifies the clinic

of the specifics regarding the final disposition of the case.

In selecting the cases to be analyzed, an exhaustive list of all persons arrested on general charges of murder in the jurisdiction was compiled from the clinic files. Four hundred and forty-four cases, a 50 percent random sample, covering the period from January 1, 1955, through December 31, 1973, were selected for analysis.

The clinic records provided thorough and systematic data on the social characteristics of the defendant, any prior offense record, the circumstances of the offense and the results of the psychiatric evaluation. As regards the legal data, however, it was found that the information was not always contained in the clinic reports or that there were certain contradictions in the records concerning award of bail, the identity of the attorney or the results of post-sentence appeals. Because of these lacks, it became necessary to secure the latter data from the indictment records maintained by the Office of the Clerk of Courts. These records supplied information regarding the type of counsel (private, private appointed or public), bail decisions, the type of trial (jury or non-jury) and the final conviction awarded.³

While it is acknowledged that the records maintained by the clinic and court are for other than scientific purposes, it is precisely this information that is used by legal authorities in the determination of the career outcome of the defendant. The data collected for this research, therefore, comprise much of the same information as evaluated by legal representatives in their decision-making processes. The comprehensiveness of the records, the opportunity they present for verification of recorded fact, and their similarity to the kinds of in-

society insure a consistency of definition of criminality. Through the diffusion of the popular conception, individuals become aware of the social characteristics of offenders, the appropriate reaction to crime and the relevance of crime to the social system.

³ Since both clinic and indictment records contained a variety of sources of the same information concerning the defendant, victim and the circumstances of the offense, a multiple check of recorded facts was made possible. Where discrepancies occurred, those sources closest to the legal stage in question were utilized for documentation. Thus, for example, discrepant recordings of final disposition by the clinic and court would result in adoption of the court documents for verification of this fact.

formation utilized throughout the legal system constitute a data source that may provide a most acceptable basis for the study of adjudication patterns.

THE "NORMAL PRIMITIVE": A CRIMINAL CONCEPTION

Evidence of the role of the criminal stereotype in the legal processing of defendants emerged somewhat serendipitously in the early stages of data collection. In a review of the psychiatric portions of the clinic reports, a diagnostic category surfaced which seemed to reflect an official usage of such a stereotype—the "normal primitive." This category has become part of the standard diagnostic terminology of the staff of the clinic and constitutes a meaningful classification for the court. The characteristics of the "normal primitive" were provided by the clinic and the diagnosis warrants a detailed description here.

While treated as a diagnostic category, the designation "normal primitive" constitutes a *social* description of a group of people whose behavior, *within their own social setting*, is best described as normal. The "normal primitive" comes largely from the foreign-born and black populations. Their lives are characterized by impoverished economic conditions which, as with their behavior, may be described as "primitive." Occupational achievements center around unskilled, menial labor, and these careers are often sporadic. Educational levels are minimal and testing indicates borderline to low-average intelligence. While the children of the foreign-born do acclimate to a less "primitive" existence, the offspring of the black population seem unaffected by improved educational and social opportunities.

The personality characteristics of the "normal primitive" are childlike or juvenile, the behavior and attitude being similar to that of an eight- to twelve-year-old boy. At the same time, acceptance as a *man* by his group is very important. In this regard, the "normal primitive" is sensitive and takes offense to any question of his masculinity.

Interaction among such individuals often occurs in bars where arguments readily result in aggressive encounters. Compelled to fight any challenger of his masculinity or courage, the "normal primitive" protects himself by carrying a lethal weapon.

While sexual patterns among the foreign-born are relatively stable, promiscuity among the blacks provides additional

grounds for aggression. Sexual prowess is a reflection of the masculinity of males, but is denied to females. Thus, when infidelity occurs, "as it inevitably does," the humiliation perceived by the male will result in threats and physical abuse that may produce the death of any one, or all members, of the sex triangle.

In summary:

the primitive man is comfortable and without mental illness. He has little, if any, education and is of dull intelligence. His goals are sensual and immediate—satisfying his physical and sexual needs without inhibition, postponement or planning. There is little regard for the future—extending hardly beyond the filling of his stomach and the next payday or relief check. His loyalties and identifications are with a group that has little purpose in life, except surviving with a minimum of sweat and a maximum of pleasure. He has the ten-year-old boy's preoccupation with muscular prowess and "being a man." Unfortunately, he lacks the boy's external restraint and supervision so that he is more or less an intermittent community problem and responsibility. (clinic description)

The "normal primitive" classification represents a conception of criminality that combines both class and race characteristics. The imagery suggests a group of people whose style of life and innate attributes predispose them to violence. The tendency toward physical aggression over "trivial" issues, histories of family disorganization, and tenuous marital ties, combined with a perception by the offender that the violent response is appropriate and necessary, are manifestations of the volatile life style in which the "normal primitive" is said to exist.⁴

The importance of this diagnostic category lies in its expression of a more general conception of criminality as it operates in the judicial system. In that the clinic diagnosis is part of the *sealed* information forwarded to the presiding judge, it is only *following* conviction that it becomes rele-

⁴ The "normal primitive" criminal conception is remarkably similar to social scientific depictions of members of the subculture of violence (see, e.g., Pittman and Handy, 1944; Benay, 1966; Wolfgang and Ferracuti 1967). Elsewhere we have argued that the social scientific "discovery" of such a subculture may be testimony to the effectiveness with which the stereotype of violent criminality has been applied (see Swigert and Farrell, 1976).

vant information in the adjudication process. Evidence that individuals diagnosed as "normal primitive" receive differential treatment prior to sentencing would suggest, therefore, a more diffuse operation of the conception. Thus, an exploration of the effects of such a stereotype on the processing of defendants in the case of homicide becomes the empirical focus of this work.

DESIGN FOR ANALYSIS

Research on the question of differential legal treatment is characterized by a tradition of limited empirical focus. Concerned almost exclusively with an examination of specific stages in the legal system, studies have concentrated on *either* access to bail (Foote, 1959; Ares et al., 1963), the nature of counsel (Ehrmann, 1962; Sudnow, 1965; Blumberg, 1967; Siegel, 1968), jury selection (Robinson, 1950; Strodbeck et al., 1957; Erlanger, 1970) *or* final disposition (Johnson, 1941; Garfinkel, 1949; Bensing and Schroeder, 1960; Bullock, 1961; Vines and Jacob, 1963; Wolf, 1964; Forslund, 1969; Farrell, 1971; Chiricos et al., 1972; Burke and Turk, 1975). While intervening relationships involving the status characteristics of the victim (Johnson, 1941; Garfinkel, 1949), the severity of the offense (Bensing and Schroeder, 1960; Wolfgang, 1961; Farrell, 1971; Burke and Turk, 1975) and the extent of prior record (Green, 1961; Farrell, 1971) have been examined, still the empirical treatment of adjudication has been confined to the effects of a limited number of variables on single stages of the legal process.

Such an approach fails to acknowledge the processual nature of adjudication. Differential treatment occurring early in the judicial process may have consequences for final disposition. Thus, the relationships between social characteristics of defendants and disposition may not be direct but, rather, may operate through a number of other variables relevant to the adjudication process. Therefore, it is imperative that the sequence of events leading to the determination of guilt or innocence be incorporated into the study of treatment patterns.

The nature of the data utilized in criminological research has traditionally limited the kinds of investigations conducted. Many of the variables relevant to such studies involve nominal data. Statistical analyses, therefore, have relied upon various cross-tabular schemes. While developments in log-linear techniques (Goodman, 1972; 1973a; 1973b) have introduced the possibility of imposing causal order on nominal data, the usefulness of this method is somewhat restricted. With the addition of each new variable, the possibility of alternative causal interpretations increases exponentially.

It has recently been demonstrated (Knoke, 1975; Goodman, 1976), however, that regression analysis of dichotomous variables produces results comparable to those obtained through log-linear techniques. The argument is that multiple regression may be robust enough to overcome the violation of assumptions produced when a dependent variable is dichotomized. Thus, with the restriction that some degree of variance exists in the dependent variable, the use of regression analysis may be approached with confidence.

The highly structured nature of the judicial system lends itself to a systematic analysis of legal processing. The discrete ordering of events from assignments of counsel, bail decisions and selection of trial format to final adjudication constitutes a series of stages that allows the researcher to assert the causal sequence of events.

Capitalizing on the nature of the system, then, an effort is made to trace the paths of influence among certain social and legal variables and the final disposition of cases by the court. The path analytic technique is particularly suited for such an exploration. For, having established causal priority among a system of variables, paths of direct as well as indirect influence may be assessed.

The construction of complete recursive path diagrams was executed. Using multiple regression techniques, each stage of the model was fitted to all preceding variables. Paths failing to produce standardized coefficients of at least .100 were de-

leted, and the remaining coefficients were recalculated.⁵ The path and correlation coefficients generated by this analysis are presented in Table 1.

HOMICIDE AND THE CRIMINAL CONCEPTION:
AN EMPIRICAL MODEL OF LEGAL
PROCESSING

Prior Conviction Severity

The first stage of analysis involves the specification of the effects of the social characteristics of homicide defendants on the development of prior conviction records. The importance of a criminal record in the adjudication process has been noted by a number of researchers. The use of this concept, however, has been restricted to an observation of the presence or absence of prior offenses (Farrell, 1971; Burke and Turk, 1975) or, if severity of criminal record is considered, to a simple summation of the number of offenses, felonies or misdemeanors for which each defendant was found guilty (Bullock, 1961; Green, 1964; Chiricos et al., 1972).

Given the reported significance of the variable, a more sensitive indicant of the seriousness of prior convictions seems required. Toward that end, it was noted that state law establishes the maximum penalty allowable for each degree of felony and misdemeanor. Such penalties would appear to provide an adequate estimate of the seriousness of offenses as officially recognized in a particular jurisdiction. In the present research, therefore, a summation of the prescribed maximum penalties for offenses for which the defendant was found

guilty constituted a ratio measure of prior conviction severity.⁶

The coefficients calculated for the exploration of this variable are shown in column D of Table 1. The sex and occupational prestige⁷ of the defendant do contribute to the development of a prior record. Males and individuals of lower occupational status have more extensive criminal histories, blacks do not.

This latter finding has important implications for earlier explanations concerning the role of race in legal treatment. For, it has been argued that the more severe sentences accorded blacks may be accounted for by the greater extent of prior criminal involvement among black defendants (Green, 1961). Since prior offenses are considered in pre-sentence investigations, the severity of the sentence awarded offenders with extensive records is said to be a reflection of a judicial decision to penalize more severely those whose legal backgrounds indicate a history of recidivism. Thus, if blacks receive longer sentences for their offenses than do whites, it is because of their greater involvement in prior criminal behavior.

The present analysis, however, indicates that race is not directly associated with prior offense record, but operates only through its relationship with occupational prestige ($r = .352$). The usefulness of previous convictions in explanations of racial differences in the adjudication process is therefore not apparent.

The contribution of sex to prior offense severity is presumably in terms of the provision of access to the means for enacting criminal behavior. Social roles and expectations afford males greater exposure to illegal opportunities. The association of this variable with prior offense severity,

⁵ Coefficients for the abbreviated model were computed using a least-squares estimate (Boudon, 1968:199-235). This procedure utilizes the entire set of antecedent variables in the estimation of path coefficients, rather than deleting the nonsignificant variables from calculations. The adequacy of the abbreviated model was assessed in terms of its ability to reproduce the original correlation matrices. A one-tailed test of the null hypothesis that the original and reconstructed correlations did not differ significantly from zero was utilized. Where these correlations did differ significantly and where the reinstated path produced a coefficient of at least .100, path coefficients were recalculated. This procedure was iterated until all coefficients were adequately reproduced.

⁶ Thus, a defendant with a conviction for robbery (maximum penalty, 20 years) and one for bookmaking (maximum penalty, five years) would receive a prior offense severity score of 25. Penalties ranged from 0 to 20 years, with no prior convictions for first degree murder. Sentences of less than a year were scored in terms of the percentage of the year involved. Persons awarded a 90-day sentence, for example, would receive a severity score of .25.

⁷ Occupational prestige was measured in terms of Treiman's (forthcoming) classification system.

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Table 1. Standardized Path and Correlation Coefficients for Each Stage of the Analysis

Correlation Coefficients	Path Coefficients								
	A Sex	B Race	C Occupational Prestige	D Prior Convic- tion Severity	E Normal Primitive	F Private Attorney	G Bail	H Jury Trial	I Final Conviction
Sex (F-M) *		-	-	.157 (-.051) ^b	(-.019)	(.038)	(-.061)	(.095)	.159 (.048)
Race (B-W) *	.080		-		-.260	(-.097)	(-.032)	(-.046)	
Occupational Prestige (L-H) *	.057	.352		-.154	-.154	.303	(.072)	(.086)	-.148
Prior Conviction Severity (L-H) *	.151	-.081	-.131		(.055)	-.120	-.130	(.001)	.130
Normal Primitive (No-Yes)	-.040	-.311	-.239	.092		(-.096)	-.112	-.171	(.002)
Private Attorney (No-Yes)	.034	.057	.316	.162	-.173		.199	(-.057)	(-.043)
Bail (No-Yes)	-.065	.042	.153	-.161	-.157	.230		(.096)	-.215
Jury Trial (No-Yes)	.093	.034	.104	-.010	-.148	.016	.108		-.123
Final Conviction (L-H) *	.167	-.032	-.237	.215	.098	-.165	-.293	-.171	

* (F-M) = Female-Male; (B-W) = Black-White; (L-H) = Low to High.

^b Coefficients before the variable was dropped from the equation are presented in parentheses throughout the table.

then, has little theoretical relevance to the exploration of legal treatment. Thus, only the path from occupational prestige to prior offense record is of major concern to this work. In this regard, it may be observed that individuals of lower occupational status accumulate more extensive criminal records.

The "normal primitive" classification, as used by the clinic, designates persons whose violent behavior is said to be normal within their own social setting. Applied largely to minority populations, the label is reflective of a cultural stereotype of criminality. Thus, serving as an empirical indicator of a more popular image of individuals who are thought to be prone to the spontaneous expression of violence, the "normal primitive" classification provides the foundations for an analysis of the effects of criminal conceptions on the legal process.

To assess the impact of this conception on legal treatment in the case of murder, defendants were classified according to the absence or presence of the "normal primitive" diagnosis in their clinical evaluations. All prior variables were then examined for their influence in the determination of the diagnosis. Column E of Table 1 shows that the race and occupational prestige of defendants do, in fact, contribute substantially to the designation "normal primitive." Here we note that blacks and individuals of lower occupational prestige are more likely to be designated "primitive" than are whites or higher status persons.

Such a pattern is not surprising, given the meaning of the concept as formulated by the clinic. For, as noted previously, this conception of crime is based on a number of class- and race-related characteristics. More specifically, the label connotes a predisposition to violence among the black and the poor. An imagery of personal and social disorganization within which these groups are evaluated is said to underlie their tendency toward greater levels of interpersonal aggression.

Access to Legal Resources

The social characteristics of defendants, the severity of their prior conviction re-

cords, and the designation "normal primitive" must now be assessed in terms of influence on the availability of private counsel, award of bail and trial by jury. Differential access to such resources may constitute a crucial link in the determination of adjudication patterns. The nature of the interrelationships at this stage of the analysis, therefore, provides important information for an understanding of legal treatment.

The right to counsel. The system of public defense was developed in order to guarantee the indigent defendant the right to qualified legal representation. Yet the differentials in resources available to private and public counsel (Ehrmann, 1962; Siegel, 1968; Deatherage, 1972), as well as the political milieu within which the latter must operate (Siegel, 1968), have brought into question the quality of defense provided by the public attorney. Of particular concern is the fact that the limitations inherent in appointed counsel seem to affect final disposition patterns in criminal cases (Ehrmann, 1962:21; Chiricos et al., 1972: 562-4). Given the apparent centrality of counsel in determining legal outcome, then, it is important to specify the factors that facilitate access to the privately retained defender.

Column F of Table 1 depicts the patterns associated with retention of a private attorney. The best predictor of such retention is the higher occupational prestige of the defendant. Since indigence is the basis for assignment of public defense, this relationship is expected.

In addition to financial ability, the severity of a defendant's prior conviction record has a small but independent effect. That is, individuals with extensive criminal records are more likely to be assigned public counsel. A suggested explanation of this phenomenon involves the decision of private counsel to refuse those cases where successful defense may be problematic. The presence of a criminal history resists the application of an imagery of innocence. When such a presumption cannot be sustained, a defender may be unwilling to risk the courtroom consequences. Protective of his reputation as a successful criminal lawyer and sensitive to his loss of bargain-

ing power with the prosecutor, the private attorney may withhold legal services from those defendants whose prior experiences with the law are seen as predictive of career criminality.

Of special interest is the association between the criminal conception of homicide and retention of a private attorney; the coefficient evident here, although in the right direction, approaches insignificance ($P=.096$). An explanation of this lack of relationship concerns the number of "normal primitives" who also come from lower occupational positions. Table 2 depicts a cross-tabulation of retention of private counsel and the "normal primitive" designation while controlling for occupational prestige (occupation has been dichotomized according to the mean). The findings indicate that the influence of the popular conception on type of attorney occurs through the financial inability of such persons to retain private counsel, rather than through any direct effect of conformity to the stereotype.

Award of bail. The institution of bail was developed in response to the problem of deterring the accused from fleeing adjudication, while assuring him the right to the presumption of innocence through release from incarceration before trial (Foote, 1959:43). Guided by this principle, bail is to be set in graduated amounts that motivate persons of all income levels to appear in court at the appointed time. Rather than using the financial ability of defendants, however, it has been argued that judges often rely upon the severity of the present offense, as well as the prior conviction record, to determine bail amounts (Foote, 1959). Since these factors are presumed to be associated more

often with lower income defendants, bail procedures may be said to have a strong class bias.

Evidence also suggests that the inability to obtain bail has significant effects on final disposition. Studies have shown that defendants jailed before trial were more often convicted than those charged with similar offenses who were able to finance their release (Foote, 1959:47; Los Angeles Law Review, 1961:627; Ares et al., 1963:83). Social class, therefore, through the operation of the institution of bail, may have important consequences for adjudication in criminal cases.

An additional word must be said about the nature of the present offense and the decision to set bail. As noted, offense severity is an important consideration in determining the eligibility of a defendant for pretrial release. Indictments often involve an implied statement concerning level of intent and premeditation. Thus, a charge of first degree murder is a more serious offense than a charge of involuntary manslaughter. In the present jurisdiction, however, individuals are arrested and indicted on a general charge of murder. The degree of the offense, along with the guilt or innocence of the defendant, is determined only at the time of the trial. Therefore, offense severity as it is specified in the formal charge, is automatically controlled in this study.⁸

⁸ Official criteria for determining bail eligibility include an assessment of the "nature of the offense and any mitigating or aggravating factors that may bear upon the likelihood of conviction and possible penalty" (Criminal Code Manual issued by the state in which this study was conducted). It is the determination of the effects of the criminal stereotype on evaluations of mitigating and aggravating factors, when the nature of

Table 2. Retention of Private Attorney by Normal Primitiveness Controlling for Occupational Prestige

Access to Private Attorney	Occupational Prestige							
	Low				High			
	Normal Primitive				Normal Primitive			
	No		Yes		No		Yes	
No	n	%	n	%	n	%	n	%
Yes	(80)	57.1	(53)	75.7	(51)	35.7	(5)	29.4
Total	(60)	42.9	(17)	25.2	(92)	64.3	(12)	70.6
	210				160			

$$\chi^2 = 6.203, df = 3, P < .20.$$

The results of an examination of the causal antecedents to award of bail in the case of homicide are shown in column G of Table 1. Occupational prestige was found to have little direct influence. Rather, this relationship occurs through the effects of prior offense record, the nature of counsel and application of the label "normal primitive."

That a defendant with an extensive prior conviction history is more likely to be detained before trial seems to be a consequence of two related factors. On the one hand, the withholding of bail or establishing its amount at excessive levels represents a court decision to detain the repeat offender. On the other hand, there may be operative the belief that individuals who have already been convicted of violating the law are more likely to be guilty of the present offense as well. The question of pretrial release in this case becomes inconsequential.

Another influence in the award of bail is the type of counsel. Privately retained attorneys are more successful in securing bail for their clients than are publically appointed attorneys. This relationship operates independently from the financial ability of the defendants involved.⁹ Thus, access to resources early in the legal process has consequences for the acquisition of additional resources at later stages.

Finally, individuals designated as "normal primitive" are also denied bail. This not only occurs through the nature of the legal services provided them as indigent defendants, but is also a direct effect of their status as "normal primitives." The denial of bail to those whose characteristics conform to a popular conception of the homicidal type suggests that a presumption of guilt is made by the court before official adjudication occurs.

Trial format. The final legal resource variable to be considered is that of trial format. The right to trial by jury is guaranteed all defendants who plead not guilty to the charges against them. Only those

who admit their guilt or who waive their right to trial are disposed of by a judge.

An empirical examination of the effects of the several variables discussed thus far on selection of trial format reveals that the only significant path of influence was that generated by the "normal primitive" designation (Table 1, column H).

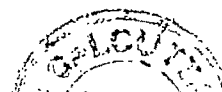
It is interesting to note that there is no direct path of influence from attorney to trial format. It has been argued that negotiations between prosecutor and attorney will often result in a bargained settlement of the case (Newman, 1956; Alschuler, 1968; Cole, 1970). While all attorneys engage in plea negotiations, such negotiations are most often utilized by appointed counsel (Blumberg, 1967). The absence of a direct relationship between attorney and trial by jury, however, suggests that the origins of the negotiated plea lie outside the initiative of the participating attorneys. Rather, we note that the single most important determinant of type of trial is the "normal primitive" criminal conception.

Once again, we interpret this finding in terms of the impact of the stereotype of criminality on legal outcome. Recalling Sudnow's (1965) discussion of normal crimes, it may be suggested that the characteristics typically associated with homicide have a direct effect on the quality of treatment afforded individuals whose offenses are seen as normal. Thus, the appearance of a normal homicide, as one committed by an individual whose culture and characteristics are said to be associated with violent behavior, exerts an influence on trial format which is independent of the nature of counsel.

As with the effects of "normal primitiveness" on the award of bail, the implications of this relationship may be viewed within the context of a presumption of guilt. The denial of bail and the entry of a guilty plea before the court represent a definition of culpability, the criterion for which is conformity to the criminal stereotype. Once that evaluation is made, the legal resources essential for the successful defense of a criminal defendant are, in the eyes of legal representatives, superfluous. Without such advantages, equalitarian legal treatment itself becomes problematic. Public

the charge is held constant, that is the focus of concern in this stage of the analysis.

⁹ The validity of such a statement, of course, depends upon the adequacy of using occupational prestige as an indicator of financial resources.



defenders are not able to conduct a pre-trial investigation comparable to that initiated by private attorneys; denial of bail not only creates a definition of dangerousness and guilt, but often results in job termination—a factor weighed heavily by the court should probation become a sentencing alternative; and a plea of guilt removes the last vestige of the combative imagery upon which the legal system is founded.

Final Disposition

The last stage in the analysis involves an assessment of defendant characteristics, prior offense record, the "normal primitive" designation and access to legal resources as they affect final adjudication. The disposition of a case was measured in terms of the charge for which the defendant was found guilty.¹⁰ Since defendants originally charged with criminal homicide were sometimes convicted of crimes other than the two degrees of murder and two degrees of manslaughter, final conviction was scaled according to felony and misdemeanor levels. Thus, charges included first degree murder, first degree felony (second degree murder), second degree felony (voluntary manslaughter), first degree misdemeanor (involuntary manslaughter), acquittal, and dismissal (including *nolle prosequi* and demurer sustained). Column I of Table 1 displays the coefficients generated in the analysis.

The sex and occupational prestige of defendants are directly related to severity of final disposition. Males and persons of lower occupational status are convicted of the most serious charges.

¹⁰ Sentencing decisions are dependent upon the conviction awarded defendants. Within each offense category, however, the range of possible sentences is both limited and typically established by formula. Thus, cross-tabular analyses of sentences imposed within each degree of homicide reveal no significant differences. It appears, therefore, that discretionary treatment occurs earlier in the legal processing of criminal defendants. In addition, it is following conviction that the judge has access to the pre-sentence recommendations and diagnoses of the clinic. Since the focus of this research is to ascertain the effects of a shared conceptualization of the violent offender on treatment severity, it is necessary to examine those stages of adjudication that precede official access to diagnostic information.

That women are convicted of lesser offenses than their male counterparts is not surprising. Being female seems to be a mitigating factor in the assessment of criminality in general. It is only in regard to certain moral offenses, especially the sex codes, that women are accorded official recognition as law violators (see, for example, Sutherland and Cressey, 1974:126-9). A combination of a social "pedestal effect" as well as the definition that violent behavior is a male phenomenon contribute, we might speculate, to the less severe treatment accorded women accused of homicide.

Of particular theoretical interest is the direct effect of the defendant's occupational prestige on final adjudication. Individuals of lower status are found guilty of serious crimes more often than those of higher status. Thus, in addition to the advantages that it effects at earlier stages of the legal process, higher occupational prestige also produces a more favorable adjudication before the court.

A defendant's prior conviction record also appears to influence final disposition. Since a decision involving guilt or innocence officially is to be without reference to prior criminal involvement, this relationship is somewhat surprising. It appears, however, that such information enters into the adjudication process through more informal channels. Whether it be the unwillingness of prosecution and defense to consider seriously a reduction of charge for the repeat offender, the judge's awareness of the criminal history of a defendant as he weighs the evidence, or the use of prior record in the attempt to impeach the credibility of one who testifies in his own behalf, the defendant's criminal history becomes an influential factor in the severity of final conviction.

The award of bail and trial by jury are the legal resources which have the most direct effects on the severity of final disposition, while the nature of counsel acts through its influence on bail. The findings, in general, indicate that access to these resources produces the less severe final convictions awarded by the court. Given such a pattern, the antecedents to legal resource acquisition become most relevant. In addi-

tion to the operation of social class in the ability to retain private counsel, the two most active determinants of adjudication in the case of homicide are the prior offense record of the defendant and his conformity to an imagery of violent criminality. The completed path diagram generated by this analysis is found in Figure 1.

DISCUSSION

The racial background and social class of criminal defendants traditionally have been the most important factors considered in the study of adjudication. Such attributes, however, have failed to produce conclusive evidence regarding the quality of justice available in the United States. Rather, there appears to be a much more subtle motivation for the discretionary dispensation of justice—the criminal stereotype.

Occupational prestige did emerge as an important factor in the present empirical analysis. It must be noted, however, that the variable most affected by occupational status (that is, attorney) is, in fact, for sale. Little effort is made to disguise the importance of financial status in the ability to retain private counsel. Rather, judicial practices based on economic advantage have become institutionalized to the extent that the inherent inequities are not part of public consciousness. Thus, while the differential ability to purchase defense may elicit concern from those who seek full equality in the legal system, it certainly does not constitute the kind of invidious discrimination charged by the more vocal critics of criminal justice.

As for the relationship between social class and final disposition, we begin to approach the heart of the issue of differential legal treatment. Independent of the financial ability to retain a private attorney, higher social status merits a defendant a more lenient conviction. For an explanation of this pattern, we must look not to an overt attempt by the court to reward the wealthy and penalize the poor, but to the imagery that surrounds the adjudication of defendants. Thus, it may be suggested that the leniency of legal treatment of middle and upper class persons is inspired, in part, by the dominant motivations of punish-

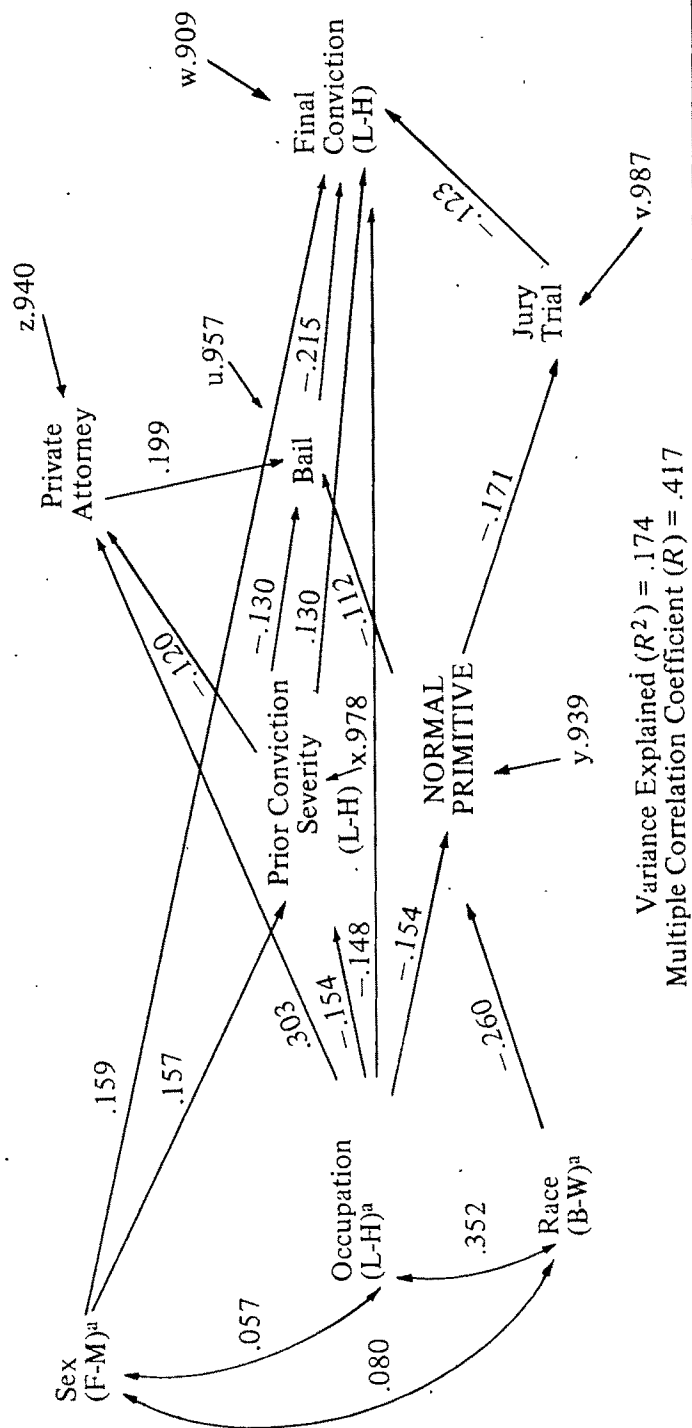
ment and deterrence, (Farrell, 1971). The offender is to suffer not only in proportion to the severity of his crime, but so that he will weigh the advantages and disadvantages of a repeated offense.

For rich and poor alike, the decision to punish, therefore becomes a calculus of loss and pain. The lower class defendant, with minimal status in his community, little occupational prestige and a personal life most frequently described as disorganized, comes to the court with little to lose except freedom from incarceration. The higher status person, on the other hand, simply as a result of arrest is said to have suffered greatly. Suspended professional license, loss of status in the community, personal and familial trauma are seen as being sufficiently punitive and deterrent. Incarceration becomes superfluous when so much is said to have already been lost.

The applicability of this argument is reinforced by observations made within the context of occupational and corporate criminality. For example, while the Incredible Electrical Conspiracy involved losses far exceeding those accrued at the hands of the more conventional criminal, the highest penalty awarded a co-conspirator was 30 days (Smith, 1961a; 1961b). Convictions and sentences of the more recently adjudicated Watergate defendants were similarly proportioned. Popular sympathy generated over the loss of job and prestige incurred by those involved in these ventures testifies to the diffusiveness of the ideology of punishment, an ideology shared by laymen and professionals alike.

The distinction between overt discrimination and compliance with an institutionalized imagery is an important one. The credibility of the legal system, as well as that of judicial administrators, would be seriously threatened should evidence of the penalization of persons for reasons of class be substantiated. Legal safeguards of the rights of lower status persons would make accountable the authorities who flagrantly violate those rights. If, however, the decision to invoke negative sanction occurs within the rationale of a popular ideology of crime and punishment, the legal authority is free to act.

It is already a common practice to take



^aL-H is low to high,
F-M is female - male, and
B-W is black - white.

Figure 1. Final Charge by Legal Resources and Defendant Characteristics

into consideration the presence of a steady job and stable marital ties in the decision to probate or parole the conventional offender. The application of this same notion of "fair play" in the administration of justice to higher status persons is also an acceptable practice and does not generally invoke outrage or allegations of preferential treatment. Rather, in terms of the original logic, justice indeed has been done. The higher status offender, given a middle class measuring rod, has the most stable ties in the community, is a most productive person socially and, as a result of apprehension, has suffered a threat to that status. "Fair play" mandates that such a person be excused from further degradation.

This approach to an understanding of legal treatment is further substantiated by the effects of race on adjudication. In the present analysis, race did not exercise immediate influence on legal processing. The only theoretically significant effects of race concern its strong associations with social class and the criminal conception, "normal primitiveness."

Here again it may be argued that the exercise of overt discrimination against minority group members is effectively prevented by statutory proscription. When class and race alone are used to determine patterns of legal treatment, therefore, race only appears to operate through its relationship with social class. For reasons cited above, the black defendant is not adjudicated on the basis of skin color; rather, decisions are made in light of his membership in the lower class—a position that earns him the perceived need for a more severe penalty.

Such an explanation of the effects of social status point to the inadequacy of the use of the objective criteria of class and race in the assessment of treatment patterns. The decision to penalize is not likely to be made simply on the basis of skin pigmentation and annual income. The search for such relationships, consequently, may account for the contradictory findings reported in the literature. A more promising effort in this area might lie in an evaluation of the effects of criminal conceptions on legal treatment. It is these conceptions that may be the media through which so-

cial and demographic attributes are filtered in judicial decision-making processes.

The evidence generated in the present research would seem to substantiate this assumption. The "normal primitive," as a conceptualization of the typical homicide offender, has emerged as a most important factor influencing final outcome in the court. That certain groups in society are differentially penalized because of the mandates of this conception of criminality becomes valuable information in an analysis of treatment patterns.

Concerning the designation "normal primitive," both race and occupation explained a significant portion of defendants who were found within this category. Blacks and individuals of lower occupational status were more often designated "primitive" than were whites or higher status persons.

The primary effects of such a label appeared in the lack of availability of legal resources. Individuals who were said to conform to this imagery both were denied bail and did not have access to trial by jury. Thus, the presumption of guilt implied by the application of the label became amplified when the court decided that bail should be withheld or when a plea of guilt before the bench was encouraged. In addition, because of the strength of the relationship between occupational prestige and "normal primitiveness," such individuals also lacked the financial ability to retain a private attorney. These legal resources, in their turn, interacted in such a way as to produce the more severe convictions accorded those with public attorneys, no access to bail, and a non-jury trial format.¹¹

¹¹ Such an interpretation involves a departure from the simple evaluation of direct and indirect paths in the determination of causal influence, for both are a function of the original association between independent and dependent variables. The use of this approach, for example, would result in the conclusion that the relationship between race and final charge is a non-significant one. Direct and indirect paths of influence, in this case, can only explain a zero-order correlation of -.03.

It is argued here that a far more useful approach to the problem of causality involves a recognition of the importance of variable sequence. Thus, it is possible to envision a series of relationships

The criminal conception of homicide surfaces as a critical variable in the analysis of legal treatment. In addition to the financial inability of certain segments of the population to purchase legal advantages, persons also may be penalized because of their conformity to such an imagery. Those defendants whose personal or social characteristics suggest their participation in a culture where the violent response is said to be appropriate are denied the presumption of innocence constitutionally guaranteed to all. The processing of such persons, consequently, takes on a routine nature. For when guilt is presumed, little justification can be found for providing defendants with the combative tools essential for successful defense of their cases. The lack of legal resources that mediate between initial charge and final outcome is, in turn, instrumental in maintaining an imagery of guilt. Assignment of public counsel identifies the individual with that class of persons, the indigent, out of which the criminal stereotype is formulated; denial of bail defines the defendant as a potential danger to society; and waiver of trial by jury is self-admission of criminal involvement. Those defendants, therefore, whose access to private attorney, bail or jury trial is blocked, are further confirmed in the stereotype of the crime within which they were originally defined. The sequence of events, from designation as apparently criminal through lack of provision of legal resources for their defense, produces the outcome predicted by legal wisdom—official award of criminal conviction.

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- where the effects of one stage become the causes of another, and where variables which are significant predictors of earlier events appear as non-significant causes of later events. (For a discussion of the logic involved in such relationships, see Lin, 1976:26-9).

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MARXIST CLASS CATEGORIES AND INCOME INEQUALITY *

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Marxian class categories have been almost totally ignored in systematic quantitative studies of social stratification and income inequality. Occupational status or a similar variable is almost always used as the core criterion defining the individual's position in the system of stratification. This study provides a preliminary operationalization of the Marxian class categories for use in quantitative research. The three most important of these classes—workers, managers and employers—then are analyzed to see what interactions occur between class position and the usual variables used in predicting income (education, occupational status, age and job tenure) and between class and race-sex categories. It was found that there is a substantial interaction between class position and the income returns to education; within class categories, however, there are no differences between race and sex groups in the returns to education.

In the study of social stratification, there is a major disjuncture between theoretical traditions and quantitative empirical work. Of all the theoretical traditions in sociology, social inequality probably plays the most central role in the Marxist perspective. Yet, quantitative investigations of the

causes and consequences of inequality have almost totally ignored Marxian categories. Marxists have been suspicious of quantitative, multivariate approaches to the study of social reality, and the practitioners of multivariate statistics generally have viewed the Marxist perspective as offering little of interest for empirical research.

The present research is an attempt to bridge this gap between the Marxist theoretical perspective and the rapidly growing body of quantitative studies of social inequality. The first part of the paper will discuss briefly the logic of the Marxist concept of class and present a preliminary operationalization of the Marxist criteria for class position for use in quantitative research. We then will examine an empirical

* We would like to express our gratitude to Robert Quinn, Graham Staines, Linda Shepherd and others at the University of Michigan Survey Research Center for their assistance throughout this study. We would also like to thank Tom Rothenberg, Arthur Stinchcombe, Barbara Heyns, Robert Kahn and Marcia Wright for their many helpful suggestions on various portions of the work, and Sam Bowles, Christopher Jencks and Otis Dudley Duncan for their written comments on an earlier version of the paper.

application of this operationalization in the study of income inequality.

THEORETICAL BACKGROUND

*The Marxist Concept of "Class"*¹

Few concepts in social science have been used in more diverse ways than "class." To some sociologists, class refers to categories of people occupying common positions within status hierarchies (Warner, 1960; Parsons, 1970:24; Williams, 1960:98). Others define classes as conflict groups determined by their position within authority or power structures (Dahrendorf, 1959:138; Lenski, 1966:95). Sociologists within the Weberian tradition see class as identifying groups of people with common economic "life chances" determined largely by market relations (Weber, 1968:927; Giddens, 1973; Parkin, 1971:18-23). In contrast to these usages, Marxists have defined class primarily in terms of common structural positions within the social organization of production (Bukharin, 1969:276; Lenin, 1969:486). In contemporary American society, this means defining class in terms of positions within capitalist social relations of production.

There are three important elements of this conception of class: classes constitute common *positions*, those positions are *relational* and those relations are rooted in the social organization of *production*. To say that classes constitute positions implies, to use Przeworski's (1976:3) apt expression, that there are "empty places" in the social structure which are filled by individuals. The analysis of class must be understood primarily as the analysis of such empty places, and only secondarily of the actual individuals who fill the slots. While questions of social mobility are important in a class analysis, there is a logical priority to understanding the empty places into which individuals are sorted (see Poulantzas, 1973:49-50; Marx, 1967:10).

Classes are not, however, just any "empty places" in social structure which

can be ordered in a hierarchical fashion. In the Marxist perspective, classes are not, as Barber (1957:73), would have it, "divisional units within systems of social stratification." Classes constitute common positions within social *relations* of production, and this means that classes must always be understood in terms of their relationship to other classes. Thus, the theoretical starting point of a class analysis is to decode the social relations of production within a particular society in order to uncover the class positions which they determine.

The traditional Marxist analysis of the class structure of capitalist society has centered on three criteria underlying social relations of production: (1) ownership of the means of production; (2) purchase of the labor power of others; (3) sale of one's own labor power.² These three criteria generate the three basic class categories of capitalist society: *capitalists* own their own means of production, purchase the labor power of others and do not sell their own labor power; *workers* do not own their own means of production and therefore cannot purchase the labor power of others, but do sell their own labor power to capitalists; and the *petty bourgeoisie* do not sell their own labor power, nor (except perhaps in a very limited way) purchase the labor power of others, but do own their own means of production. For many purposes, especially for the analysis of mid-nineteenth century capitalism, these were probably adequate criteria, at least as a first approximation; for the analysis of contemporary capitalism, they need some important extensions.

The present analysis will focus on only one of the possible extensions of this basic typology, the emergence of an authority structure within the capitalist enterprise which is *partially* differentiated from own-

¹ For a much more elaborate discussion of the Marxist concept of class, see Wright (1976a:20-90; 1976b).

² It might appear that sale of one's own labor power is equivalent to not owning the means of production and thus is a redundant criterion. However, in all class societies there are people who neither own the means of production nor sell their labor power—in precapitalist society, slaves; in capitalist society, students, many housewives and others who do not participate directly in production.

Table 1. Expanded Marxist Criteria for Class

	Criteria for Class Position			
	Ownership of the Means of Production	Purchase of the Labor Power of Others	Control of the Labor Power of Others	Sale of One's Own Labor Power
Capitalists	Yes	Yes	Yes	No
Managers	No	No	Yes	Yes
Workers	No	No	No	Yes
Petty Bourgeoisie	Yes	No	No	No

ership. This expanded typology is presented in Table 1. The traditional Marxist criterion of "employing labor power" really contains two distinct dimensions. First, being an employer gives the capitalist legal rights to the *product of labor*. Second, being an employer gives the capitalist control over the *activities of labor*, over the labor process. In nineteenth century capitalism, these two dimensions tended to be united in the entrepreneurial capitalist; today, especially in the large corporation, they tend to be partly differentiated.³ This

implies the emergence of a new social category, generally referred to as managers. Managers in this typology are wage-laborers who do not own their own means of production, do not formally employ workers, but who do control or supervise labor power. Thus they share some of the criteria of both workers and capitalists, which reflects the structural position of managers as a buffer between the capitalist class and working class.⁴ This enlarged typology will

³ Opponents of the Marxist view of class have argued that with the institutional separation of ownership and control in the modern corporation, property ownership has progressively declined as an important element in the structure of inequality and, thus, an analysis of capitalist social relations of production per se is unnecessary (see especially Bell, 1973; Berle and Means, 1932; Galbraith, 1967). Of course, no one can deny the considerable growth of managerial hierarchies in the modern corporation and the general decline of the family-owned firm in favor of the joint stock company (although, as Zeitlin (1974) argues, there are considerable data to indicate that proponents of the "managerial revolution" thesis have grossly exaggerated these changes). The issue is not whether or not professional managers play a bigger role in running corporations today than 100 years ago, but how such managerial positions should be structurally interpreted. Obviously, if property ownership is taken to be simply a juridical category defining formal legal title to the apparatus of production, then the emergence of managers at the top of large corporations signals the demise of property relations. But proponents of the class structure perspective increasingly have stressed that ownership must not be understood primarily in legalistic terms. Rather, ownership of the means of production constitutes a complex system of social relationships, of enforceable rights and claims to the apparatus of production (see especially, Balibar, 1970; Poulantzas, 1975; Bettelheim, 1973;

De Vroey, 1975). It is a sociological question, not a formal legal one, whether the appearance of a separation of ownership and control, in fact, may hide a new integration of the two. Understood in this way, the main significance of the rise of managerialism is not the abolition of property relations, nor the demise of the capitalist class but, rather, the reorganization of ownership structures from individual forms of capitalist ownership to more collective forms of ownership. The dominance of the capitalist class is no longer primarily mediated through personally manipulated individual businesses, but through collectively run and coordinated corporate empires (see Menshikov, 1969). Capitalist social relations of production have not been transcended; they have merely been transformed. The Marxist class structure perspective thus argues that relations of production—understood substantively, not formally—remain the core criteria for understanding class structure and class antagonism.

⁴ It is very important not to confuse this typology with Dahrendorf's (1959) conception of class as common position within authority hierarchies. Two points of contrast are especially important: in Table 1, "authority" represents only one dimension of class relations; whereas in Dahrendorf's analysis, authority becomes the sole criterion for class position. Second, in the present discussion, authority is defined specifically in terms of the social relations of production, not the social relations of any organization whatsoever. For Dahrendorf, authority relations within churches are as much the basis of class position as authority relations within industry. For a

provide the basic conceptual framework within which the present research will be conducted.

Several brief comments about the typology in Table 1 are necessary. First, to say that this is a structural typology in which a person is placed in a class category on the basis of the four criteria does not imply that there are no ambiguous cases. For example, should a worker who in his or her spare time is a small-scale, self-employed artisan be considered a petty bourgeois producer? It is partially an empirical question rather than a purely theoretical one how neatly and unambiguously individuals can be placed into the slots. The critical point is that such ambiguity takes shape and has theoretical meaning only in its relationship to the structural categories.

Second, and perhaps more seriously, there is a certain ambiguity in the criteria themselves quite apart from the possible ambiguities of placing individuals into particular slots. Two such ambiguities are particularly important. (1) There is an ambiguity in the boundary between the capitalist and petty bourgeois classes. As defined in Table 1, petty bourgeois employ no labor power. This is surely an overly stringent criterion; a small shopkeeper who employs one helper is not in a different class position from a small shopkeeper who employs no one. The criterion "employs labor power" thus does not provide a complete definition of the difference between capitalists and the petty bourgeoisie. (2) In a complementary way, there is a certain ambiguity in the boundary between the capitalist class and the managerial category. As defined in Table 1, the president of General Motors would be called a "manager" rather than a capitalist, in spite of the fact that most of his income comes from the direct appropriation of profits. Clearly, *formal* legal ownership of the means of production and formal legal status as the employer of labor power are not sufficient criteria to differentiate capitalists from managers. For present purposes, however,

we will ignore both of these ambiguities. The typology in Table 1 therefore should be regarded as a first approximation clearly in need of refinement rather than as a fully elaborated class typology.⁵

Third, to say that these are structural categories does not imply that they are completely homogeneous, without any significant internal differentiation. There are large, wealthy capitalists and small, local capitalists; top managers and foremen (the lowest level in the managerial hierarchy); highly educated, well-paid workers and marginal, poverty-stricken workers. It would be absurd to argue that class and class alone is the only important element defining an individual's place in the system of inequality. But again, the point is that these various forms of internal differentiation—of intra-class stratification—are to be understood theoretically in terms of their relationship to the more general structural class categories.

Finally, it is very important not to think of these class categories as occupational groupings. Class, as defined in Table 1, is a way of looking at social structure entirely different from occupation. The term "occupation" designates positions within the *technical* division of labor, i.e., an occupation represents a set of activities fulfilling certain technically defined functions. Class, on the other hand, designates positions within the *social* relations of production, i.e., it designates the social relationship between actors. Knowing that an individual is a carpenter, for example, tells you that within the technical division of

fuller discussion of the relationship of Dahrendorf's concept of class to a Marxist perspective, see Wright (1976a:ch. 1).

⁵ Many critics of the Marxist framework have argued that the fact of such ambiguities negates the value of a structural class model altogether. This is equivalent to saying that because the platypus has webbed feet and a bill, the concept of "mammal" is useless. The point is that ambiguities are ambiguities precisely because of their relationship to structurally defined categories. It is, of course, important for a structural theory to provide an understanding of the ambiguities rather than to ignore them, but there is no intrinsic incompatibility between a structural theory and structural ambiguities. For a more thorough discussion of ambiguities in the class structure, see the analysis of "contradictory locations within class relations" in Wright (1976b). See also Carchedi (1975a; 1975b).

Table 2. Occupational Distribution within Class Categories (Full-Time Participants in the Labor Force Only, 1969 Survey of Working Conditions)

Occupation	Class Categories ^a				
	Employers	Managers	Workers	Petty Bourgeoisie	Total
Professionals and Technicians	2.9%	20.2%	12.5%	11.8%	11.1%
Managers, Proprietors and Officials	72.1	17.1	1.7	41.2	14.5
Sales	2.9	4.5	5.6	2.0	4.9
Clerks	0.0	14.1	20.0	0.0	15.2
Craftsmen	6.7	21.8	14.5	9.8	16.0
Operatives	1.0	13.8	29.4	3.9	20.3
Laborers	1.0	0.8	4.5	2.0	2.8
Service Workers	1.9	6.1	10.4	2.0	7.7
Farmers	11.5	0.4	0.0	27.5	2.8
Farm Laborers	0.0	1.2	1.2	0.0	1.0
Total	100.0%	100.0%	100.0%	100.0%	100.0%
N	104	491	664	51	1310

^a See Table 3 for operationalizations of class position in the 1969 Survey of Working Conditions.

labor he/she physically transforms lumber into buildings; but it tells you nothing about that individual's class position. A carpenter could easily be a worker, a petty bourgeois producer, a manager or even a small capitalist. The occupational distribution for the various class categories is given in Table 2.

Operationalization of the Class Categories

The data for the present study come from the 1969 "Survey of Working Conditions" and the 1973 "Quality of Employment Survey" conducted by the University of Michigan Survey Research Center.⁶ A number of questions from the 1969 survey enabled us to construct a class typology that was reasonably close to the Marxian typology.

1. "Most of the time on this job do you work for yourself or someone else?"
2. "If you are self-employed, are there any people who work for you and are paid by you?"
3. "Do you supervise anybody as part of your job?"

⁶ The 1969 survey consisted of a national random sample of 1,533 adults 16 years and older; the 1973 sample consisted of a national random sample of 1,496 adults. In both surveys, only individuals active in the labor force were included.

These three questions generate a total of five categories as illustrated in Table 3.⁷

⁷ Several comments about these categories are necessary:

Employers: We are calling this category "employers" rather than capitalists since most of the individuals in the sample who fall into this category are extremely small businessmen. Seventy-eight percent reported that they employed nine or fewer workers, and only eight percent said that they employed 50 or more.

Managers and Workers: In response to the question about having subordinates, a majority of elementary and secondary school teachers responded that they did supervise people on their jobs. This would formally place them in our manager category. However, with very few exceptions (such as teachers who hold administrative jobs as well as teach), teachers should be classified as workers, not managers, since they do not supervise labor power. Therefore, we have reclassified all teachers as workers regardless of their responses to the question about having subordinates.

Petty Bourgeoisie: A pure *rentier* capitalist—i.e., an owner of stocks and other assets who did not employ anyone directly—would fall into the petty bourgeoisie on the basis of the criteria in Table 3. In reality, such an individual should be classified within the capitalist class, since the role of managing the flow of capital represents part of the capitalist position within social relations of production. Since it is unlikely that many such individuals are included in the present sample, this misclassification makes little practical difference.

Ambiguous: This category does not have a clear meaning in the present study. Except for the special case of a self-employed consultant who su-

Table 3. Operationalization of the Marxist Class Typology (1969 Survey)

	Operationalized Criteria for Class Position				N	%
	Self-Employed	Have Employees	Have Subordinates on the Job	Employed		
1. Employers	Yes	Yes	Yes*	No	110	7.4%
2. Managers	No	No	Yes	Yes	561	37.4%
3. Workers	No	No	No	Yes	739	49.2%
4. Petty Bourgeoisie	Yes	No	No	No	65	4.3%
5. Ambiguous	Yes	No	Yes	No	27	1.8%

* Three individuals reported that they were employers but had no subordinates. These individuals were included in the employer category.

EMPIRICAL APPLICATION OF THE CLASS TYPOLOGY

General Hypotheses

The class categories discussed above will be used in an empirical study of income inequality. There will be three focuses to this investigation.

Comparison of the explanatory power of class and occupational status. Virtually all of the recent sociological work on income inequality has used occupational status, occupational prestige or some closely related metric of occupations as the key mediating variable between the individual's social background and education and the individual's income. We have posed the concept of class, defined in terms of capitalist social relations of production, as an alternative paradigm for understanding inequality. One way (by no means the only way) of comparing these paradigms is to estimate a series of income regression equations and then to examine the relative changes in explained variance when class and other variables are included and excluded in various combinations. The expectation is that class position will have at least as much an effect on an individual's income as will the individual's occupational status, and that this effect will not disappear when status is controlled for.

Examination of the different patterns of

pervises other people's employees, this category probably represents a response error. Since less than two percent of the sample falls into this category, we will exclude it from all subsequent analyses.

income determination between classes. If class position is a critical mediating variable between social background and income, then it would be expected that class position would affect the ways in which background characteristics get transformed into income. That is, we hypothesize not only that class position has an independent impact on income from occupational position, but also that it affects the extent to which background characteristics themselves can be "cashed in" for income. In particular, the expectation is that class position will have a strong influence on the extent to which education influences income.

More concretely, we predict: (a) that the returns to education will be much greater for managers than for workers and (b) that the income of employers will be higher than that of managers and workers at every level of education, but that the returns to education for employers will be less than for managers.

The reasoning behind the first hypothesis is as follows. For *both* workers and managers there should be some positive returns to education for a variety of reasons: educated labor costs something to produce and, thus, the income going to educated workers and managers generally will include an increment to cover those costs at least partially; educated labor tends generally to be in short supply relative to uneducated labor and, thus, market forces will tend to push up the incomes of both skilled workers and managers.

However, because of the specific posi-

tion of managers within the social relations of production, there are forces which tend to increase the returns to education among managers above the returns for workers. The argument has two steps. First, because of the problems of social control of managerial labor, managerial hierarchies will tend to be characterized by elaborate incentive structures. In particular, the income gradients associated with managerial career ladders will tend to be quite steep. This, in turn, will create steep income gradients associated with position within the managerial hierarchy. Second, among managers, there will tend to be a fairly close relationship between education and position within the managerial hierarchy. This association is the result of several factors. (1) Higher levels of the hierarchy require certain skills that are produced (or at least certified) within the educational system. (2) The educational system socializes people to the work norms demanded for different positions in the hierarchy. In particular, higher education develops habits appropriate for higher levels within the authority structure. (3) The educational system helps to legitimate the managerial hierarchy as a whole through the meritocratic ideology of rule by experts and, thus, there will be a tendency for people with lower credentials *not* to be promoted above people with higher credentials. As a result of this combination of steep income gradients and steep educational credential gradients tied to managerial position, we expect that managers will have particularly high income returns to education. This reasoning will be discussed in greater detail in the final section of this paper.

The second hypothesis is based on the view that the income of employers is fundamentally a consequence of the quantity of property (capital) controlled by the employer rather than the employer's education. Therefore, education should matter for the employer's income only if, *among employers*, there is a strong relationship between quantity of property and level of education.

It is very important to understand the logic of these comparisons of returns to education in different classes. We are not

touching directly on questions of social mobility, i.e., how people get sorted into class positions. What we are asking is: once a person gets into a class position, by whatever sorting process, does the class position itself have an impact on how much difference an individual's education is likely to make for the individual's income? We are therefore, in a sense, using regression equations to tap characteristics of the structure of class itself rather than to represent the status-attainment process at the individual level.⁸

Examination of the differences in patterns of income determination between blacks and whites and between men and women within classes. One of the main preoccupations in the stratification literature has been the analysis of differences in the status-attainment process among blacks and whites and, more recently, men and women. One of the most robust findings of this body of research is that blacks in general receive less income per increment in education than do whites (see Siegel, 1965; Duncan, 1969; Weiss, 1970).⁹ None of

⁸ This point may be somewhat clearer if we consider a more concrete example. Suppose we wished to compare the relation of education to wages in two businesses. In one business, education is irrelevant for pay; in the second business, pay scales are closely pegged to educational credentials. There are two empirical strategies that could be adopted for revealing this pattern: formal pay scales and job requirements in the business records of the two firms can be used to estimate the returns to education; or a survey of the personnel of the businesses and use of individual-level data to estimate the returns. In either case, the resulting regression equations should be seen as characterizing the firms rather than the individuals. This becomes most obvious in a situation in which education itself becomes an important criterion for an individual entering one or the other of the two businesses. In such a case, the regressions of income on education *within* each firm would tell very little about the total relationship of education to income for individuals. In our analysis, class categories are analogous to the firms, and the regression equations should be interpreted as reflecting characteristics of the class positions as such.

⁹ The work of Stolzenberg (1975) represents a partial exception to this general finding that the returns to education are lower for black males than for white males. Stolzenberg finds that within detailed occupational categories the *rate*

these studies, however, has controlled for class position defined in terms of social relations of production. If it is true that managers receive much higher returns to education than workers, as hypothesized above, and if it is true that blacks are more concentrated than whites in the working class, then it would be expected that much of the differential returns to education for blacks and whites might be due to the distribution of races across class categories. Therefore, we will compare the returns to education for blacks and whites *within class categories* in order to control for this class composition effect. We will do the same analysis for men and women.

Statistical Method

For the direct comparison of class and status the following standardized regression equation will be estimated:

$$\text{Income} = \beta_1 \text{ Education} + \beta_2 \text{ Age} + \beta_3 \text{ Status} + \beta_4 \text{ Employer Dummy} + \beta_5 \text{ Worker Dummy}$$

We will then examine how much the R^2 for the equation drops when the class dummy variables are excluded, when status is excluded and when both are excluded.

The analysis of the interaction patterns will involve a fairly straightforward application of the analysis of covariance. We will compare the various class categories and sex and race categories within classes in terms of a series of regression equations to see (a) if they differ significantly in the

slopes of the independent variables in the equations and (b) if a significant "gap" in income exists between the compared groups when the independent variables are controlled for. Figure 1 illustrates these two kinds of comparisons for the simple regression of income on education for managers and workers. The test for the significance of slope differences simply involves testing whether the slope of the manager minus worker regression is significantly different from zero.¹⁰ It is somewhat less obvious how to test the significance of the gap in income. The difficulty is that the magnitude of this gap is strictly dependent upon the level of education at which it is assessed. Thus, in Figure 1, if managers and workers were compared at zero education (the usual constant term in regression equations), the gap would be slightly negative; if they were compared at the workers' mean education (\bar{E}_w), the gap would be positive, but relatively small; if they were compared at the managers' mean education (\bar{E}_m), the gap would be positive and large. There is no standard convention as to which of these possibilities is the most appropriate. In the present analysis, we will assess the gap in income at the value of the independent variables halfway between the means for each of the two groups being compared. In Figure 1, this is indicated as $\frac{\bar{E}_w + \bar{E}_m}{2}$. The gap at this point represents

the expected difference in income between a worker and a manager with identical education equal to the average of the mean worker and mean manager education. We will refer to the comparison of expected incomes at this point as the analysis of the "average gap" in income between the groups being compared.

of income returns are fairly similar for the two races. However, Stolzenberg's results are not strictly comparable to those of the other studies cited above or the present research since he measures the *rate* of income returns to education rather than income returns as such. Stolzenberg uses a log transformation of the income variable in order to measure approximately the proportional increase in income for a unit increase in education rather than the absolute increase in income for a unit increase in education. It is quite consistent to find that blacks receive much lower absolute increases in income than whites per unit increase in education and yet that they receive equal (or even greater) proportional increases in income. Since at every level of education the income of blacks tends to be less than that of whites, a smaller absolute increase in income may produce an equivalent proportional increase.

¹⁰ This is accomplished by constructing a dummy variable for one of the nominal categories in the comparison and then estimating a regression equation with education, the dummy variable, and the dummy variable times education as independent variables. In such an equation, the dummy variable interaction term represents the difference in slopes for the two groups being compared. A direct t-test of this coefficient thus tests the significance of slope differences between the two groups (see Kmenta, 1971:419-23).

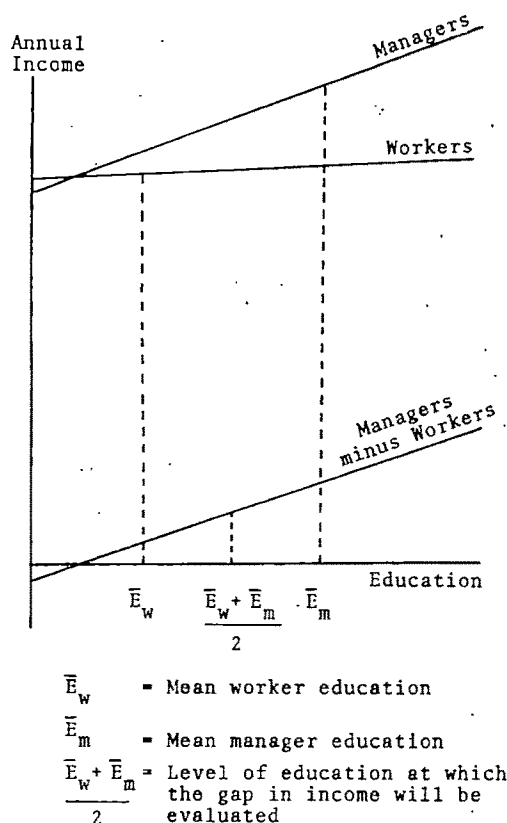


Figure 1. Illustration of the Method of Structural Comparisons

Since the average income gap is assessed at a different level of the independent variables in each comparison, it is not possible directly to compare average income gaps for different comparisons. In order to do this, it is necessary to assess the gap at the same point on all comparisons. We will therefore also calculate a "standardized income gap" by assessing the gap at the level of the independent variables of the most privileged category in our analysis—employers. (This is equivalent to the common technique of substituting the means from a privileged group into the equations for a disadvantaged group as discussed by Duncan, 1969.) In Tables 6A and 7A, where we report the basic regression equations for our analysis, this standardized income gap constitutes the difference in "adjusted constants" for the various groups.¹¹

¹¹ To conduct a t-test on these various gaps, it

Specification of Equations for the Analysis of Interaction Patterns

Every regression equation contains the answer to some question. The trick is for the equation which is estimated in fact to answer the question which is asked. Too often in sociological research any variable which is thought to be interesting is thrown into a regression equation without regard to its substantive relevance to the questions at hand.

There are two somewhat different questions which we would like to answer in the analysis of interaction patterns:

1. Do individuals in different classes, and in different race and sex categories within classes, differ in the amount of income they can expect to receive for *each additional increment* of education (comparison of slopes)?
2. Do individuals who are alike in all respects except that they differ in their class position, or who differ only in their race and sex while sharing a common class position, differ in the absolute amount of income which they can expect to receive (analysis of average income gap)?

These two questions pose somewhat different issues for the correct specification of the regression equations. For the first equation, ideally we would like to hold all factors constant which can be considered *causes* of education, but not those which are *consequences* of education. In particular, we would like to include a number of variables which describe the individual's social background and "intelligence." Unfortunately, no such data are available in the present survey. The result is that the estimated education coefficients in our analysis inevitably will be somewhat biased upwards.¹²

simply is necessary to shift the constant term in the dummy-variable interaction regressions to the desired value of the independent variables. A t-test on the constant term in such a regression then becomes a t-test of the income gap between the two groups being compared.

¹² The reason why it would be desirable to have such background variables in the equation

There is certainly no unanimity on the potential biases in education coefficients created by improperly measuring or by omitting social background variables. Bowles (1972), for one, argues that such biases are likely to be substantial. There is, however, considerable research on the effects of social background and education on income (Duncan, 1969; Blau and Duncan, 1967; Jencks et al., 1972) and of measured IQ on income (Jencks et al., 1972) which suggests that the bias in the education coefficient caused by the omission of the relevant background variables should not be terribly large. More importantly, even if these biases are not trivial, since our main interest centers on the *differences* between various groups rather than the absolute values of the regression coefficients, the problem of bias should become less critical. If we are willing to assume that the bias due to the omission of background variables is in the same direction for both groups, then the bias in the estimate of the difference in coefficients will necessarily be less than the most biased of the two individual coefficients and, generally, less than both. In the limiting case where two groups have identical biases, the estimate of the difference in coefficients actually will be unbiased.

Because of this absence of background variables, therefore, the core of the analysis of slopes will rest on comparisons of the simple regression of income on education for the various categories.

For the second question, we would like to hold constant all factors in addition to

education which have any significant impact on income. The list of such factors is, of course, very large and includes occupational status, on-the-job training, seniority, work experience, industrial sector, geographical location, migration, and so forth. For the present purposes, we will limit ourselves to three of these: job tenure, age (as a proxy for work experience) and occupational status.

We are including these additional equations not to provide more precise estimates of the education coefficient as such, but to see if the average gaps in income disappear as controls are added.¹³ Again, the income gap, as we are measuring it, represents the expected difference in incomes between two individuals, one from each group, that would occur if the two individuals had the same levels on all the independent variables equal to the averages of their respective group means. If this gap were entirely due to differences in the levels of education, occupational status, tenure and experience, then the gap should become negligible when these factors are all included in the equation.

While the actual value of the education coefficient in these expanded equations has little intrinsic interest, the magnitude of the difference between groups in the edu-

is *not* because they necessarily would tell us anything about how people get sorted into class positions, but because the class interactions could conceivably be artifacts of various unmeasured background characteristics of the individuals occupying given class positions. Thus, if managers have greater returns to education than workers, it might be because they have different motivations due to higher status backgrounds. What appears as an education slope difference between managers and workers would, in fact, turn out to be a slope difference between high status and low status backgrounds. In effect, therefore, when we compare education slopes we are comparing returns to education plus returns to all the unspecified causes of education which also influence income.

¹³ In certain respects, these expanded regression equations provide less meaningful estimates of the effects of changes in education per se on income than does the simple regression. In particular, the inclusion of occupational status introduces a substantial downward bias in the estimate of the education coefficient since it holds constant one of the basic mechanisms by which education influences income. The regression which contains occupational status as well as education provides an answer to a rather strange question: how much additional income could an individual expect to receive for an increase in education providing that this increased education did not lead to a change in occupation? The constraint of not changing occupation (or at least not changing occupational status) certainly reduces the amount of extra income per increment of education, i.e., it biases the education coefficient downward. Unless this equation appears as part of a system of recursive equations (as in path analysis), the inclusion of the occupational status variable in the equation clearly gives a less meaningful estimate of the effects of education per se on income than does the simple regression of income on education.

cation coefficient in these expanded equations may give some clue as to the possible mechanisms which produce the differences between classes observed in the comparisons of the simple regressions. In particular, if the inclusion of the occupational status variable wipes out any differences between classes in the slope of the education variable, it would suggest that the differences in slope in the simple regression might be substantially the result of the occupational composition of the different class categories. Thus, while the basic purpose for constructing these expanded equations is to examine the average income gap between groups, we will also look at the effect of including the additional variables on the differences in the education slopes.

Therefore, for each interaction comparison, we will estimate two equations:

1. the simple regression of income on education;
2. the regression of income on education, tenure, age and occupational status.

Variables

Class. Class is measured by the criteria in Table 3. Unfortunately, in the 1973 replication of the original 1969 survey of working conditions, the question about employing others was dropped from the questionnaire. This means that the employer category and category 5 in Table 3 were merged, making the 1973 data somewhat less reliable in comparisons between employers and other classes. We will therefore rely on the 1969 survey for the analysis of interaction patterns.

Occupational status. Status was measured using the standard Duncan SEI scores. However, in the 1969 survey, only decile values from the Duncan scale were coded rather than the full two-digit scale.¹⁴ In our direct comparisons of class and status, we will rely on the 1973 data in order to avoid any problems of attenuation which might result from using decile scores. By using the full status scale and a slightly less well-measured class variable in these comparisons, we are maximizing the relative explanatory power of status.

Since in the analysis of the interaction patterns we are less concerned with the coefficients of the status variable as such, we will use the 1969 data in order to have the most reliable measures of class position. In any event, the results for the direct comparisons of class and status using the 1969 data and for the interactions using the 1973 data are substantively the same as those reported below.

Annual income. Respondents were asked to indicate their total personal annual incomes before taxes as well as the amount and frequency of their paychecks. If they failed to answer the annual income question, annual income was estimated from the response to the paycheck question. Annual *income* is being used rather than simply annual *earnings* since, for comparisons between classes (especially between employers and other classes), the exclusion of unearned property income obviously would understate real class differences. Total income therefore should be seen sociologically as an indicator of total economic rewards regardless of economic source.

Education. Education is measured by a quasi-credential scale rather than by years of education in the following manner:

- 0=elementary school or less
- 1=completed elementary school
(grade 8)
- 2=some high school
- 3=high school completed
- 4=some college

¹⁴ The decile status scale used in the survey is related to the full two-digit scale as follows:

Decile	Full Scale
0	01-06
1	07-13
2	14
3	15-18*
4	19-21
5	22-31*
6	32-39*
7	40-51*
8	51-65*
9	66 and over

* The decile split in these cases occurred in the middle of a category. In these situations, cases were randomly assigned above and below the 10 percent cut-off point.

5=college degree
6=post-college.

Each step on this scale represents a socially-recognized level of education. Although in practice it is highly unlikely that any of our results would have been different if years of education had been used instead of this scale, we felt that in an analysis of income determination a "credential" is a more appropriate unit of education than a year.

Job tenure and age. The job tenure variable is a simple measure of the number of years worked on the current job. Unfortunately, no question was asked on the survey about general work experience beyond tenure in current job. While realizing that there are some problems with age as a variable in an analysis of income determination, age will be used as a loose proxy for general work experience.

The Sample Used in the Analysis

For the purposes of the present research, we have restricted the analysis to those respondents who work full time, defined as 35 hours a week or more. While this reduced the sample size somewhat, especially for women, we felt that the analysis would be more straightforward if we avoided the special problems of analyzing the market for part-time labor.

In the analysis of interaction patterns, the sample also will be limited to workers, managers and employers, the three most important classes in advanced capitalist societies. In many respects, the petty bourgeoisie represents a remnant from an earlier era of capitalist development and, as a class, certainly is progressively becoming less important. While this class may be of considerable interest for certain problems; we will simplify the present study by excluding it from the analysis.

Finally, farmers and farm laborers (less than 4% of the total sample) are being excluded because of the difficulties of assessing income in kind for agricultural occupations. All of the results reported below remain virtually unchanged if farmers and farm laborers are included in the analysis.

RESULTS

Direct Comparison of Occupational Status and Class Position

Table 4 presents the regressions used to assess the relative contributions of occupational status and class to the explained variance in income. Table 5 presents the corresponding correlation matrix. In order to make these results as comparable as possible to the existing research on income determination and status attainment, we have restricted the sample to white males, nonfarm full-time participants in the labor force.

Education and age account for just over 15% of the variance in income. The additional increment in R^2 resulting from adding occupational status to this equation is only 4.1%, whereas the increment from the two class dummies is 9.4%. All of the variables taken together explain just under 27% of the variance in income among white males. Looked at in a slightly different way, when class is added to the equation containing status, the R^2 increases by 7.6%, whereas when status is added to the equation containing class, the increase is only 2.3%. Status and class alone each explain just over 14% of the variance in income. These results are highly suggestive that an extremely simple version of the Marxian class typology is at least as potent a variable in predicting income as the full Duncan socioeconomic scale.

Class Comparisons

Figure 2 and Table 6 present the results of the comparison of the various class categories for the simple regressions of income on education. Table 7 presents the results for the expanded regressions. Table 8 presents the mean values and standard deviations of the variables for all the class-race-sex categories being compared.

The results shown in these tables and figures strongly support the view that class has a substantial effect on the relationship of education to income. The results can be summarized as follows.

Workers versus employers and managers. The education slope for workers

Table 4. Comparison of the Explanatory Power of Occupational Status and Class (1973 Survey, White Male, Nonfarm, Full-Time Participants in the Labor Force Only)

Variables Included in the Regression Equation	Standardized Regression Coefficients [Dependent Variable=Annual Income]					R ²
	Education	Age	Decile Occupational Status	Employer Class Dummy	Worker Class Dummy	
1. All Variables	.16	.19	.21	.26	-.06	.269
2. Education Only	.27					.071
3. Education and Age	.32	.28				.152
4. Education, Age and Status	.15	.23	.26			.193
5. Education, Age and Class	.28	.22		.27	-.11	.246
6. Status Only			.38			.143
7. Class Only				.29	-.17	.145
Comparison of Relative Changes in R ²						
Equations Compared	Interpretation of the Comparison					Increment in R ²
4-3	Status Net of Education and Age					.041
5-3	Class Net of Education and Age					.094
1-5	Status Net of Education, Age and Class					.023
1-4	Class Net of Education, Age and Status					.076

was significantly flatter than for managers or for employers, and the overall level of the regression line was considerably lower in the simple regression equations (Table 6A:2-3; Figure 2A). Workers received, on the average, \$1,119 less for each increment in education than managers and \$3,413 less than employers. The average income gap between workers and managers was \$2,529 and between workers and employers, \$6,865. All of these differences were significant at the .001 level.

The addition of job tenure, age and occupational status into the equations did not eliminate the average gap in income between workers and managers or employers (Table 7B:2-3); i.e., most of the gap between the regression lines in Figure 2A cannot be attributed to the age-tenure-occupational status composition of the class categories.

Not only do the age, tenure and occupational status variables fail to close the

income gap between classes, they fail to eliminate the differences in the slopes of the education variable. Even when the control variables are added, workers still receive some \$2,563 less income per increment of education than employers, and \$870 less than managers. Furthermore, workers also receive much less additional income for increments in decile occupational prestige than either employers or managers; workers receive over \$2,000 less for each decile of prestige than employers and \$400 less for each decile than managers. These results further support the notion that the differences between classes cannot be considered simply artifacts of the mix of occupations in the different classes for if that were the case, the classes would not be expected to differ in the slopes of the occupational status variable itself.

Managers versus employers. The income gap between managers and employers was substantial in the simple regression equa-

Table 5. Correlation Matrix for Variables in Table 4

Variables	2	3	4	5	6	Mean
1. Income	.27	.23	.38	.35	-.26	11,715
2. Education (Credentials)		-.17	.60	.05	-.09	3.3
3. Age			.12	.15	-.21	38.3
4. Occupational Status				.15	-.31	43.3
5. Employer Class Dummy					-.30	.105
6. Worker Class Dummy						.440

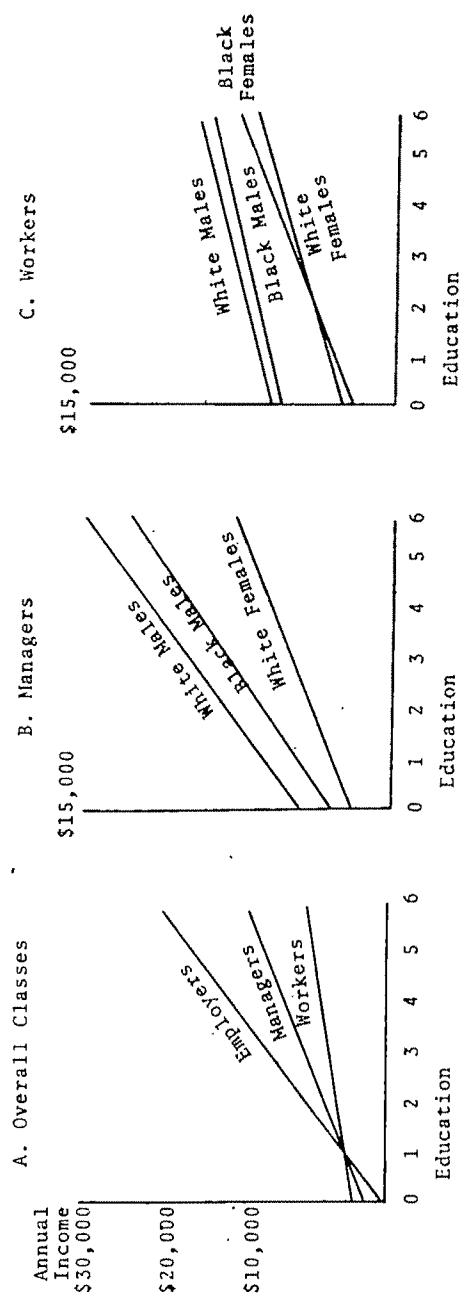


Figure 2. Comparison of Classes and of Race and Sex Groups within Classes for Simple Regression of Income on Education

	Adjusted Constant *	Unstandardized Education Coefficient	N
Overall Class Categories			
Employers	\$14,273	\$4,091	72
Managers	9,398	1,797	452
Workers	6,756	678	628
White Males			
Employers	15,062	3,927	64
Managers	10,784	1,937	319
Workers	8,218	764	318
White Females			
Managers	5,445	1,042	94
Workers	4,875	862	230
Black Males			
Managers	8,796	1,766	24
Workers	7,492	670	37
Black Females			
Workers	5,190	1,086	35

	Average Income Gap *	t-Value for Income Gap	t-Value for Differences in Education Coefficients
Overall Class Comparisons			
1. Employer versus Manager	\$5081	.5.3†	3.0***
2. Employer versus Worker	6865	10.4†	6.6†
3. Manager versus Worker	2529	9.2†	6.0†
White Males			
4. Employer versus Manager	.4471	4.0†	2.3**
5. Employer versus Worker	6162	6.8†	4.7†
6. Manager versus Worker	2436	6.3†	4.6†
White Females			
7. Manager versus Worker	574	2.4**	<1
Black Males			
8. Manager versus Worker	653	<1	1.9*
Managers			
9. White Male versus Black Male	1986	1.5	<1
10. White Male versus White Female	5527	8.6†	1.5
11. Black Male versus White Female	3351	4.2†	1.3
Workers			
12. White Male versus Black Male	659	1.3	<1
13. White Female versus Black Female	-230	<1	1.2
14. White Male versus White Female	3373	16.0†	<1
15. Black Male versus Black Female	2620	5.7†	1.6

Significance levels (two-tailed test) * .10 level
** .05 level
*** .01 level
† .001 level.

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Table 7A. Unstandardized Regression Coefficients for Expanded Regression Equations (Nonfarm, Full-Time Participants in the Labor Force Only, 1969 Survey)

	Unstandardized Coefficients [Dependent Variable=Annual Income]				
	Adjusted Constant*	Education	Decile Occupational Status	Age	Job Tenure
Overall Class Categories					
Employers	\$13,850	\$3,170	\$2,359	-\$110	\$77
Managers	10,090	1,477	570	67	43
Workers	7,512	607	147	18	25
White Males					
Employers	14,112	3,195	2,786	-51	23
Managers	11,246	1,555	671	71	6
Workers	8,889	729	184	33	12
White Females					
Managers	5,724	822	357	28	1
Workers	5,504	691	110	-2	72
Black Males					
Managers	10,288	2,245	-15	95	12
Workers	8,896	450	285	30	-123
Black Females					
Workers	5,871	1,227	33	65	-35

* The constant term is evaluated at the employer's mean levels on the independent variables: Education=3.2; Decile Status=7.0; Age=45.9; Job Tenure=11.3.

Table 7B. t-Values and Significant Levels of Average Income Gaps and Differences in Slopes for Various Class, Race and Sex Comparisons, Expanded Regression Equations

	Average Income Gap ^a	t-Values for Differences in Coefficients				
		t-Value for Income Gap	Education	Decile Occupational Status	Age	Tenure
Overall Class Comparisons						
1. Employers versus Managers	\$3,883	3.9†	2.1**	2.9***	1.8*	<1
2. Employers versus Workers	4,270	5.2†	4.6†	5.2†	1.9*	<1
3. Managers versus Workers	1,793	6.3†	3.8†	3.1***	2.1**	<1
White Males						
4. Employers versus Managers	3,238	2.8***	1.8*	2.7***	<1	<1
5. Employers versus Workers	2,909	2.6***	3.4†	4.2†	<1	<1
6. Managers versus Workers	1,557	3.9†	2.7***	2.5**	1.1	<1
White Females						
7. Managers versus Workers	246	<1	<1	2.2**	1.5	1.4
Black Males						
8. Managers versus Workers	379	1.8*	<1	<1	<1	<1
Managers						
9. White Male versus Black Male	997	<1	<1	<1	<1	<1
10. White Male versus White Female	5,365	8.3†	1.2	<1	<1	<1
11. Black Male versus White Female	3,821	4.7†	1.8*	<1	<1	<1
Workers						
12. White Male versus Black Male	537	1.1	<1	<1	<1	<1
13. White Female versus Black Female	-320	1.1	1.7*	<1	2.0**	1.7*
14. White Male versus White Female	3,352	16.2†	<1	<1	2.0**	1.5
15. Black Male versus Black Female	2,377	4.5†	1.6*	<1	<1	<1

^a See note to Table 6B.

Significance levels (two-tailed test) as in Table 6B.

Table 8. Means and Standard Deviations of Variables Used in Regression Equations

	Income mean (s.d.)	Education mean (s.d.)	Decile Status mean (s.d.)	Age mean (s.d.)	Job Tenure mean (s.d.)
All Respondents	\$8,344 (6862)	2.96 (1.5)	5.8 (2.7)	39.6 (13.4)	6.4 (6.9)
Overall Classes					
1. All Employers	15,179 (16252)	3.24 (1.4)	7.0 (1.7)	45.9 (12.2)	11.3 (8.0)
1a. Big Employers (≥ 10 Employees)	19,188 (7918)	3.79 (1.4)	7.6 (1.4)	45.3 (12.7)	10.6 (9.6)
1b. Small Employers (<10 Employees)	12,915 (16064)	3.02 (1.3)	6.9 (1.7)	46.2 (12.3)	11.5 (7.6)
2. Managers	9,226 (6403)	3.42 (1.5)	6.6 (2.3)	41.4 (12.7)	6.3 (6.9)
3. Workers	6,145 (3180)	2.86 (1.5)	5.1 (2.8)	37.4 (14.0)	5.2 (6.1)
White Males					
1. Employers	16,241 (16780)	3.24 (1.4)	7.2 (1.6)	45.1 (12.6)	11.6 (8.4)
2. Managers	10,943 (6730)	3.45 (1.5)	6.8 (2.1)	42.3 (12.5)	7.2 (7.3)
3. Workers	7,727 (3225)	2.82 (1.5)	5.0 (2.6)	38.2 (14.1)	5.7 (6.7)
White Females					
1. Managers	5,188 (2790)	3.46 (1.1)	6.7 (2.1)	39.1 (13.3)	5.6 (5.6)
2. Workers	4,391 (2053)	3.06 (1.3)	5.6 (2.9)	37.0 (14.3)	4.1 (5.2)
Black Males					
1. Managers	8,037 (6011)	3.02 (1.8)	5.5 (2.3)	38.1 (10.9)	7.0 (7.2)
2. Workers	6,804 (2588)	2.28 (1.7)	4.2 (2.6)	36.8 (13.5)	5.3 (6.2)
Black Females					
1. Managers	5,131 (1982)	3.20 (1.8)	5.1 (2.9)	40.3 (11.9)	4.5 (4.5)
2. Workers	4,490 (2419)	2.69 (1.9)	4.4 (3.1)	33.4 (11.6)	4.5 (6.4)

tion (\$5,081) and was only partially reduced by the addition of the control variables. Contrary to our expectations, however, the slope on the education variable was significantly flatter for managers than employers in both regression equations. Some possible interpretations of this result will be discussed in the final section of this paper.

Class comparison within the white male category. By and large, the same results are obtained for white males examined separately as for the entire sample. The education coefficients are significantly lower for white male workers than for white male managers or employers in the simple regression and in the expanded regression. The income gaps were also substantial and statistically significant (Table 6B:5-6 and Table 7B:5-6). Thus, these class differences cannot be considered artifacts of the race-sex composition of the class categories.

In the comparison of white male managers and employers, the income gap is also significant in both equations, and the education slope for managers is significantly flatter in the simple regression (Ta-

bles 6B:4 and 7B:4). In the expanded regression, the absolute magnitude of the difference in education slopes between white male managers and employers is essentially as large as for all managers and employers (\$1,640 compared to \$1,693), although the *t*-ratio of the difference drops just below the 5% significance level. The unexpected result of employers having greater returns to education than managers thus occurs among white males as well as in the entire sample. Again, these class interactions cannot be considered a consequence of the race and sex composition of the class categories.

Class comparisons within the black male and white female categories. Although the direction of the income gap and the differences in the education slopes between managers and workers within the black male and white female categories are the same as within the white male category, the magnitude of the differences tends to be smaller. Among white females the education slopes of workers and managers were not significantly different for either of the regressions and, while there was a significant income gap in the simple regression, this gap was

Table 9. Class-Race-Sex Distributions (Full-Time Participants in the Labor Force Only, 1969 Survey)

Distribution of Classes within Race-Sex Categories					
	White Males	Black Males	White Females	Black Females	Total
Employers	10.9%	6.6%	3.0%	0.0%	7.9%
Managers	42.9	36.8	27.7	22.9	37.5
Workers	41.5	55.3	66.6	77.1	50.7
Petty Bourgeoisie	4.6	1.3	2.7	0.0	3.9
Total	100.0%	100.0%	100.0%	100.0%	100.0%
N	804	76	365	48	1293

Distribution of Race-Sex Categories within Classes					
	Employers	Managers	Workers	Petty Bourgeoisie	Total
White Males	84.6%	71.1%	50.9%	77.1%	62.5%
White Females	10.6	20.8	37.0	20.8	27.9
Black Males	4.8	5.8	6.4	2.1	6.0
Black Females	0.0	2.3	5.6	0.0	3.7
Total	100.0%	100.0%	100.0%	100.0%	100.0%
N	104	485	656	48	1293

almost eliminated with the addition of the control variables (Tables 6B:7 and 7B:7). Among black males, the education slopes were marginally significantly different in both regressions, but the average gap in income was not significant in either regression.

Race and Sex Comparisons within Class Categories

Before examining the differences between race and sex categories within classes, it is important to stress that some of the most significant race and sex effects undoubtedly operate through mechanisms which sort people into the various class categories in the first place. Both women and blacks are underrepresented in the employer category, and women are underrepresented in the manager category as well (see Table 9). Furthermore, the present study is limited to full-time participants in the labor force. It has been shown that, even when educational and skill levels are controlled for, blacks have considerably higher levels of involuntary unemployment than whites (Harrison, 1972). It hardly needs to be added that sexism has acted as a powerful force for keeping women out of the labor force entirely or restricting them to part-time work. When we examine the differences between sex and race categories

among workers and among managers, it should be kept in mind that these are differences which occur after the effects of racism and sexism have operated to keep some people out of the labor force altogether, to prevent others from finding stable full-time employment, and to influence the race and sex distribution among classes of those people who are full-time participants in the labor force.

Race and sex comparisons within the manager class. Black and white male managers do not differ significantly in the slopes of any variables in either the simple or the expanded regressions (Tables 6B:9; 7B:9). It can reasonably be said that an increment of education has essentially the same payoff for blacks and whites, *once they become managers*.

There is greater ambiguity, however, in the analysis of income gaps. While in formal statistical terms, the average income gap between black and white male managers is not statistically different at the 5% level in either the simple or the expanded regression, the absolute magnitudes of the gaps are still large when compared to many of the other comparisons we are making. These results reflect the intrinsic ambiguity of using "significance level" as the criterion of the relative strength of the difference between groups. A low level of significance

of a difference in coefficients can mean either that the two groups really do not differ in the coefficient or that they do differ substantially, but the difference is measured very imprecisely (i.e., the test is of low power).

The comparison of sexes within the managerial class is less problematic (Tables 6B: 10-11; 7B: 10-11). Again, there are no significant differences in the slopes of any of the variables, with the exception of the education slope in the expanded regression in the comparison of white female and black male managers. We have no explanation for this result. However, there is a very substantial average gap in income between white female managers and both white and black male managers in both regression equations. With a large sample, even if it should turn out that there was a significant income gap between black and white male managers, these data strongly indicate that the sex differences among managers are considerably greater than the race differences.

Race and sex comparisons within the working class. By and large, the results for the comparisons of sex and race categories among workers are substantially the same as among managers. None of the race-sex categories differ in education slope in the simple regression and, with the marginal exception of the comparison of black and white women, none differ in the expanded regression (Tables 6B: 12-15; 7B: 12-15). Therefore, it appears that the returns to education are roughly the same for all workers regardless of race and sex.

Furthermore, *within* sex categories, there are also no significant income gaps between blacks and whites. Therefore, as in the managerial category, we cannot reject the null hypothesis that black and white male workers have essentially the same regression equations, and black and white female workers have essentially the same regression equations, especially for the simple regressions of income on education.

Between male and female workers of both races, however, there is a very significant average gap in income. The gap between white female and white male workers is some \$3,300 and between black

male and white female workers, over \$2,600 in the simple regressions. Adding the control variables has virtually no effect on these differences. Thus again, as in the managerial category, the sex differences within the working class are much more striking than the race differences.

GENERALIZATIONS AND INTERPRETATIONS

At the most general level, the results of this study clearly show that classes defined in terms of social relations of production are consequential in American society. The differences between classes in levels of income and in the relationship between education and income are substantial, and these differences do not disappear when we control for variables such as occupational status, age, job tenure, sex or race. Furthermore, in terms of explained variance in income, class position is at least as powerful an explanatory variable as occupational status.¹⁵ A number of other more specific generalizations can be made.

The returns to education within the managerial category are greater than within the working class category. This result was strongly supported both for the sample as a whole and for white males taken separately in both regression equations.

This is one of the most important findings of the study and has considerable implications for the understanding of income inequality. While much more research is necessary to decipher fully this class interaction in returns to education, we can offer some preliminary lines of interpretation. As suggested earlier, we argue that the higher returns to education among managers than among workers reflect the steep income gradients associated with mana-

¹⁵ It must be recognized that class itself still explains only a relatively small proportion of the total variance in income (14.5 percent in Table 4). A full model explaining income variation would have to include many other variables, such as economic sector, region of country, unionization, etc. The critical argument of this paper is not that class explains everything but, rather, that other variables must be examined in terms of their interactions with class position in order to unravel the nature of income determination.

gerial hierarchies on the one hand, and the role of education in sorting people into different levels of the hierarchy on the other.

In both Marxist and non-Marxist traditions, "wages" (and, correspondingly, income from wages and salaries) have been conceptualized as part of an exchange relationship—an unequal, asymmetrical exchange in the Marxist perspective which hides an exploitive relation; an equal, reciprocal exchange in non-Marxist perspectives which reflects the marginal productivity of the wage earner.

An alternative view is that wages are not merely payment for services rendered (i.e., exchange), but are also part of a *structure of social control* within businesses and bureaucracies. Wage differentials should be considered, in part, a political response to the problem of social order within organizations rather than either simply an economic response to the human capital of the wage earner or merely the phenomenal form of a system of surplus value extraction.

The conformity of the individual to the norms of any institution is guaranteed by a variety of threats of punishment for deviance and promises of rewards for compliance. The balance between these punishments and inducements; and their specific forms, depends strongly upon the individual's position in the process of production. For workers in the lowest skill levels, especially when they are in peripheral businesses, the dominant mode of control is threats of various sorts, especially the threat of being fired. For workers with higher levels of skills, particularly when they are employed in core industries, there is much more reliance on various positive inducements: progressively increasing pay and vacation time, good pensions, job promotions, and so on (Stone, 1974). For employees who occupy positions in the authority structure of an enterprise, the inducement mode of control is even more dominant. Repressive forms of control (such as the threat of being fired) become residual instruments used only in the last resort.

The dominance of rewards as the basic

mode of control of managers reflects a basic asymmetry of punishments and inducements as mechanisms of social control: punishments are administered only when concrete infractions are committed and discovered. They are, therefore, generally effective only for preventing bad behavior. Punishments are not a very effective strategy for encouraging responsible behavior. Rewards, on the other hand, can be doled out roughly in proportion to the quality of the individual's performance and thus can be used as a more flexible instrument for encouraging enthusiastic, responsible and even innovative behavior. In the case of workers at the bottom, there is little need for them to perform in a responsible and creative way. As with the private in the army, the employer is mainly concerned that the production line worker more or less does what he/she is told to do. Obedience rather than initiative is the basic performance norm. But for supervisors in general, and supervisors at the middle and top levels of the management hierarchy in particular, the interests of the organization require more than dependability and mechanical obedience. For the power of managers to be wielded effectively, their behavior must be controlled by mechanisms which encourage responsibility rather than simply repress deviance from the rules. It is therefore expected that authority hierarchies will be characterized by very steep income gradients.¹⁰

It might well be asked, what has this to do with the relationship of income to *education* in the manager category and the worker class? In addition to developing various skills, education can be seen as

¹⁰ It should be noted that this interpretation of the authority hierarchy income gradient is quite different from the logic of the Davis-Moore (1945) interpretation. We are not arguing that there is any inherent scarcity of talent or any inherent problems of recruiting people for positions in the authority hierarchy, but rather that once a person is in such a position, there is a problem of social control which is, in part, solved by the structure of income rewards. For a somewhat different elaboration of this perspective which emphasizes the punishment aspect of the control apparatus rather than inducement (see Wright, 1973:314-20).

having two complementary functions with respect to authority structures (Edwards, 1972; Bowles and Gintis, 1975). First, education serves as a legitimation for inequalities of power and second, education helps to socialize people to the different work habits, patterns of discipline and social demands of different positions in the production process. In particular, higher levels of education tend to socialize people to the work habits appropriate for occupying positions in the authority hierarchy. Among managers, it would be expected that there would be a fairly close association between their position in the authority hierarchy and their level of schooling. This association has been demonstrated by Tannenbaum et al. (1974:110-3). On the basis of the logic of social control elaborated above, we would also expect that there should be a fairly close association between income and the authority hierarchy (see Tannenbaum et al., 1974:106-8). Thus, we would predict that among managers there should be a fairly steep income gradient for educational attainment as well. This is precisely the result we have observed.

In these terms, the education coefficient for workers in Figure 2A can be thought of as the payoff to education for those people with zero authority, while the difference between the worker and manager regressions at each level of education reflects the extra income managers can get by cashing in their education on positions in the managerial hierarchy. If this interpretation of the steeper managerial returns to education is correct, then it would be predicted that, within specific levels of managerial hierarchies, the returns to education should be much closer to those of workers. Unfortunately, in the present study there are no data appropriate for testing this proposition.

The class differences between managers and workers tend to be less marked among white women and black men than among white men. White female managers and workers differ hardly at all in returns to education, and the average gap in their income is very small in both regression equations. Although there is a significant

difference in education slopes between managers and workers among black men, the income gap is quite small.

These results suggest that black male and white female managers look more like workers than do their white male counterparts. One partial explanation for this might be that black male and white female managers are more highly concentrated at the very bottom of managerial hierarchies than are white male managers. It would be expected that they would tend to be line supervisors of various sorts rather than middle managers, and that very few indeed would become top level managers in either private businesses or public bureaucracies. This would tend to depress the overall payoff for becoming a manager and reduce the differences between managers and workers. The present survey provides no data directly relevant to this hypothesis, and we have been unable to find other studies which systematically analyze the position of blacks and women in comparison with white males in authority hierarchies.

Even when education, occupational status, age and job tenure are controlled for, employers have greater income than either managers or workers. This result occurs in spite of the fact that the employers in the sample overwhelmingly are small businessmen rather than large capitalists (see footnote 7). Thus, despite theories of the managerial revolution, the postindustrial society and other perspectives which claim that authority and/or knowledge stratification has superseded property stratification, the results of the present research indicate that the class division between property-holders and non-property-holders is still very real, even when only small property-holders are considered.

The returns to education for small employers are much greater than for workers and managers. This was an unexpected result. Since employers are not paid a direct return to education but, rather, pay themselves an income out of their profits, the income of employers should be linked to education only to the extent that the quantity and profitability of their property was

closely associated with the level of their education. We did not expect this to be the case but, apparently, at least for small businessmen, such a link is present.

In retrospect, this is not so terribly surprising. Among quite small employers, after all, differences in education would correspond to differences between shopkeepers on the one hand and doctors, lawyers and other professionals on the other. Since 78% of the employers in the present sample employ fewer than ten employees, it seems likely that the relationship of education to type of business may account for much of the steepness of the employer education coefficient. Among larger employers, it would be expected that education would make less difference in income.

There is some suggestive evidence in the present survey which supports this interpretation. Eighteen employers in the sample stated that they employed ten or more employees. When the regression equations are run separately for this subgroup, it is found that indeed they do have much flatter education coefficients than employers who employed fewer than ten employees. In the simple regression, small employers received \$4,285 for each increment in education whereas the larger employers received only \$1,221.¹⁷ This coefficient is smaller than the one for managers. The average income gap between large and small employers, as one would expect, was relatively large—\$4,155. Because of the small number of large employers, it was not possible to test formally the significance levels of these comparisons. However, they do suggest that as the actual labor of an employer becomes a smaller proportion of the total labor of a business,

the education of the employer matters less as a determinant of his income. It would be expected that for truly large capitalists (employing 500 or more workers) the returns to education would be even smaller and the adjusted constants correspondingly higher.

The class differences between workers and employers are considerably greater than the differences between men and women or between blacks and whites within the working class. The standardized income gap between workers and employers is generally two to three times as great as the standardized gap between male and female workers of either race, and the gap between black and white workers of the same sex is, at most, a tenth as large as the gap between workers and employers.¹⁸ The differences in education slopes are very small between sexes or races within the working class, but are quite substantial and highly significant between workers and employers. Furthermore, race and sex groups within the working class differ by a maximum of about \$200 in returns for a decile increase in occupational status, whereas employers and workers differ by over \$2,000. In a Marxist perspective, these results are very much what would be expected. *In economic terms*, class exploitation is a theoretically more fundamental division within capitalist society than is either sex or race, and thus the differences between the working class and the capitalist class should be more substantial than the differences between male and female workers or black and white workers.

Within class categories, races and sexes appear to have very similar returns to education. These results must be considered somewhat tentative since the sample size, especially for the race comparisons, is relatively small. Nevertheless, it seems fairly safe to conclude that at least part of the

¹⁷ There was one outlier in the large employer group—an individual who employed more than ten workers but reported an annual income of only \$3,000. This individual also happened to have a low level of education, and leaving him in the regression increased the education slope for large employers considerably (to \$2,100 from \$1,221.) Since it is implausible that an employer of more than ten people would actually have a real income of only \$3,000 a year (presumably in this case the employer had a bad year and lived off his capital), we have excluded this individual from the regression.

¹⁸ Note that we are comparing "standardized income gaps" rather than "average income gaps" in this conclusion. It is impossible to compare average income gaps since they are assessed at a different point for each comparison. The standardized income gaps, on the other hand, are assessed at the same point, mean values of the independent variables for the employer category.

differential returns to education for blacks and whites is a consequence of the class composition of the two races. If this result is confirmed with more extensive data, it would suggest that racial discrimination operates more in sorting people into class positions in the first place than in giving them lower income for given levels of education and skills once they are in a class position.

Within class categories, the income gap between races tends to be much smaller than between sexes. The standardized income gap between white males and white females is nearly five times greater than between black males and white males in both the working class and the managerial category in the simple regression. The difference is even greater in the expanded regressions. Again, racism may be particularly important in allocating people into different class categories in the first place. However, once a person is located in a class position, sex differences in income are considerably larger than race differences.

Obviously, this study is only a first step in the development of a sophisticated use of structural class categories in quantitative research. It would be desirable in the future to use these categories in more elaborate path models of income determination including extensive information on background. It is also crucially important to examine the extent to which various class boundaries are crossed in the course of individual work-lives. While much research has been done on the movement across the blue-collar/white collar occupational "boundary," both within generations and between generations, none we have found systematically has explored the movement across structural class boundaries. The many questions concerning the internal stratification within structural classes also need to be researched. It is our hope that the present work will open up the possibility of bringing Marxist categories into the heart of quantitative research on social inequality as well as making quantitative research seem more relevant to Marxist social scientists. To ignore these theoretical and empirical issues is to ignore a crucial

dimension in the structure of inequality in American society.

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STRUCTURAL AND EXCHANGE COMPONENTS OF VERTICAL MOBILITY *

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This paper focuses on the measurement of structural and exchange components of vertical mobility, with primary emphasis on occupational status mobility. The shortcomings of the study of vertical mobility are discussed. Then, assuming the use of interval scales, absolute, exchange and structural mobility are defined and measured at the individual level. Aggregate measures of each mobility component are also presented which can be computed easily from the means, standard deviations and correlation of the origin and destination distributions. Finally, the aggregate measures are applied to occupational status mobility data for 1962 and 1972-1974. Interperiod comparisons are discussed, as well as racial comparisons and sex comparisons. Educational mobility is briefly compared to status mobility.

Students of social stratification have long been interested in intergenerational occupational mobility because of its relevance to the subject of equality of opportunity or the "openness" of society (Sørensen, 1975a; Coxon and Jones, 1975). For this reason, they have been concerned primarily with vertical mobility, as opposed to horizontal mobility, and with exchange or pure mobility, as opposed to structural mobility. However, it will be argued in this paper that due to methodological problems and a lack of conceptual clarity, efforts to empirically separate vertical mobility into its exchange and structural components have been largely unsuccessful.

This paper represents an attempt to define and develop measures of exchange and structural vertical mobility. Absolute, exchange and structural mobility first will be defined and measured at the individual level. Measures of aggregate population mobility then will be developed from the individual level measures. And finally, these aggregate measures will be applied to recent data for the United States to assess interperiod changes and intergroup differences in mobility. No effort will be made

to explain individual differences in mobility or occupational achievement.

The definitions and measures of vertical mobility proposed in this paper are based upon three premises about the current state of thought and research on mobility. First, occupational mobility tables are inappropriate for the study of vertical mobility. Second, although status attainment models are more suited to the study of vertical mobility than are mobility tables, status attainment models do not directly address the subject of mobility and have, in fact, diverted attention away from improving the measurement of mobility. And third, the conceptualization of mobility has been less than adequate, particularly with respect to the task of measuring vertical mobility and its exchange and structural components. These premises will be elaborated upon further, but first it will be necessary to briefly review the distinction between exchange and structural mobility as well as previous efforts to measure the two.

Intergenerational mobility is most frequently studied through the use of an occupational mobility table consisting of the joint distribution of current occupation and father's occupation for a sample of the work force. The relevance of such a table for assessing the openness of society is complicated by the fact that the current occupational distribution of members of the

* I would like to thank David L. Featherman and Robert M. Hauser for their comments on an earlier draft of this paper. The comments of two anonymous ASR readers were also helpful.

work force typically differs from that of their fathers. The difference between the two distributions is due, in part, to the changing occupational structure of society, but also to such factors as differential fertility, mortality and marriage rates (Duncan, 1966). Whatever the relative importance of these factors, the difference between the two distributions "forces" a certain amount of mobility and certain patterns of mobility, which are not indicative of the openness of society. This mobility often has been called structural mobility (Boudon, 1973; Matras, 1975).

Although the term "structural mobility" will be adopted in this paper, the use of this term is not meant to imply that the "forced" mobility can be traced to a change in the occupational structure over a specific period of time. First, as Duncan (1966) has pointed out, the fathers' occupational distribution does not represent a specific point in time. Second, factors other than the changing occupational structure, such as the differential fertility of the fathers, cause the origin and destination distributions to differ. Thus, the term "structural mobility" will be used to refer to the mobility resulting from all factors which cause the current distribution to differ from that of the fathers. Whatever the sources of such mobility, it is not due to equality of opportunity or the openness of society.

The general goal of mobility research has been to control for structural mobility in order to measure pure mobility, or exchange mobility, that is, mobility which is not due to differences between the origin and destination distributions. Critiques of methods designed to separate exchange mobility from structural mobility can be found in Duncan (1966), Blau and Duncan (1967) and Hazelrigg (1974b) and will not be reviewed here. Much of the effort at measuring exchange mobility has been inadequate or only partially successful. Recently, however, two techniques have been adopted which appear to control successfully for structural mobility. For example, a technique for the proportionate adjustment of contingency tables (Deming, 1943) has been used by Duncan (1966), Tyree and Treas (1974) and Hazelrigg (1974a), and Goodman's (1972) hier-

archical models for the analysis of contingency tables have been used by Hauser et al. (1975a).

The first premise of this paper is that although valid techniques are now available for comparing exchange mobility in different mobility tables, occupational mobility tables are inadequate for measuring or comparing vertical mobility, in either its exchange or structural components. The occupational categories, usually major census occupational groups, used to construct mobility tables are, strictly speaking, nominal categories and thus cannot be used to reliably assess vertical mobility. This premise is not meant to imply that those who use occupational mobility tables are interested only in vertical mobility. There may be sound reasons for studying occupational mobility per se. Carlsson (1958) has argued for the validity and utility of using aggregate occupational categories. Nevertheless, the importance of studying vertical mobility is evident from the fact that although persons who analyze mobility tables usually acknowledge the nominal nature of their data, they almost always include a discussion of upward and downward mobility (e.g., Hauser et al., 1975b). Typically, the major census detailed categories (or other categories) are ranked according to their average scores on some vertical dimension, such as Duncan's (1961) SEI. Duncan and Hodge (1963) reported that the major occupational categories accounted for about three-quarters of the variance in occupational SES among males in the labor force. This still leaves sizeable error in the measurement of an individual worker's status, however, and even in the rank ordering of individuals with respect to SES.

In addition to the measurement error that results from attempting to rank individuals according to the major occupational groups, the typical method of measuring vertical mobility only compounds the error. The typical method of computing upward mobility, for instance, involves counting all the individuals who move to a higher ranked category, with a move across one boundary counting the same as a move across two or more boundaries. Thus, this method of measuring

vertical mobility does not even use all of the ordinal information present in the data. Even if the number of category boundaries crossed were used as a measure of the vertical distance moved, there would still be considerable error involved since the average SEI distance between adjacent major occupational groups ranges anywhere from one to nineteen SEI points (Hauser et al., 1975b). In short, the use of occupational mobility tables to measure vertical mobility is faced with all of the limitations of ordinal scales, and the occupational categories only provide an imperfect ordering of individuals at that, since the categories are, strictly speaking, only nominal categories. Thus, an alternative to mobility tables is needed for measuring vertical mobility.

The development of a socioeconomic index (Duncan, 1961) and a prestige scale (Siegel, 1970) for the census detailed classification of occupations provided potentially fruitful means for the measurement of vertical occupational mobility. As Duncan and Hodge (1963) have pointed out, there is little question that these scales are more suitable for measuring vertical mobility than the census major occupational groups used in mobility tables. Although these scales may only be quasi-interval (particularly the prestige scale), they will be treated as interval scales when using the measures of vertical mobility to be presented in this paper. (The measures are just as appropriate for measuring education and income mobility, both of which are undoubtedly interval scales.)

In light of their potential fruitfulness, it is surprising how little these scales have been used for the study of vertical mobility.¹ Following the construction of the socioeconomic index (SEI), a reformulation of the basic problem to be studied occurred in the work of Duncan and his

colleagues. In Duncan and Hodge (1963), Duncan (1966) and, particularly, Blau and Duncan (1967), the orientation shifted from occupational mobility to one of explaining individual differences in occupational achievement, as measured by the SEI. Instead of using father's SEI score as a reference point from which mobility could be measured, father's SEI became one of several independent variables used to explain the variance in son's SEI. These multivariate models have come to be called models of the status attainment process.

The second premise of this paper is that the development of status attainment models, although extremely valuable in their own right, has diverted attention away from a legitimate concern with vertical mobility as a system characteristic. Instead of utilizing the SEI, or a prestige scale, to improve the measurement of the amount, rate and patterns of vertical mobility, as well as its exchange and structural components, these scales have been used almost exclusively in status attainment research. For those still concerned with the study of mobility (and the interest remains high) the conventional mobility table has continued to be the main vehicle, despite its limitations with respect to vertical mobility.

It is not claimed that status attainment models are irrelevant to the subject of mobility. The simple model showing son's status regressed on father's status implicitly contains much information on vertical mobility, some of which is occasionally noted. The difference between the father's mean and the son's mean indicates the average distance moved. Also, the standardized regression coefficient, the Pearson product-moment correlation, provides an index of mobility, or immobility, although no one has been very precise about what kind of mobility is being indexed. This is particularly problematic in light of the fact that a perfect correlation would not necessarily mean that there is no mobility. As Duncan (1966) has shown, the regression model also can be implicitly used to predict which sons will be upwardly mobile and which sons will be downwardly mobile. But, as Duncan (1966) has also stated, the regression model, as such, makes no direct statements about mobility. Although a

¹ Prestige scales occasionally have been used to construct mobility tables. For instance, Svalastoga (1959) used a Danish scale to construct mobility tables based on five aggregate prestige categories. Because of the necessity to aggregate data into a relatively small number of categories when constructing mobility tables, the measurement of social distance in such tables is not based on the full detail of the prestige scales.

great deal of information about mobility can be garnered indirectly from least-squares regression statistics, as we shall see, students of status attainment have made very little use of this information. In particular, they have paid no attention to the distinction between exchange mobility and structural mobility, a subject of great importance to those interested in mobility as a system characteristic.

In short, it can be concluded that the development of occupational status scales has had little or no effect on improving our measurement of vertical mobility, particularly with respect to exchange and structural mobility, because these scales have been used almost exclusively for status attainment research. Thus, it is time to use these scales to measure vertical mobility directly rather than to continue to make indirect inferences about mobility on the basis of their usage in regression models.

The third premise of this paper is that the conceptualization of social mobility has been rather unsophisticated and lacking in sociological insight. This seems to be true particularly with respect to vertical mobility. Social mobility is usually simply defined as a change in social position (Sørensen, 1975b). In mobility tables, social mobility means a change in occupation, as defined by the occupational categories chosen for the table. When we turn to vertical mobility, this definition of social mobility creates some problems.

If the occupational categories have been ranked according to some dimension of stratification and a person moves to a higher-ranked category, the move is defined as upward mobility. The trouble with this definition of vertical mobility is that it ignores the "social" aspect of social mobility. For instance, if the mobility table is constructed from major occupational groups, an intergenerational move from the managerial category to the professional category would be counted as one unit of upward mobility. This definition of upward mobility is based upon an absolute change of position and ignores the fact that in industrializing societies there is typically an expansion in the number of professional positions. Thus, it ignores the fact that

relative to other persons, a professional position does not rank as high in the destination distribution as in the origin distribution. The consequence of ignoring this fact may be seen more clearly if we consider a person who is in the professional category in both the origin and destination distributions. By conventional definitions, that is, in an absolute sense, such a person would be defined as stable with respect to vertical mobility. However, if the person's position relative to others were taken into account, say by looking at his percentile rank or some other standardized score, the person should be classified as downwardly mobile.²

Thus, it is seen that the conventional definition of vertical mobility is one of absolute mobility which ignores relative mobility. If we are to take the concept of social mobility seriously when discussing vertical mobility, it seems at least as important to study relative mobility as to study absolute mobility, if not more so. It could be argued that vertical "social" mobility should be defined as relative mobility, but this will not be done here. As will be seen, the conventional definition of social mobility will be called absolute mobility and, for reasons given below, relative mobility will be called exchange mobility.

To summarize the premises of this paper, it is believed that the study of vertical mobility has been retarded because of the continued reliance on mobility tables, because of the failure to explicitly exploit the detailed occupational status scales and because of the lack of a conception of relative vertical mobility. The remainder of this paper will attempt to define and measure absolute, exchange and structural

² To point out the fact that the percentile rank of *individuals* in professional occupations is not as high as the percentile rank of fathers in the same occupations is not necessarily to argue that professional occupations now rank lower, relative to other *occupations*, on income, education or "prestige." The available evidence indicates that the rankings or ratings of occupations have been rather stable over time (Hodge et al., 1966). However, the apparent immobility of occupations clearly does not preclude changes in the rankings of individuals who hold these occupations. Such changes in the relative statuses of individuals are eminently worth investigating.

vertical mobility with particular reference to occupational status mobility.

Definitions and Measures

In the definitions and measures of intergenerational vertical mobility to be presented, it will be convenient to refer to X as a score in the origin status distribution and Y as a score in the destination status distribution. By conventional definitions of social mobility, a person's vertical status mobility would equal $Y - X$. $Y - X$ gives both the absolute distance and the direction of the status move and will be called absolute mobility (not to be confused with the term "absolute value," which gives the numerical value of a real number irrespective of its sign). However, $Y - X$ reveals nothing about the person's mobility relative to other individuals in the status distributions. Due to the typical intergenerational shift upward in the status distributions, as indicated by a positive value of $\bar{Y} - \bar{X}$, a person with zero absolute mobility ($Y - X = 0$) actually would be downwardly mobile relative to others and a positive absolute mobility score ($Y - X > 0$) may or may not indicate upward mobility relative to others. Thus, in order to assess vertical mobility relative to others, a measure of a person's relative position in the origin and destination distributions is needed. Percentile ranks could be used but percentile ranks are only ordinal measures of status and, thus, a change in percentile rank would not indicate the relative status distance traveled.

In order to gain an interval measure of a person's relative position in a status distribution, a person's raw status score will be standardized with respect to the mean and standard deviation of the distribution, the conventional standard score used in regression analysis. Thus, a person's relative position in the X and Y distributions will be measured by $Z_x = (X - \bar{X})/s_x$ and $Z_y = (Y - \bar{Y})/s_y$, respectively, and his relative mobility will be indicated by $Z_y - Z_x$. The sign of $Z_y - Z_x$ will indicate the relative mobility direction, and the magnitude of

the difference will be proportional to the relative mobility distance.

A significant property of $Z_y - Z_x$ as an indicator of relative mobility is the fact that $\Sigma(Z_y - Z_x) = 0$. This zero-sum characteristic is usually associated with the conception of exchange mobility or circulation mobility, as it is sometimes called. It is assumed to result from the fact that persons are shifted upward and downward to adjust for differences in talent, skills and interests. Hazelrigg (1974b) has shown that when a mobility table is proportionately adjusted to a model of constant occupational structure, and therefore freed from structural influences on mobility, the number of person-steps upward will be equal to the number of person-steps downward. Thus, such an adjusted table has the zero-sum characteristic of circulation mobility. When using interval scales to measure mobility, a valid measure of exchange mobility should have the characteristics that the total upward mobility distance should equal the total downward mobility distance. $Z_y - Z_x$ has this zero-sum characteristic and, therefore, would seem to be a valid indicator of exchange mobility in this respect.

The other defining characteristic of exchange mobility usually given is that it is mobility that does not result from differences between the origin and destination distributions; it is distinguished from structural mobility. Since the Z_x and Z_y distributions are equal with respect to their means and standard deviations (which are equal to zero and one, respectively), $Z_y - Z_x$ can be treated as freed from structural influences, to the extent that differences between the origin and destination distributions can be reliably described by differences in their means and standard deviations.⁸ Thus $Z_y - Z_x$ appears to be a valid

⁸ Since standard scores are linear transformations of the raw scores, the standard score distributions will have the same shapes as the raw score distributions. Thus, any difference in the shapes of the origin and destination distributions will be a structural influence on mobility that is not eliminated by using standard scores. If the shapes of the distributions differ greatly, the use of standard scores to measure exchange

indicator of exchange mobility in respect to the second defining characteristic of exchange mobility.

Because of its zero-sum nature and its relative freedom from structural influences, $Z_y - Z_x$ will be treated as an indicator of exchange mobility. As a result of our interest in measuring relative mobility, we have arrived at a valid indicator of exchange mobility which truly reflects the "social" aspects of mobility.

Although $Z_y - Z_x$ is an indicator of exchange mobility, it does not provide a measure of exchange mobility distance in the metric of the status scale being used. To acquire such a measure, we will compute a person's score on Y who has experienced no exchange mobility. Since $Z_y = Z_x$ when there is exchange immobility, the destination status of a person with zero exchange mobility would be given by

$$Y' = s_y Z_x + \bar{Y}. \quad (1)$$

The expression for Y' given in (1) provides a very important reference point in our analysis which will be called the exchange immobility score. The deviation of a person's score from Y' provides the measure of exchange mobility distance, which can be written as

$$Y - Y' = s_y (Z_y - Z_x). \quad (2)$$

Since $\Sigma(Z_y - Z_x) = 0$, $\Sigma(Y - Y')$ will also equal zero and, thus, our measure of exchange mobility distance has the zero-sum characteristic. The expression for $Y - Y'$ also indicates that the exchange mobility of a person is, in part, a function of the amount of inequality in the population, as indicated by s_y . And finally, it is important to note that due to regression toward the mean, $Z_y - Z_x$ will tend to be negative for persons above the mean on X and positive for those below the mean on X. Thus, persons from above average origins will tend

to have downward exchange mobility and persons from below average origins will tend to have upward exchange mobility. The strength of this tendency will vary inversely with the strength of the correlation between X and Y.

The exchange immobility function also provides a reference point from which structural mobility distance can be computed. Since $Y' - X$ represents the distance a person would move who experiences no exchange mobility, this move would result from intergenerational structural change. Thus $Y' - X$ provides a measure of structural mobility which can be written as

$$Y' - X = (s_y - s_x)Z_x + (\bar{Y} - \bar{X}). \quad (3)$$

Examination of the expression for $Y' - X$ reveals several interesting facts about structural mobility. The amount of structural mobility a person experiences is a function of the changes in two basic structural parameters (the mean and standard deviation) and the person's relative position in the origin distribution. The mean structural mobility equals $\bar{Y} - \bar{X}$. Thus, when status attainment researchers note that the average mobility in their sample is equal to $\bar{Y} - \bar{X}$, they are taking note of the average mobility created by structural change in the status distributions. The standard deviation of structural mobility equals $s_y - s_x$, which is an indicator of the intergenerational change in inequality. Since the only variable in the expression for $Y' - X$ is Z_x , structural mobility will be perfectly correlated with X, with $r = +1$ when $s_y > s_x$ and $r = -1$ when $s_y < s_x$. Assuming that $\bar{Y} - \bar{X}$ is positive (which is typically the case), when inequality is increasing ($s_y > s_x$) persons from above average backgrounds will experience greater upward structural mobility than persons from below average backgrounds. The opposite will be the case when inequality is decreasing. Thus, an intergenerational increase in inequality favors persons from high status origins while a decrease in inequality favors those from low status backgrounds.

Perhaps the relationship between structural mobility and the origin distribution

mobility might lead to substantial errors. Although preliminary investigation suggests that the error may be relatively small for typical mobility distributions, we are as yet unable to present a satisfactory answer to this potential problem. Clearly, further investigation of this question is needed.

can be further clarified by looking at a simplified example. Imagine the following origin and destination distributions where

$\bar{Y} = \bar{X} = 50$, $s_x = 14.1$, $s_y = 21.2$, and $r = +1$.

X	Y	Y - X
70	80	+10
60	65	+5
50	50	0
40	35	-5
30	20	-10

Since the origin and destination distributions are perfectly correlated, all of the mobility is due to structural change. Since the means of both distributions are equal, the structural mobility results entirely from the increase in the standard deviations. This increase in inequality causes those who were below average on X to have downward structural mobility and those who were above average on X to have upward structural mobility, resulting in a perfect positive correlation between structural mobility and X. It should be noted that this example shows an exaggerated change in the standard deviation, if we are thinking in terms of occupational status.

By adding an intergenerational increase in the mean, the hypothetical example becomes more realistic. Suppose the X distribution remains the same but 15 is added to each Y making $\bar{Y} = 65$, but leaving s_y and r_{xy} unchanged.

X	Y	Y - X
70	95	+25
60	80	+20
50	65	+15
40	50	+10
30	35	+5

Now the intergenerational increase in the mean is sufficiently large to make each person have upward structural mobility. However, although the average structural mobility is $\bar{Y} - \bar{X} = 15$, the increase in the standard deviation causes those with higher scores on X to have greater structural mobility.

It is also instructive to consider the rela-

tionship between exchange and structural mobility. The covariance of exchange and structural mobility, which is given without proof, can be written as

$$s_{(y-y')(y'-x)} = s_y(s_x - s_y)(1 - r_{xy}). \quad (4)$$

The term $s_x - s_y$ indicates that the covariance between exchange and structural mobility will be positive when inequality is decreasing and negative when inequality is increasing (assuming a positive correlation). Since persons from below average origins tend to have upward exchange mobility and since they will have greater structural mobility than persons from above average origins when inequality is decreasing, structural and exchange mobility will covary positively when $s_y < s_x$ and thus complement one another. However, in the more typical case where inequality of occupational status is increasing, the downward exchange mobility tendencies of persons from above average backgrounds will be ameliorated somewhat by their greater than average upward structural mobility. Whenever inequality is changing, exchange and structural mobility will have some covariance.

Given the measures of exchange and structural mobility which have been presented, it is easily recognized that absolute mobility is simply the sum of exchange mobility and structural mobility

$$(Y - Y') + (Y' - X) = Y - X. \quad (5)$$

The manner in which exchange and structural mobility combine to give absolute mobility is shown in Figure 1. The data displayed in Figure 1 are from the 1962 OCG subsample of white males (Duncan et al., 1972). The solid line $Y = X$ represents the absolute immobility line, the dashed line $Y' = s_y Z_x + \bar{Y}$ is the exchange immobility line and the dotted line $\hat{Y} = a + bX$ is the least-squares prediction of Y. For any given score on X, the vertical distance between the Y' line and the \hat{Y} lines indicates the expected exchange mobility, the distance between the Y' line and $Y = X$ equals the structural mobility and the distance between the \hat{Y} line and $Y = X$ equals the ex-

pected absolute mobility. The figure shows upward structural mobility for all values of X , with the amount of structural mobility increasing as the level of X increases. The least-squares line and the exchange immobility line intersect at approximately $X = \bar{X} = 28$. Below the mean of X , it can be seen that both exchange mobility and absolute mobility tend to be upward. Next, observe the area above the mean of X but below $X = 50$, the approximate value of X at which the line $Y = X$ and the \hat{Y} line intersect. For this range of values of X (from 28 to 50), exchange mobility tends to be downward but absolute mobility is still upward. Therefore, although these men

from above average backgrounds tend to be losing ground relative to others, they are still experiencing absolute upward mobility due to their structural mobility. And, finally, those men with background scores of 50 or higher (at least one standard deviation above the mean) are experiencing both downward exchange mobility and downward absolute mobility, despite the fact that they have the greatest structural mobility of the three categories of men discussed. Their status (\hat{Y}) still tends to be higher than the others, of course. The negative covariance between structural and exchange mobility is clearly displayed in Figure 1. The vertical distance between Y' and $Y = X$ (structural mobility) becomes

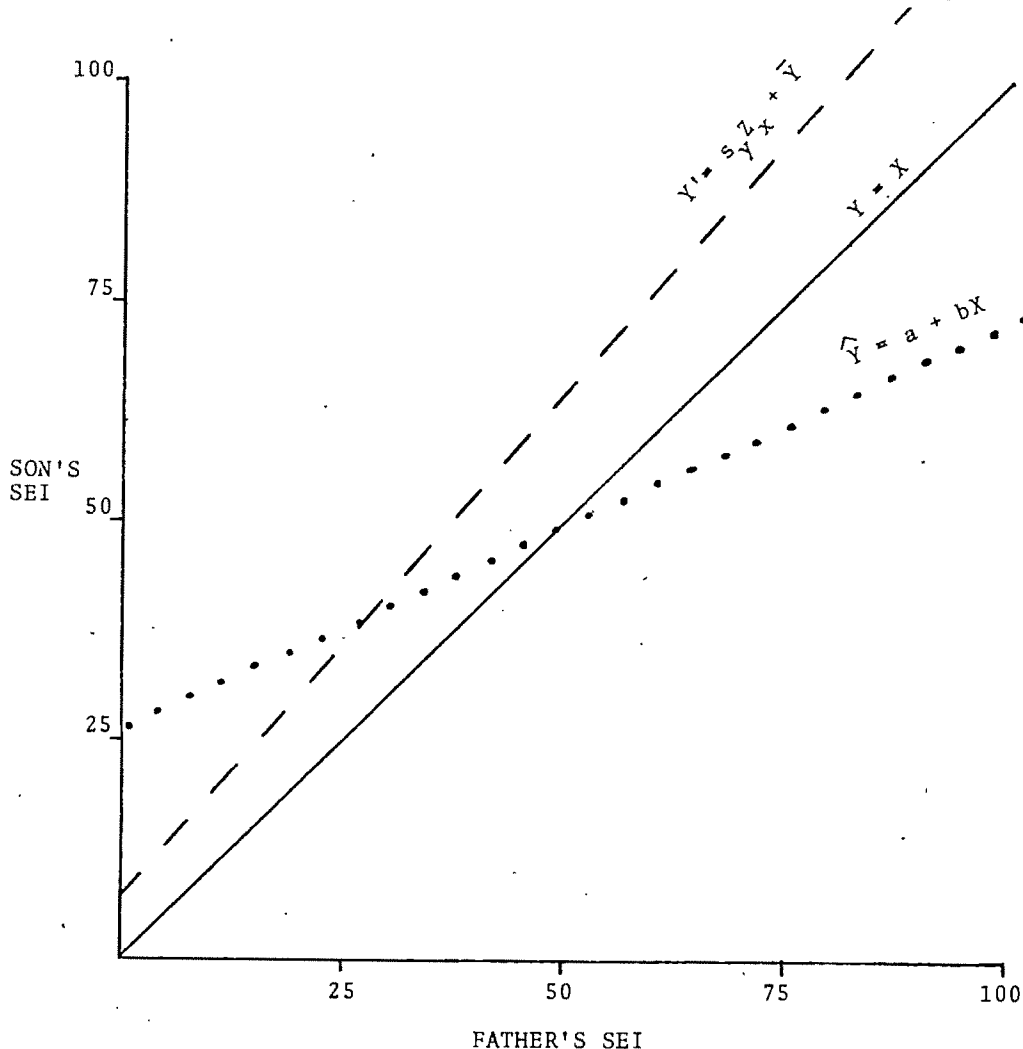


Figure 1. Relationships between Absolute, Exchange and Structural Mobility for White Males, 1962

increasingly positive as the vertical distance between Y' and \hat{Y} (expected exchange mobility) changes from positive to increasingly negative.

Definitions and measures of exchange, structural and absolute mobility have been given at the individual level, and their characteristics and relationships have been discussed. Absolute mobility is seen to be a function of exchange and structural mobility. Structural mobility results from changes in the demand for labor as well as from differential fertility, mortality and marriage rates. No attempt has been made to explain individual differences in exchange mobility, but exchange mobility is assumed to result from individual differences in talents, skills and desires. Since the goal of this paper is not to explain vertical mobility but to define and measure it in its several components, the next step is to develop measures which describe the aggregate mobility of the entire population or system.

The mean is not a very useful statistic for describing the average amount of absolute and exchange mobility since the downward mobility moves tend to cancel out the upward moves in $\Sigma(Y - X)$ and do so exactly in $\Sigma(Y - Y')$. We could compute the mean of the absolute values $|Y - X|$ and $|Y - Y'|$, but absolute values are not mathematically tractable to work with. Our choice of a single statistic to describe the aggregate amount of each type of mobility is to compute the square root of the second moment of each mobility distribution. As we shall see, this measure can be meaningfully expressed and calculated from readily available statistics such as the means and standard deviations of the origin and destination distributions and the product moment correlation between the distributions.

Although our procedure will be to take the square root of the second moment in order to convert our measure of the amount of mobility back into the original metric, for the sake of simplicity the square root sign will be omitted in the following presentation of formulas. The second mo-

ment of the absolute mobility distribution is given by

$$\Sigma(Y - X)^2/N = (\bar{Y} - \bar{X})^2 + s_y^2 + s_x^2 - 2s_y s_x r. \quad (6)$$

Thus, our measure of absolute mobility can be expressed in terms of the structural characteristics of the origin and destination distributions, as measured by the means and standard deviations, and the correlation between the distributions (which will be defined as an index of exchange immobility). However, since the effects of the statistics in equation (8) can be seen more clearly in the formulas to be presented for the aggregate exchange and structure mobility, as well as in equation (4) for the covariance between exchange and structural mobility, formula (6) will not be discussed further.

The second moment of the exchange mobility distribution can be written as

$$\Sigma(Y - Y')^2/N = 2s_y^2(1 - r). \quad (7)$$

Thus, it can be seen that the amount of exchange mobility in a population is a function of the interaction between the variance of the destination distribution and the correlation between the origin and destination distributions. The greater the inequality of status, the greater will be exchange mobility, and the higher the correlation, the less will be exchange mobility. As the reader has probably suspected by now, $1 - r$ can be used for comparative purposes as an index of exchange mobility. This was recognized by Rogoff (1966:213) who pointed out that the coefficient of correlation standardizes for differences between the means and standard deviations of the fathers' and sons' occupational status, and thus controls for structural changes. Although $1 - r$ can range mathematically from 0 to 2, practically speaking we would never expect to find a negative correlation and, thus, the index can be interpreted on a scale from 0 to 1. The index will equal one when there is "perfect" mobility, that is, statistical independence between X and Y as indicated by $r = 0$.

The second moment of the structural

mobility distribution can be expressed rather simply as

$$\Sigma(Y' - X)^2 / N = (\bar{Y} - \bar{X})^2 + (s_y - s_x)^2. \quad (8)$$

This expression indicates that the amount of structural mobility in a population is a function of structural changes in the average level of status and changes in the inequality of status.

Although absolute mobility equals the sum of exchange mobility and structural mobility at the individual level, such is not the case for our measures at the societal level, unless the covariance between exchange and structural mobility is zero. It can be shown that the second moment of absolute mobility is equal to the sum of the second moments of exchange and structural mobility plus twice their covariance:

$$\begin{aligned} \frac{\Sigma(Y - X)^2}{N} &= \frac{\Sigma(Y - Y')^2}{N} \\ &+ \frac{\Sigma(Y' - X)^2}{N} + 2s_{(Y-Y')(Y'-X)}. \end{aligned}$$

The expression for the covariance was given in equation (4), which shows that when inequality is increasing, the covariance of exchange and structural mobility will be negative. Thus, it is possible for the total amount of absolute mobility to be less than the sum of exchange and structural mobility and, in some cases, as will be seen, the amount of absolute mobility may be less than the amount of exchange mobility.

This concludes the presentation of definitions and measures of absolute, exchange, and structural mobility. Hopefully, we have shown that valid population measures for the components of mobility can be computed easily from three basic types of statistics readily available from regression analysis outputs (the means, the standard deviations and the correlation). We have given these statistics meaningful interpretations with respect to mobility in its several forms.

It might be objected that since these statistics are present in regression models of status attainment, they add nothing new to our knowledge in the form introduced here.

For instance, the least-squares prediction of Y can be rewritten in terms of these basic statistics as follows:

$$\hat{Y} = \bar{Y} + \frac{rs_y}{s_x} (X - \bar{X}).$$

Thus, it can be seen how a person's expected absolute position on Y is determined by the structural and exchange aspects of the X and Y distributions. However, a comparison of two populations with respect to their regression equations in this form, or in any form for that matter, could not provide answers to such basic questions as "which society has the most absolute mobility, the most exchange mobility, and the most structural mobility?" It can only be reiterated that status attainment analysis is not the same as mobility analysis. They are closely related, however. It is believed that the strength of this paper is to show how the basic statistics from regression analysis can be brought directly to bear on problems still being struggled with in the analysis of mobility tables, particularly problems in the analysis of vertical mobility.

Occupational Status Mobility

The measures of absolute, exchange and structural mobility will now be used to examine occupational status mobility in the United States. Occupational status is measured by Duncan's (1961) Socioeconomic Index (SEI). The mobility of black and white males will be analyzed for the years 1962 and 1972-1974. The mobility of black and white females will be analyzed only for 1972-1974. The data are for persons 25 to 64 years of age in the experienced civilian labor force. The 1962 data are from the Occupational Changes in a Generation (OCG) survey as reported in Duncan et al. (1972: Tables A.1 and A.3). The data for 1972-1974 are from the combined General Social Surveys of 1972, 1973 and 1974 conducted by the National Opinion Research Center (NORC). The components of mobility are given in Table 1, along with the means, standard devia-

Table 1. Components of Occupational Status Mobility of the Experienced Civilian Labor Force, 25 to 64 Years of Age, by Sex, Race and Year

	1962		1972-1974		1972-1974	
	Males		Males		Females	
	Whites	Blacks	Whites	Blacks	Whites	Blacks
\bar{Y}	39.6	17.7	44.3	26.4	44.8	26.2
\bar{X}	28.0	16.1	32.7	19.9	35.6	19.3
s_y	24.5	15.3	24.2	19.8	21.7	21.7
s_x	21.3	12.9	22.9	16.3	22.9	16.4
r	.40	.15	.36	.14	.34	.27
N	14,347	1,394	1,022	130	575	81
Mobility *						
Absolute	27.7	18.5	29.1	24.7	27.2	24.4
Exchange	26.7	19.9	27.4	26.0	24.9	26.2
Structural	12.0	2.9	11.7	7.4	9.3	8.7
Covariance	-46.7	-31.2	-20.1	-59.6	17.2	-84.0

* Absolute, exchange and structural mobility equals the square root of the second moment of the respective mobility distributions, as given in equations (6), (7) and (8).

tions and correlations from which the mobility components were computed.⁴

⁴ Since mobility trends between 1962 and 1972-1974 will be discussed, the comparability of the 1962 Current Population Survey (CPS) data and the 1972-1974 NORC data are of some concern. Differences in sampling methods, item wording and method of enumeration could make comparisons unreliable. As a check on the comparability of the CPS and NORC data, we will compare the means and standard deviations of father's and son's SEI with those from the 1973 CPS replicate of the OCG study as reported in Featherman and Hauser (1975). A difference between the CPS and NORC methods of coding "father's" occupation can be expected to cause some discrepancies between these statistics. The CPS asked for mother's or other female head's occupation when the respondent was not living with his father or father substitute at age 16, whereas NORC did not elicit the occupation of respondent's mother or other female head under this circumstance. Thus, the NORC data on "father's" occupation does not include female head's occupation.

Blau and Duncan (1967:33) indicate that in the 1962 CPS data, "father's" occupation pertains to a male head in 93 percent of the cases and to a female head in seven percent of the cases. Thus, we would estimate that the NORC data does not cover about seven percent of the population covered by CPS. Actually, 11.5 percent of the NORC males in the ECLF came from female headed families, but it would be expected that not all of these female heads had jobs. Knowing the direction of the NORC undercoverage allows us to predict the direction of NORC-CPS discrepancies. Since Duncan et al. (1972) report that the average SEI of males,

both black and white, from female headed families is several points lower than that of males from intact families, we would expect the black and white means in the NORC data to be somewhat higher than in the 1973 CPS data. (In order to insure that the means and standard deviations in Table 1 are based on the same number of cases as the correlations, males from female headed families were excluded from the calculations for current SEI.) Since it is reasonable to assume that the average SEI of female heads should also be lower than the average SEI of male heads, we would also expect the mean SEI for "father's" occupation to be higher in the NORC data than in the CPS data, particularly for blacks who are more likely to come from female headed families (22 percent of the blacks, versus 10 percent of the whites, are from female headed families).

Both of these predictions are borne out by the data. The mean SEI's for the CPS and NORC white males are 42.6 and 44.3, respectively, and 25.8 and 26.4 for the black males. The means for father's SEI in the CPS and NORC surveys are 30.2 and 32.7 for white males, and 16.0 and 19.9 for the black males. Note that the CPS-NORC discrepancy is greatest for the black "father's" since there is a greater proportion of blacks from female headed families. Since $\bar{Y} - \bar{X}$ contributes to structural mobility, the NORC data appear to underestimate black structural mobility. The NORC "error" with respect to $\bar{Y} - \bar{X}$ for whites is quite small, however. The NORC standard deviations for respondent's SEI are about one point lower than the CPS standard deviations, for both blacks and whites (24.2 versus 25.2 for whites and 19.8 versus 20.4 for blacks). With respect to "father's" SEI, the NORC and CPS standard deviations are about equal for whites (22.9 and 22.6, respectively), while the NORC standard deviation for blacks is

There are several striking characteristics of the mobility data which should be noted before turning to a comparison of the groups and years. In both 1962 and 1972-1974, each group experienced an average intergenerational increase in status, as indicated by $\bar{Y} - \bar{X}$. This upward shift in status is greatest for white males. An intergenerational increase in status inequality, as indicated by $s_y - s_x$, also occurred for all groups except white females. The increase in inequality results in a negative covariance between structural and exchange mobility. These intergenerational structural changes caused a substantial amount of structural mobility, equaling about half a standard deviation of the status distribution of white males. Nevertheless, the amount of exchange mobility is consistently more than twice as great as the amount of structural mobility. The index of exchange mobility ($1 - r$) is high for all groups, equaling at least .6 in each case. Thus, it is clear that exchange mobility contributes much more to the amount of absolute mobility than does the not inconsequential amount of structural mobility.

Turning to the mobility of specific

2.5 points higher than the CPS standard deviation (16.3 versus 13.7, respectively). Since $s_y - s_x$ contributes to structural mobility, the NORC data again would appear to underestimate the structural mobility of blacks. Featherman and Hauser (1975) do not report the correlation between father's and son's SEI, so the NORC correlation cannot be checked.

In general, the NORC-CPS discrepancies appear small. Since they are small and in the direction predicted by the omission of "father's" SEI for female heads in the NORC surveys, it appears that valid comparisons between the 1972-1974 NORC surveys and the 1962 CPS survey may be made, when proper caution is exercised with respect to the known discrepancies. The most serious discrepancies are for the mean and standard deviation of the black fathers' SEI. These discrepancies appear to lead to a significant underestimate of black structural mobility. However, despite this discrepancy and other smaller ones, the major conclusions concerning the comparisons and trends of black and white mobility will not be affected. In addition, much of the analysis in this paper does not involve trend analysis, and thus the omission of "father's" occupation for female heads does not invalidate the analysis, as long as this omission is kept in mind.

groups, white males, who are a majority of the labor force, will be examined first. White males have a greater amount of exchange mobility, structural mobility and absolute mobility than either of the other groups. When the mobility of white males in 1972-1974 is compared with that in 1962, however, there does not appear to be any significant change. The ever so slight reduction in structural mobility and increase in exchange mobility are too small to be reliable or of interest. The slight increase in exchange mobility resulted from the smaller correlation in 1972-1974, but the interperiod change in the correlation is significant at only the .15 level. The change in absolute mobility is somewhat greater than the changes in exchange and structural mobility. It is interesting to note that the increase in absolute mobility resulted primarily from a decrease in the covariance between exchange and structural mobility, rather than from a change in either of the latter. The decrease in the covariance occurred because the increase in intergenerational inequality ($s_y - s_x$) was smaller in 1972-1974 than in 1962. The smaller increase in inequality meant that men from above average backgrounds did not have as great a structural mobility advantage over men from below average backgrounds in 1972-1974 as they did in 1962. Thus, the negative covariance between exchange and structural mobility decreased and, consequently, the amount of absolute mobility increased. The increase in absolute mobility is not very great, however, and the mobility of white males in 1972-1974 is more striking for its similarity to that in 1962 than for its differences.

If there was little or no change in white mobility, the interperiod changes in black mobility were rather dramatic. The absolute mobility of black males increased by 34 percent, exchange mobility increased by 31 percent and structural mobility increased by fully 155 percent. The sharp increase in structural mobility resulted primarily from the larger intergenerational difference in average status ($\bar{Y} - \bar{X}$) in 1972-1974, although the slight interperiod increase in $s_y - s_x$ (the intergenerational increase in inequality) also contributed to

the increase in structural mobility. The average status of the black sons in 1972-1974 was 7.5 points higher than that of their fathers, whereas the difference between fathers and sons was only 1.6 points in 1962.

Whereas the increase in structural mobility resulted primarily from the increase in $\bar{Y} - \bar{X}$, the increase in exchange mobility resulted solely from the interperiod increase in status inequality (s_y). The index of exchange mobility ($1 - r$) of black males did not change between 1962 and 1972-1974, but s_y increased by 29 percent and, thus, an increase in exchange mobility occurred. Since both exchange mobility and structural mobility increased, both contributed to the increase in the absolute mobility of blacks. However, although structural mobility increased by a greater percentage than did exchange mobility, exchange mobility increased by a greater amount and contributed somewhat more to the increase in absolute mobility than did structural mobility. Since the increase in exchange mobility was due to the increase in s_y indicating that black males were receiving a wider range of positions in the status hierarchy in 1972-1974 than in 1962, the single most important contributor to the increase in absolute mobility was the increase in inequality among black workers.

Having examined the change, or lack of change, in the mobility of black and white males separately, the mobility of blacks and whites will now be compared. Three generalizations seem clear. First, blacks have less mobility than whites. Second, the makeup or nature of black mobility is different. Third, black mobility is becoming more similar to white mobility.

In order to facilitate racial comparisons, Table 2 gives the ratios of black status to white status and black mobility to white mobility. In both 1962 and 1972-1974, black males had lower and less varied status than whites. This is a well-known fact. Blacks also came from lower and less varied status backgrounds in both years. Although black males showed significant gains relative to whites over the 10-12 year period, the racial gap in average status re-

Table 2. Ratio of Black Status to White Status and Black Mobility to White Mobility for Males in the Labor Force, 25-64 Years of Age, in 1962 and 1972-1974

	1962	1972-1974
Status		
\bar{Y}	.45	.60
\bar{X}	.58	.61
s_y	.62	.82
s_x	.61	.71
Mobility		
Absolute	.67	.85
Exchange	.75	.95
Structural	.24	.63

mains great. Featherman and Hauser (1975) present a more detailed analysis of the changes in racial status attainment.

Although the racial differences in mobility are not as great as the differences in status, in each year the exchange mobility, structural mobility and absolute mobility of blacks is less than that of whites. Black structural mobility is smallest relative to whites while exchange mobility is greatest. Also, just as with status attainment, the amount of black mobility more nearly approximated that of whites in 1972-1974 than in 1962. This is particularly true for the amount of black exchange mobility, which nearly reached parity with white exchange mobility. However, although the racial difference in the amount of exchange mobility is slight in 1972-1974, the makeup of this mobility is different for blacks than for whites. The extremely high index of exchange mobility of black males contributes more to their exchange mobility than is the case for whites. Conversely, the relatively greater amount of status inequality of whites contributes more to their exchange mobility. The reason that black exchange mobility nearly equaled white exchange mobility in 1972-1974 is that the variation in black status increased relative to that of whites while the black index of exchange mobility ($1 - r$) remained high, both absolutely and relative to whites. In other words, while the black status structure has become more nearly like the white structure (although significant differences remain), a closer articulation between father's and son's status among blacks has

not occurred. Thus, we should not infer from the racial similarities in the aggregate amount of exchange mobility that blacks have achieved some sort of equality with respect to mobility.

Although blacks experienced large gains in structural mobility relative to whites between 1962 and 1972-1974, the amount of black structural mobility was still only .63 as large as that of whites in 1972-1974. The composition of black structural mobility in 1972-1974 was also different than that of whites. Whereas white structural mobility resulted almost solely from the intergenerational increase in status ($\bar{Y} - \bar{X}$), black structural mobility was significantly affected by the intergenerational increase in inequality ($s_y - s_x$), as well as by the intergenerational increase in average status. In other words, the black occupational structure was changing in a more complex way, relative to their fathers, than was the case for whites, and thus the nature of structural mobility is different for blacks.

The black gains in exchange and structural mobility relative to whites naturally lead to black gains in the amount of absolute mobility. But again we note racial differences in the process of mobility. The increase in absolute mobility relative to whites was not as great as might have been expected on the basis of the black gains in exchange and structural mobility. This is due to the fact that the negative covariance between the exchange and structural mobility of blacks in 1972-1974 was considerably greater than the negative covariance for whites. The greater negative covariance for blacks resulted from the very same factors that made black exchange mobility and structural mobility different in nature from that of whites, that is, from a higher index of exchange mobility ($1 - r$) and a greater intergenerational increase in inequality ($s_y - s_x$) among blacks. Thus, the tendency for males with downward exchange mobility to have greater upward structural mobility was greater among blacks than among whites and led to less absolute mobility among blacks (relative

to whites) than would otherwise have been expected.

In summary, the comparison of black and white mobility has shown that blacks experience less mobility than whites, although the differences are diminishing. The comparison has also shown racial differences in the nature of exchange mobility and structural mobility, as well as differences in the relationship between the two components of mobility. Thus, not only do blacks and whites differ with respect to the amount of mobility, they also show clear differences in the process of mobility.⁵

The occupational status mobility of females in 1972-1974 will now be examined. Although white females experience less mobility of each type, than white males, the mobility of white females is more similar to that of white males than is the case for black males. The exchange mobility of white females is only .91 as great as the exchange mobility of white males. Since there is little or no difference between the indices of exchange mobility of white males and females, the smaller amount of exchange mobility among white females is due to the fact that they have less variation in status than white males (the female standard deviation being only .9 as large as the male standard deviation). The smaller white female standard deviation reflects the fact that they are less likely to have high

⁵ As indicated in footnote 4, the discrepancies between the 1972-1974 NORC survey and the 1973 CPS would not affect these conclusions. The 1973 CPS data suggest that there would be little change in the relative amounts of black and white exchange mobility, nor in the racial differences in the makeup of exchange mobility. Although the 1973 CPS suggests a greater amount of black structural mobility, white structural mobility would still be somewhat greater and, more importantly, the racial differences in the makeup or process of structural mobility would remain. That is, black structural mobility would still be affected much more by $s_y - s_x$, and much less by $\bar{Y} - \bar{X}$, than would white structural mobility. These racial differences still would cause a greater negative covariance between exchange and structural mobility for blacks than for whites and lead to less absolute mobility, relative to whites, than would be expected from the racial differences in exchange and structural mobility.

and low status occupations than white males (Treiman and Terrell, 1975; McClendon, 1976).

The structural mobility of white females is also less than that of white males. The smaller female structural mobility results from the fact that they experience a smaller intergenerational shift in average status ($\bar{Y} - \bar{X}$). This, in turn, results from the fact that white females in the labor force come from a somewhat higher status background than white males. Since white males and females have about equal occupational status, on the average, white females experience less structural mobility than white males.

The nature of female structural mobility is also somewhat different from that of males. White males and females come from equally varied backgrounds. However, females have less varied status than their fathers while males have more varied status than their fathers. Thus, white males experience an intergenerational increase in status inequality ($s_y - s_x$) while females experience a decrease in inequality. This produces a positive correlation between structural mobility and father's status for males and a negative correlation for females. In other words, males from high status backgrounds have more upward structural mobility than males from low status backgrounds, whereas females with high status fathers have less structural mobility than those with low status fathers. The intergenerational decrease in female inequality also produces a positive covariance between exchange and structural mobility, as opposed to the more typical negative covariance for white males. The female positive covariance means that they have a greater amount of absolute mobility, relative to white males, than would otherwise be expected, although the absolute mobility of white females is still less than that of males.

In summary, although white females experience less mobility than white males, the differences are not great. The differences result primarily from the fact that white females come from slightly higher status backgrounds and have less varied occupational status than white males. That is, the

sex differences in mobility result from differences in the male and female occupational structures, not from differences in the index of exchange mobility. These structural differences not only result in somewhat less female mobility, but also lead to an interesting, if not overly pronounced sex difference in the process of mobility, as indicated by the positive covariance between the exchange and structural mobility of females.

The mobility of black females will not be examined in detail because the sample size ($N = 81$) is too small to be reliable. The data suggest that the mobility of black females is different from that of the previous groups analyzed. With respect to the amount of exchange, structural and absolute mobility, black females are similar to black males. However, the makeup of the structural and exchange components of black female mobility differs from that of black males. Black females appear to have a lower index of exchange mobility than black males and, with respect to structural mobility, they appear to experience a greater intergenerational increase in inequality.

Educational Mobility

It will be instructive to examine educational mobility, which differs in several interesting ways from occupational status mobility. The NORC General Social Surveys of 1972-1974 will be used to estimate the educational mobility of white males and females and black males and females. The analysis will not be confined to members of the labor force, but will instead include all respondents 25 to 64 years of age. The means and standard deviations of father's education and respondent's education, the correlation between father's and respondent's education, and the exchange, structural and absolute mobility components are given in Table 3.

The most obvious difference between educational mobility and occupational status mobility is that educational structural mobility is much greater, relative to exchange mobility, than was the case for status mobility. This is true for each group, with structural mobility actually being greater

Table 3. Components of Educational Mobility of Persons 25 to 64 Years of Age by Sex and Race, 1972-1974

	Males		Females	
	Whites	Blacks	Whites	Blacks
\bar{Y}	12.8	10.7	12.3	10.8
\bar{X}	9.0	6.9	8.2	6.7
s_y	3.5	4.1	2.5	3.3
s_x	4.3	4.3	4.0	4.5
r	.45	.44	.51	.52
N	1,002	115	1,118	114
Mobility				
Absolute	5.6	5.9	4.7	5.7
Exchange	3.7	4.3	2.5	3.2
Structural	3.9	3.8	3.4	4.3
Covariance	1.5	.5	1.8	1.9

than exchange mobility for each of the groups except black males. Part of the reason for the relatively great amount of structural mobility is that the index of exchange mobility is lower for each group than was the case for occupational status mobility. But this is not the only reason. The structural changes in the educational distribution are greater than the structural changes in occupational status. For each group, the percentage increase in mean years of school completed is greater than the percentage increase in mean occupational status. Thus, structural changes in schooling (i.e., an increase in the amount of schooling and a declining inequality of schooling) contribute more to the absolute amount of educational mobility than structural changes in occupational status contributes to absolute status mobility.

A second striking feature of educational mobility is the intergenerational decline in inequality ($s_y < s_x$). This decline means that structural mobility is negatively correlated with father's education. Thus, persons from above average educational backgrounds will have less upward structural mobility than those from below average backgrounds. Since the decline in inequality is greater for females, and greatest for white females, this process is most pronounced for them. The decline in inequality also, of course, means that there is a positive covariance between exchange and structural mobility, as opposed to the nega-

tive covariance which is typical of occupational status mobility. Thus, persons who are upwardly exchange mobile with respect to schooling will tend to have more upward structural mobility. Since exchange mobility and structural mobility tend to reinforce each other, the aggregate amount of absolute mobility is greater than would otherwise be expected.

Several factors also stand out with respect to exchange mobility. As mentioned above, the index of educational exchange mobility is lower than the index of occupational exchange mobility; and the index of educational mobility is lower for females than for males. This is usually attributed to the fact that education is more of a luxury for females and more of a necessity for the occupational pursuits of males (Treiman and Terrell, 1975; McClendon, 1976). Because of their lower index of exchange mobility, females have less exchange mobility than males. They also have less exchange mobility because the amount of female schooling does not vary as much as that of males. This is mostly true for white females, who have considerably less exchange mobility than the other groups. In fact, white females also have less structural mobility and less absolute mobility, making them the least mobile of the groups.

In summary, the most distinctive features of educational mobility, as compared to occupational status mobility, are the relatively greater amount of structural mobility, the lower index of exchange mobility and the positive covariance between exchange and structural mobility.

Summary and Conclusion

This paper has argued that the study of vertical social mobility has suffered from overreliance on occupational mobility tables, from underutilization of occupational status scales and from failure to conceptualize social mobility as relative mobility. Definitions and measures of absolute mobility, exchange mobility and structural mobility were then given at the individual level. Each type of mobility involves the idea of mobility distance on some relevant vertical dimension and, thus, the measures for each type of mobility assume that an

interval scale for the dimension exists. Exchange mobility was conceptualized as relative mobility and a measure was given which has a zero sum nature and is relatively free from structural influences. Structural mobility was then defined as the distance a person would move if the person experienced no exchange mobility. It was seen that the sum of an individual's exchange mobility and structural mobility is equal to his absolute mobility, that is, the distance between the person's absolute scores in the origin and destination distribution.

Measures of aggregate population mobility were then given for each type of mobility. It was seen that the three aggregate measures could be computed from five readily available statistics, the means and standard deviations of the origin and destination distributions and the correlation between the distributions. These measures were applied to intergenerational occupational status data for the United States. There was little or no evidence that the mobility of white males had changed between 1962 and 1972-1974. However, a substantial increase in the mobility of black males was found for this period. The mobility of black males was still considerably different from that of white males in 1972-1974, both with respect to the amount of mobility and the patterns of mobility. Although several interesting differences were found between the mobility of white males and females, the mobility of white females was more similar to that of white males than was the case for black males. Finally, aggregate educational mobility was briefly examined and compared to occupational status mobility. Several interesting differences were found, most important of which was the larger amount of structural mobility, relative to exchange mobility, in educational mobility.

Although the application of the measures presented in this paper has been limited to aggregate interperiod and intergroup comparisons within a single society, this limitation was dictated only by considerations of space. There are at least two obvious directions in which the analysis could be expanded. First, each type of mo-

bility could be analyzed in greater depth than the aggregate summaries given in this paper. For instance, questions concerning the symmetry of a mobility distribution might be of interest. Is long range mobility more likely in an upward direction or in a downward direction? This question concerning the skewness of the mobility distribution can be easily answered from the raw mobility data. Another question of interest concerns the degree to which aggregate mobility is made up of a few persons making relatively long-distance moves versus many persons making relatively short-distance moves. This question concerns the kurtosis of the mobility distribution, which can also be easily computed from the raw data. Unfortunately, neither skewness nor kurtosis can be computed from the means, standard deviations and correlation, as could aggregate mobility. Preliminary examination of these questions indicates that, with respect to occupational status mobility, the exchange mobility distribution and absolute mobility distribution for all members of the labor force in 1972-1974 are both approximately normally distributed. However, both distributions appear more peaked than normal for the black male subpopulation.

The second obvious direction in which the analysis could be expanded is to cross-societal comparisons of mobility. Until very recently, such comparisons have been impossible with respect to occupational status mobility because of the lack of a standardized status scale. Now, however, the publication of Treiman's (1975) Standard International Occupational Prestige Scale makes such comparative studies possible, at least for occupational prestige. Treiman (1975) has recoded the occupational data from mobility studies in at least eight different nations into the Standard Prestige Scale. If we add the occupational mobility data for Costa Rica and Haiti, which have been recoded into the Standard Prestige Scale by Lin and Yauger (1975), there appear to be at least ten nations for which absolute, exchange and structural prestige mobility could be compared.

Of course, the ultimate goal of cross-national studies is not just to compare

rates, amounts and patterns of mobility, but to find societal correlates of mobility. There has been considerable interest in the relationship between industrialization and mobility. For instance, although Hazelrigg and Garnier (1976) found no relationship between energy consumption and the rate of circulation (exchange) mobility for seventeen nations, they did report a moderately strong correlation between energy consumption and the rate of mobility when inter-county variations in the shape of the origin occupational distribution were not controlled, using rather crude (nonmanual, manual, farm) mobility tables. Their research clearly suggests that different components of mobility have different relationships to the level of industrial development. There also must be other societal characteristics, in addition to the level of development, which are relevant to mobility. It is hoped that the individual and aggregate measures of exchange, structural, and absolute mobility proposed in this paper will help specify these relationships.

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A THEORETICAL MODEL FOR THE INTERPRETATION OF TABLES OF SOCIAL MOBILITY *

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This paper describes a procedure for analyzing tables of social mobility. The method is deduced from a few simple postulates relating to the mobility process. The major assumption is that sons may be ranked on a single dimension of status and that their occupational attainment is determined by their ranking on this dimension and by current labor needs. A priori specification of the general nature of the distribution of sons along this dimension is an operational requirement of the model. The procedures are applied to the intergenerational mobility tables of Britain and Denmark. The results show that, in large measure, the tendency of sons to take positions similar to those of their fathers can be accounted for without recourse to a principle of status inheritance. Another advantage of these procedures is that, within the framework of the model, they permit the effects on mobility of changing labor requirements to be separated from the effects due to the openness or fluidity of the social system.

The intergenerational mobility table or cross-classification of sons' and fathers' statuses typically is constructed to show the extent to which sons in a population inherit or profit from the social and economic positions of their fathers. In general, these tables are used for comparative purposes to determine whether a principle of

inheritance operates more strongly in one population than in another or whether it is changing over time. The degree of social inheritance is taken to be strong insofar as the statuses of sons are positively influenced by those of their fathers. Unfortunately, statements based on the comparison of mobility tables typically must be qualified in that the marginal distribution of statuses generally will differ from population to population. If, as in standard analyses, the marginals are taken as given or as logically prior to the allocation of sons, similar or proportional cross-tabulations cannot be expected. As a consequence, the same principle of inheritance operating in different contexts will typically give rise to different allocations. The problem is dis-

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cussed in an extensive literature. See, for example, Lipset and Zetterberg (1956) and Boudon (1973).

It is important to note that the degree to which a principle of inheritance operates in a population cannot be inferred straightforwardly from its mobility table. The observed cross-tabulation is an outcome of a rule of allocation and, in general, a plurality of rules may be constructed which will yield the outcome. Moreover, some of these will reflect a stronger principle of inheritance than will others. As an illustration, consider the following "observed" allocation:

		Son's Position		
		High	Low	Σ
Father's Position	High	50	50	100
	Low	50	100	150
	Σ	100	150	250

In the context of the given marginal distributions, this allocation would arise if (1) the chance that a son of low origins will attain a high position is always two-thirds those of sons of high status origins or if (2) 50% of high status positions are reserved for sons of low status backgrounds. In general, the first principle would be taken as more consistent with a principle of inheritance than the second. The indeterminacy which this example illustrates cannot be escaped unless the interpretation of mobility tables is approached with an explicit theory regarding the allocation of human labor.

Inherited Advantages, Competition, and Career Outcomes

It is reasonable to conceive of occupational mobility as a simple two-stage process. Prior to settling into careers, sons accumulate a set of advantages (or disadvantages) which bear on their acceptability for and desire to attain the more prestigious occupations. The value of the advantages a son will accumulate is conditioned by the position of the family in which he was born and reared. In general, sons of families of high standing are relatively well-educated and, perhaps, more socialized to the demands of the more responsible employ-

ments than sons of meaner origins. This is not to exclude purely ascriptive influences. Family name and family connections, for example, can figure into the sum of an individual's advantages. Sons are recruited to the more prestigious positions on the basis of their accumulated advantages or "quality" with the highest class recruiting the most qualified sons, the next highest classes choosing the most qualified among those not recruited to the highest class, and so forth. Under this formulation, the degree of status inheritance lies in the differences in the distribution of quality of sons of varying origins. It should be noted that the word "quality," as used here, is assigned a very broad definition. Stated strictly, the quality of a son's labor is taken to be high insofar as he can compete successfully for prestigious employment. Thus, his qualifications include not only level of skill and any valued ascriptive factors but also the intensity of his own desire or motivation for high-level work.

We also assume that labor quality can be measured on a single dimension and that sons are susceptible to ordinal ranking in this respect. This means that the rank order of sons with respect to quality should fully correspond to the rank order of their occupational positions. We may illustrate this with two sons and three positions, viz., physician, attorney and dentist. Both sons are highly talented, responsible and ambitious, and both perceive the value of occupations in the same way. Thus, if recruits were needed in all positions both would become doctors; if this option were closed, both would become lawyers; and if neither of these options were open, both would become dentists. In other words, the same rule of labor substitution applies to both sons. Suppose, alternatively, that this rule applies to just one son and that the other, if denied the opportunity to become a physician, will become a dentist rather than a lawyer. This could occur because the second son evaluates occupations differently than the first, because his verbal skills are weak relative to his other talents or simply because he prefers the work of a dentist over that of an attorney. In any case, the two sons do not scale in the same

way. In this example, the two sons are not susceptible to the same ordinal ranking and a single rule of substitution does not apply for both of them.

The counter-example describes a quite realistic situation and, thus, the assumption of a single rule of substitution is not expected fully to reflect actual recruitment procedures. On the other hand, it is reasonable to expect that a single rule will dominate the recruitment process. In this regard, it should be noted that research results show a broad population consensus on the prestige ranking of occupations and a widespread preference for working in the more prestigious jobs. Also, insofar as jobs are rated highly, they typically are allocated to persons with strong educational and vocational training and, in some measure, to persons with positively valued ascriptive characteristics. It is also of interest that the "functionalist" notion, *viz.*, that the reward system operates to insure that the most qualified persons will occupy the most important positions is quite consistent with the rule we propose. Finally, we would strain credibility if we pressed the counter-example. It would be a rare son who, denied the option of becoming a physician but having at hand the opportunity of becoming lawyer or dentist, would wind up as an office clerk. Thus, it is our expectation that a single rule of substitution, while an incomplete representation of reality, will serve as a strong first approximation to the complex process of labor allocation.

If the set of destination classes of a standard mobility table represent ordinal groupings with respect to the rule of substitution, it is possible, given assumptions regarding the general distribution of labor quality, to obtain the specific distributions of labor quality for sons of each origin class. The comparison of these within-class distributions provides a description of the competitive standing of sons of differing origins. These distributions are, under the present formulation, the basis of labor allocation but stand prior to any specific allocation. In other words, these distributions are not determined by the marginals. Our purpose is to extract these within-class dis-

tributions of labor quality from national mobility tables and thus draw inferences as to the manner and degree in which a son's origins affect his chances in job competition. We begin with a formal description of the recruitment process and proceed to illustrate the manner in which variation in marginal distributions affects labor allocation. We next will describe an operational method for identifying within-class densities of labor quality. The paper concludes with an empirical application.

The Structure of Status Attainment

If quality is measured on a single dimension, there is a number, x , associated with each son such that the value of x is related to the summary value of his status-related advantages. The density function for sons of the i^{th} origin class is defined as $f_i(x)$ so that $f_i(x)dx$ is the proportion of sons of the i^{th} origin class with status-related advantages valued at x to $x + dx$. Figure 1 is a hypothetical representation of the densities of three origin classes. The graph of the first origin class, $f_1(x)$, is weighted to the right of those of the other two classes, indicating that these sons are relatively advantaged. The graph of the third class, $f_3(x)$, is furthest to the left, so that the average value of labor quality is intermediate in the second class and lowest in the third. Also, the relative peakedness of these densities indicates that sons of the first origin class are the least dispersed and that sons of the third social class are the most dispersed over values of labor quality:

If sons of these three origin classes are to be allocated over ordered destination classes on the basis of quality, each son allocated to the highest ranked class will exceed in quality every son not allocated to that class. Insofar as quality follows a continuous scale, ties will not occur and we will be able to define a value of quality, say x_1 , so that all sons with values above x_1 are in the highest social class and all sons with values below x_1 are in lower ranked classes. Moreover, this number will approach a unique value insofar as the origin class is large. If α is the upper bound of labor quality, the area under any curve lying to the right of x_1 , *viz.*,

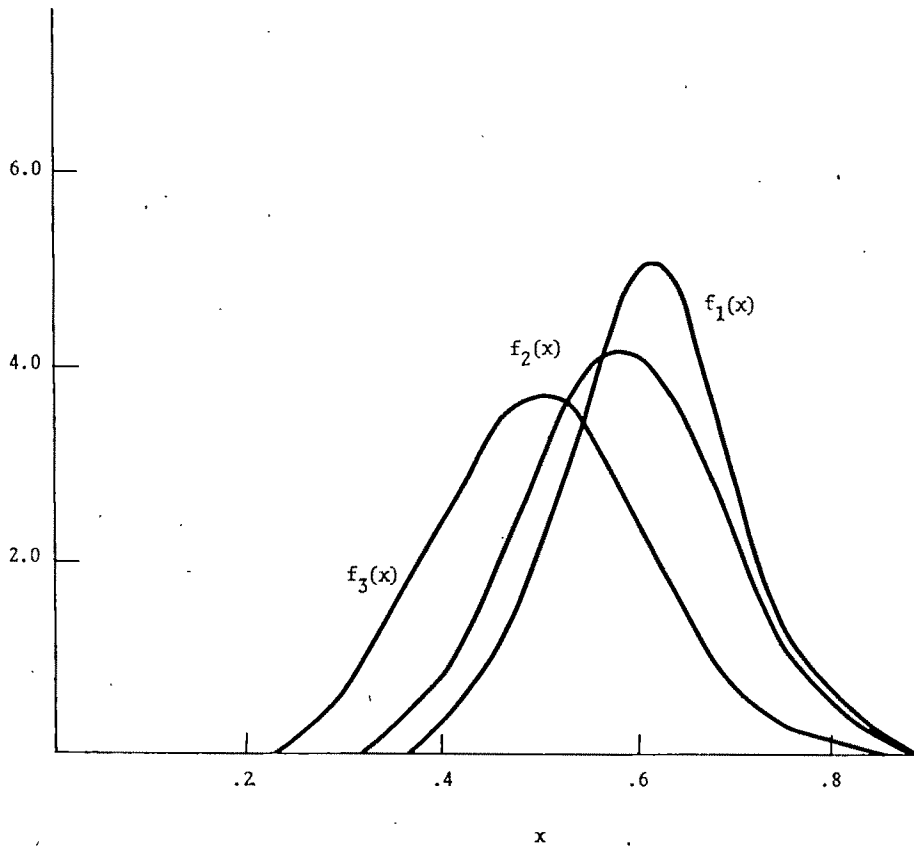


Figure 1. Hypothetical Densities of Status-Related Advantages in Three Social Classes

$$F_1(x_{1,\alpha}) = \int_{x_1}^{\alpha} f_1(x) dx, \quad (1)$$

will be equal to the probability that a son of the specified origin class will attain a position in the highest destination class. Also, if N^0_i is the number of sons of the i^{th} origin class, there will be $N^0_i F_1(x_{1,\alpha})$ sons of this class in the preferred destination class. Finally, the total number of sons attaining positions in the highest class will be

$$N^D_1 = \sum_{i=1}^k N^0_i F_1(x_{1,\alpha}), \quad (2)$$

if k is the number of origin classes. Given the marginal values, N^0_i and N^D_1 , and given the within-class densities of labor quality, $f_i(x)$, the specification of the numbers and proportions of each origin class who enter the highest destination class is

contained in the specification of x_1 . Since the functions $F_1(x_{1,\alpha})$ are monotonically decreasing functions of x_1 and since N^D_1 is a weighted sum of the set, $F_1(x_{1,\alpha})$, N^D_1 is itself a monotonically decreasing function of x_1 . Thus if $f_i(x)$ is continuous and the origin classes are large, a unique value x_1 will fulfill equation (2).

As an illustration of this relation, suppose labor quality in the three social classes is distributed as in Figure 1 and that each origin class contains 1000 sons. In addition, we presume that the market is static so that 1000 sons are to be recruited to the highest ranked class. If we pick as a trial value $x^*_1 = .63$, the area under the first curve, $N^0_1 F_1(.63, 1.00)$, will be $1000 \times .352 = 352$, indicating that 352 sons of this class will enter the highest origin class. Also, $N^0_2 F_2(.63, 1.00) = 272$ and $N^0_3 F_3(.63, 1.00) = 99$, so that substitution on the right hand side of equation 2 yields

$$355 + 272 + 99 = 726,$$

which is lower than the true value of N^D_1 . If we choose $x^*_1 = .58$, we obtain $N^0_1 F_1(.58, 1.00) = 597$, $N^0_2 F_2(.58, 1.00) = 473$ and $N^0_3 F_3(.58, 1.00) = 213$, and these add to 1283 which is too high. If we select $x_1 = .604$, the relation is fulfilled exactly with 477 sons of the first origin class, 72 sons of the second class and 151 sons of the third origin class obtaining positions in the highest destination class. While these numbers add to 1000, those obtained setting x^*_1 above .604 will always sum to a number smaller than 1000 and those obtained setting x^*_1 lower than .604 will always sum to a number larger than 1000. Thus, .604 is a unique solution of x_1 . Finally, taking the marginal distributions as given, the allocation of 477, 372 and 151 sons of the first, second and third origin classes, respectively, to the highest destination class is the only allocation consistent with the hypothesized densities of labor quality. The cutting point for recruitment to the highest social class might be indicated in Figure 1 by dropping a vertical bar at .604.

Recruitment to each successive ranked class follows a similar logic. Thus, if x_1 is the cutting point for the highest destination class, all sons recruited to the second highest class will have status related advantages valued less than x_1 . Also, any son recruited to the second ranked class will exceed in value any son recruited to lower ranked classes. We can, therefore, find a number, x_2 , separating the second and third class so that x_2 fulfills the relation

$$N^D_2 = \sum_{i=0}^k N^0_i F_i(x_2, x_1), \quad (3)$$

where $F_i(x_2, x_1) = \int_{x_2}^{x_1} f_i(x) dx$ is the propor-

tion of the sons of the i^{th} origin class with status-related advantage valued between x_2 and x_1 and where N^D_2 is the total number of sons recruited to the second ranked social class. In the present example, x_1 is set at .604 and by iteration we find x_2 is located at .516, given $N^D_2 = 1000$ and that each origin class contains 1000 sons. This

implies that 370, 354 and 276 sons will be recruited to the second ranked destination class from the first, second and third origin class, respectively. More generally, we determine x_j , the lower bound of the j^{th} social class as the number fulfilling the relation

$$N^D_j = \sum_{i=1}^k N^0_i F_i(x_j, x_{j-1}), \quad (4)$$

given that the marginals, N^D_j and N^0_i , and the densities, $f_i(x)$, are known and that x_{j-1} is determined by solution of the preceding equation. Finally, if the number recruited to the lowest ranked social class is N^D_k , the number recruited to this class from the i^{th} origin class will be $N^0_i F_i(\beta, x_{k-1})$

$$= N^0_i \int_{\beta}^{x_{k-1}} f_i(x) dx \text{ if } \beta \text{ is the lower bound}$$

of labor quality. In fact, this number will be equal to the number of sons in the i^{th} class who were not recruited to higher classes. If, in the present example, we recognize three destination classes, there will be $N^0_1 F_1(\beta, x_{k-1}) = (1000 - 477 - 370) = 153$ sons of the first origin class recruited to the lowest ranked destination class. Proceeding in the same fashion, we have 274 sons of the second origin class and 573 sons of the third origin class recruited to the lowest ranked destination class.

Marginal Distributions and Patterns of Mobility

Under the present formulation, any specific mobility allocation depends not only on the within-class distributions of status-related advantages but on the numbers of sons originating in each social class and on current labor requirements. This is reflected in equations (2), (3), and (4) where the allocation is a function not only of the densities, $f_i(x)$, but of the marginal distributions, N^D_j and N^0_i , as well. Moreover, by varying the marginals, it is possible to construct allocations which seem to imply more or less equalitarian rules of labor allocation and which measure differently on standard indices of social mobility. Thus, it is possible to conclude for two populations which are similar in within-

class distribution of status-related advantages that one is more open or equalitarian than the other.

A few illustrations are sufficient to show the implications of marginal distributions for measured social mobility. Suppose that the within-class densities of status-related advantages are those displayed in Figure 1 and that, as in our initial illustration, there are 1000 sons in each of the three origin classes, but that we vary the example to reflect an upgrading of labor by allocating 1500 sons to the highest ranked destination class, and 1000 and 500 sons, respectively, to the middle and to the lowest ranked destination classes. Solving successively for

$$1500 = 1000 F_1(x_1, \alpha) + 1000 F_2(x_1, \alpha) + 1000 F_3(x_1, \alpha)$$

and for

$$1000 = 1000 F_1(x_2, x_1) + 1000 F_2(x_2, x_1) + 1000 F_3(x_2, x_1),$$

we obtain $x_1 = .562$ and $x_2 = .457$ and this implies the allocation shown in the middle panel of Table 1. The allocation implied under the initial illustration is shown for comparison in the upper panel of the table. If we use chi-square as an index of the degree to which the status of sons depends on the status of their fathers we obtain $\chi^2 = 509.6$ as against a value of 462.2 for the initial illustration. Also, Pearson's contingency coefficient takes on a value of .381 when labor needs are upgraded as against .365 when requirements are static. In addition, the fraction of sons who are mobile or whose situation is described by off-diagonal cells is 54.3% given upgrading and 53.2% for the static case. These considerations imply slightly more association between 'sons' and fathers' statuses and slightly more mobility in the case in which labor requirements are upgraded.

If we take a different tack and evaluate the amount of circulation mobility as a measure of the openness of the social structure, we find that there are 1129 moves under upgrading which occurred net of the 500 moves which are required to fulfill the marginals and that there are 1596 moves in the static case, none of which is needed to satisfy the marginals. Thus, only 37.6% of

Table 1. Labor Allocations under Three Sets of Marginal Distributions, Given Hypothetical Densities of Status-Related Advantages

Origin Class	Destination Class			
	I	II	III	Σ
I	477	370	153	1000
II	372	354	274	1000
III	151	267	573	1000
	1000	1000	1000	3000
	I	II	III	Σ
I	681	279	40	1000
II	550	340	110	1000
III	296	381	350	1000
	1500	1000	500	3000
	I	II	III	Σ
I	173	135	192	500
II	134	112	254	500
III	193	253	1554	2000
	500	500	2000	3000

sons "circulate" under upgrading as against 53.2% in the static case. Using these procedures, one might well conclude that the cross-tabulation in the middle panel of Table 1 describes a less open status structure than the cross-tabulation in the upper panel of the table.

If the status structure were bottom-heavy and static so that there were 500 sons in the first and second classes and 2000 in the third in both generations, measured mobility would fall. In this case, we obtain .562 and .457 for x_1 and x_2 , respectively, and this yields the allocation of the third panel of Table 3. Chi-square falls to 362.9 and the value of the contingency coefficient falls to .328. Also, only 38.7% of sons are mobile. Thus, this allocation would seem to describe a less fluid or less open population than that of the initial allocation. Finally, we will obtain varying results when we apply other standard measures such as mobility ratios to the three tables.

The Identification of the Densities of Status-Related Advantages

The preceding illustrations only confirm the well-appreciated fact that cross-population differences in the marginal distributions of origin and destination statuses introduce a measure of indeterminacy into



comparative statements relating to the openness or fluidity of class structures or to the degree and manner of dependence of a son's status on that of his father. In particular, we showed how measured mobility can vary even when the within-class densities of status-related advantages do not. However, there can be little satisfaction in this demonstration unless something can be said about the densities themselves. In other words, the theory of mobility outlined here will provide a structure for comparative statements about social mobility only if information relating to the within-class quality densities can be inferred from data.

A set of densities will fulfill the requirements of the model insofar as it preserves the ordering of status-related advantages across classes and a plurality of sets should approximate this condition. We are near to an operational solution if we assert that each density in the set is a member of some specified family of functions. This assertion will be reasonable to the extent that the set of curves described by the family is highly flexible since this will optimize the chances that it includes a set which closely preserves the between-class ordering of status-related advantages.

Let $\phi(x)$ be a family of functions, any member of which is uniquely identified by a set of parameters, $\theta_i = [\theta_{i1}, \theta_{i2}, \dots, \theta_{ip}]$. If the densities of status-related advantages are members of $\phi(x)$ for all classes, then any specific density, say $f_i(x)$, may be expressed as $\phi(x, \theta_i)$ which is to say that we know $f_i(x)$ if we know the associated vector, θ_i . Thus, if recruitment is based strictly on quality and if the family of functions which describe the within-class densities of quality is specified, the observed cross-classification of sons' and fathers' statuses can be expressed in terms of the marginal distribution of class origins and the parameters θ_i associated with each origin class. Stated explicitly, the observed cross-classification,

$$[N_{ij}] = \begin{bmatrix} N_{11} & N_{12} & \dots & N_{1k} \\ N_{21} & \dots & \dots & \dots \\ \vdots & & & \vdots \\ N_{k1} & \dots & \dots & N_{kk} \end{bmatrix}, \quad (5)$$

in which the element N_{ij} is the number of sons of the i^{th} origin status obtaining positions in the j^{th} ranked destination status, will, net of stochastic disturbances, correspond to

$$E[N_{ij}] = \begin{bmatrix} N^o_1 \Phi(x_1, \alpha, \theta_1) & N^o_1 \Phi(x_2, x_1, \theta_1) & \dots & N^o_1 \Phi(\beta, x_{k-1}, \theta_1) \\ N^o_2 \Phi(x_1, \alpha, \theta_2) & \dots & \dots & \vdots \\ \vdots & & & \vdots \\ N^o_k \Phi(x_1, \alpha, \theta_k) & \dots & \dots & N^o_k \Phi(\beta, x_{k-1}, \theta_k) \end{bmatrix}. \quad (6)$$

In this latter matrix, N^o_i is the number of sons originating in the i^{th} social class and

$$\begin{aligned} \Phi(x_j, x_{j-1}, \theta_i) &= \int_{x_j}^{x_{j-1}} \phi(x, \theta_i) dx \\ &= \int_{x_j}^{x_{j-1}} f_i(x) dx \end{aligned} \quad (7)$$

is the fraction of sons of the i^{th} origin class with status-related advantages valued between x_j and x_{j-1} . The cutting points are chosen to correspond to the boundaries of the destination classes so that $\Phi(x_j, x_{j-1}, \theta_i)$ is the expected proportion of sons of the i^{th} origin class who obtain positions in the j^{th} ranked destination class.

If p is the number of parameters required to identify $\phi(x)$, for each origin class, then $k \times p$ parameters must be estimated if the set $f_i(x)$ is to be recovered. In addition, the $k-1$ cutting points must be fixed to assure that the marginal distribution of destination statuses agrees with the data. Thus, the total number of equations needed for the estimation of parameters and for constraint of the marginals is $kp + k - 1$. If, as is typically the case, the matrix $[N_{ij}]$ is of full rank, it will yield $(k-1)^2$ linearly independent equations. Since $kp + k - 1$ is less than $(k-1)^2$ when p is less than or equal to $k-3$ and since this is not the case when p is greater than $k-3$, estimation

of the set $f_j(x)$ is operational if and only if p is less than $k-3$. Thus, we are allowed one parameter in a 4×4 table, two in a 5×5 table, and so forth.

These considerations indicate that the number of parameters needed for the identification of the members of the selected family of functions must be kept small if the model is to be operational for tables of modest dimensions. In other words, parsimony as well as flexibility must be taken into account in the specification of a family of curves. Balancing these needs, we have chosen the beta family. Members of this class of curves are identified by two parameters so that estimation of the set, $f_i(x)$, is possible when at least five status categories are recognized. Also, the functions included in the family are quite varied and can be U-shaped, an inverted U-shape, J-shaped or flat. The density of the beta function is

$$\phi(x, a, b) = \frac{x^a(1-x)^b}{\int_0^1 x^a(1-x)^b dx}, 0 \leq x \leq 1 \quad (8)$$

where a and b are the parameters which identify members of the family. Thus, for a specific within-class density we write

$$f_i(x) = \phi(x, a_i, b_i) = \frac{x^{a_i}(1-x)^{b_i}}{\int_0^1 x^{a_i}(1-x)^{b_i} dx}, \quad (9)$$

and for the expected proportion of sons of the i^{th} origin status with positions in the j^{th} ranked destination status we have

$$\begin{aligned} F_i(x_j, x_{j-1}) &= \Phi(x_j, x_{j-1}, a_i, b_i) \\ &= \frac{\int_{x_{j-1}}^{x_j} x^{a_i}(1-x)^{b_i} dx}{\int_0^1 x^{a_i}(1-x)^{b_i} dx}. \end{aligned} \quad (10)$$

The likelihood of obtaining the observed cross-classification of origin and destination statuses given the marginal distribution of origins, specified cutting points x_j , and specified pairs of parameters a_i and b_i for a table of k dimensions is

$$\begin{aligned} L &= \prod_{j=1}^k \prod_{i=1}^k [\Phi(x_j, x_{j-1}, a_i, b_i)]^{N_{ij}} \\ &= \prod_{j=1}^k \prod_{i=1}^k \left[\frac{\int_{x_{j-1}}^{x_j} x^{a_i}(1-x)^{b_i} dx}{\int_0^1 x^{a_i}(1-x)^{b_i} dx} \right]^{N_{ij}} \end{aligned} \quad (11)$$

Taking derivatives of the logarithm of L with respect to each a_i and each b_i and setting the resulting expressions equal to zero, we obtain expressions in which the a_i and b_i are the values most likely to give rise to the observed outcome. Specifically, we have k equations of the form

$$\begin{aligned} \frac{\partial \ln L}{\partial a_i} &= \sum_j N_{ij} \left[\frac{\int_{x_{j-1}}^{x_j} (\ln x) x^{a_i}(1-x)^{b_i} dx}{\int_{x_{j-1}}^{x_j} x^{a_i}(1-x)^{b_i} dx} \right] \\ &\quad - N_{0i} \left[\frac{\int_0^1 (\ln x) x^{a_i}(1-x)^{b_i} dx}{\int_0^1 x^{a_i}(1-x)^{b_i} dx} \right] = 0 \end{aligned} \quad (12)$$

and k equations of the form

$$\begin{aligned} \frac{\partial \ln L}{\partial b_i} &= \sum_j N_{ij} \left[\frac{\int_{x_{j-1}}^{x_j} (\ln(1-x)) x^{a_i}(1-x)^{b_i} dx}{\int_{x_{j-1}}^{x_j} x^{a_i}(1-x)^{b_i} dx} \right] \\ &\quad - N_{0i} \left[\frac{\int_0^1 (\ln(1-x)) x^{a_i}(1-x)^{b_i} dx}{\int_0^1 x^{a_i}(1-x)^{b_i} dx} \right] = 0. \end{aligned} \quad (13)$$

The marginal distribution of origin statuses is accounted for explicitly in equations of forms (12) and (13) but the marginal distribution of destination statuses is taken account only insofar as the cutting points, x_j , are accurately specified. To obtain values of x_j which are consistent with the marginals we fix the constraints

$$\sum_i N_{ij} \Phi(x_j, x_{j-1}, a_i, b_i) = \sum_i N_{ij} \left[\int_{x_j}^{x_{j-1}} x^{a_i-1} (1-x)^{b_i-1} dx / \int_0^1 x^{a_i-1} (1-x)^{b_i-1} dx \right] = N_{ij}^D, \quad (14)$$

which is to say we constrain the expected cell entries to agree with the observed column sums. There are $k-1$ cutting points or values of x_j and so $k-1$ equations of form (14) are needed. Thus, one among the k possible equations of this form is redundant and is, therefore, not used in the estimation.

We now have $2k + (k-1)$ equations for the identification of $2k$ parameters and $k-1$ cutting points so that a unique solution will exist if matrix of observed values is of full rank. However, analytic solutions for the desired values cannot be written in closed form and numerical methods therefore are required. A quadrature routine was adapted to the present system of equations and yielded the results we will report. In some preliminary results, the marginal distribution of destination statuses implied by the estimated x_j 's deviated modestly from the observed distribution. A doubling of the penalties associated with deviations from the equations of form (14) reduced the discrepancies to small proportions. The routine converges on the same solution when the trial values of a_i , b_i and x_j are varied and convergence is reasonably swift, given the dimensions of the system. Also, the residual error or lack of fit of the estimated values to the equations is very small.

Finally, it should be noted that these estimation procedures are based on asymptotic considerations and, thus, are appropriate only insofar as sample size is large. From an analytic point of view, the data are joint order statistics of the k origin populations. The development of estimation procedures appropriate to small samples would be a most tedious process and it is unlikely that the resulting formulation would be operational except at great cost. Considerations of cost also have dictated that we neglect the sampling distributions of the estimated parameters. Thus, any statistical analysis of the results is necessarily

limited to summary methods such as χ^2 tests of goodness of fit. In general, it is best to apply the model to large samples and to interpret the results deterministically.

An Evaluation of the Fit of the Model

A cross-classification of sons' and fathers' statuses in post-war Britain is provided by Glass (1954). Five origin and destination classes are recognized and the set of occupational categories were intended to represent ordinal classes. Thus, the requirements for application of the method are met.

The observed cross-classification is shown in the upper panel of Table 2. Appearing above and to the right of these numbers and set in brackets, we have the expected values or the elements of the solution for $E[N_{ij}]$ under the assumption that the within-class densities of labor quality are members of the beta family. A comparison of the observed and expected values provides a general description of the fit of the model. In general, the expected values reproduce the original table rather well. Thus 97.1% of sons are correctly classified under the expected allocation, which is to say only 99.9 sons or 2.9% of the total would have to be reallocated in order to reproduce the observed cross-tabulation exactly. On the other hand, the lack of fit cannot be assigned to randomness. Of the sixteen degrees of freedom of the original table, fourteen were spent in estimation, so that two remain for testing. The chi-square for goodness of fit is 41.8, and this means that the notion that randomness accounts for the discrepancy of observed and expected values cannot be seriously entertained.

It should be recalled that this model is offered as a first approximation to the recruitment process and not as a complete representation of it. Thus, non-random deviations from the expected pattern, while identifying limitations of the model, do not necessarily indicate rejection of it. Moreover, the pattern of deviations can provide us with additional information by suggesting other significant dimensions of the recruitment process. It is of interest in this connection that 58.4 of the 99.9 misplaced cases involve a shortfall in the assignment

Table 2. Observed and Expected Cross-Classifications of Statuses for British and Danish Sons*

Origin Status	Destination Status					
	British Sons					Σ
	1	2	3	4	5	
1	50 [48.1]	45 [42.7]	8 [14.3]	18 [18.9]	8 [5.0]	129
2	28 [36.6]	174 [149.5]	84 [90.6]	154 [166.8]	55 [51.5]	495
3	11 [7.2]	78 [87.9]	110 [86.7]	223 [232.8]	96 [103.4]	518
4	14 [6.8]	150 [148.3]	185 [195.2]	714 [709.0]	447 [450.7]	1510
5	0 [4.5]	42 [60.4]	72 [72.8]	320 [301.0]	411 [406.3]	845
Σ	103 [103.2]	489 [488.8]	459 [459.6]	1429 [1428.5]	1017 [1016.9]	3497

	Danish Sons					Σ
	1	2	3	4	5	
	1	2	3	4	5	
1	18 [18.4]	17 [16.4]	16 [14.4]	4 [6.6]	2 [1.2]	57
2	24 [34.5]	105 [80.8]	109 [116.4]	59 [72.2]	21 [14.2]	318
3	23 [18.8]	84 [103.1]	289 [258.7]	217 [234.9]	59 [56.5]	672
4	8 [5.8]	49 [47.5]	175 [191.8]	348 [327.4]	198 [205.4]	778
5	6 [1.5]	8 [14.5]	69 [75.0]	201 [188.3]	246 [250.7]	530
Σ	79 [79.1]	263 [262.3]	658 [656.3]	829 [829.5]	526 [527.9]	2355

* Expected cross-classifications in brackets.

of sons to diagonal cells. This suggests a principle of class inheritance whereby there is a tendency, net of the broad considerations of labor quality, for sons to take jobs similar in kind to those held by their fathers. The presence of such a rule is testified to in much of the literature and we are neither surprised nor dismayed to observe its influence here.

We obtain an indication of how the pattern of deviations stands with the effects of status inheritance taken into account if we adjust the diagonals to the observed values and proportionately reduce the other expected values in each row to make up the difference. The value of χ^2 in the adjusted table stands at 27.8 with 20.0 points due directly to four cells in the lower left-hand

corner of the table. Specifically, the expectation involves the allocation of too many sons of origin classes three and four to the highest destination class and the assignment of too few sons of origin class five to the two highest destination classes. It is of interest that these errors occur at the extreme tails of the densities of these three origin classes, involving only the upper 2% of sons of classes three and four and the upper 5% of class five. This pattern arises in other applications and it possibly reflects limitations in the flexibility of the beta density near its tails. If this is the case, it implies that, insofar as the beta function fails to fit the data, the errors will be felt differentially at the extremes of the distribution. From a practical point of view, this

means that substantive inferences relative to the distribution of labor quality will be weak to the extent that they are based on the extremes of the estimated densities.

The lower panel of Table 2 shows the observed and expected cross-classification of Danish sons (Svalastoga, 1959). The χ^2 statistic for goodness of fit is 31.8 and so the deviations are not accounted for by random error. The smaller χ^2 statistic can be explained since the size of the Danish sample is smaller than that for British sons. A rough index of the seriousness of non-random error, taking account of sample size, is the ratio χ^2/N . The value of the ratio is .014 for the Danish table and .012 for the British table, indicating that systematic errors of fit are present to approximately the same degree in both cases. To obtain agreement between the observed and expected values of the Danish table, it would be necessary to reallocate 4.8% of the sons. The fact that this proportion is greater than that observed in the British table can be largely explained by the difference in sample sizes. The bulk of the misplaced cases (75.7%) involves a shortfall of predictions on the diagonals and here, as in the British table, there are some serious discrepancies in the proportions at the extreme tails.

Social Mobility in Britain and Denmark

The occupational categories of the British and Danish tables are similarly defined and so it is reasonable to compare the two. The estimated within-class densities of labor quality of the five origin classes of British and Danish sons are displayed in Figure 2. Insofar as the relative quality of sons may be approximated by beta curves, this set of graphs provides a picture of the way in which a son's origins affect his chances of occupational attainment in the two societies. In addition, it should be noted that in both populations, the locations of the graphs are strictly ordered from right to left in descending order of the social rank of the origin class, an outcome which reflects the well-documented tendency of sons of high status origins to compete successfully for the more preferred occupational positions. We also note

that there is less overlap in the densities of the three highest classes in Britain than there is in Denmark, and this indicates that a British son's competitive advantage is more influenced by his father's position *within* the middle class than is the case for a Danish son. This result is consistent with Goodman's (1969) finding of discrepancies in the relations between sons' and fathers' statuses in the three upper classes of the two populations. Insofar as readers accept our formulation of the mobility process, the graphs of Figure 2 will provide them with a description of the nature of this discrepancy.

The simultaneous visual interpretation of ten graphs is cumbersome and can lead to confusion. Accordingly, we move to measures which extract the most important information relating to these densities. Some basic summary statistics are provided in Table 3. The parameters a_1 and b_1 , while of little interpretive use in themselves, may be used to construct the moments of the densities. For the mean and standard deviation, respectively, we have

$$\mu_1 = \frac{a_1 + 1}{a_1 + b_1 + 2} \quad \text{and}$$

$$\sigma_1 = \sqrt{\frac{(a_1 + 1)(b_1 + 1)}{(a_1 + b_1 + 2)^2(a_1 + b_1 + 3)}}.$$

If we take weighted averages of μ_1 and σ_1 , using the size of the origin classes as weights, obtaining $\bar{\mu}$ and $\bar{\sigma}$ and proceed to construct the "standard scores,"

$$z_1 = \frac{\mu_1 - \bar{\mu}}{\bar{\sigma}}$$

for the two populations, we have a one-dimensional scale of the location of the densities in terms of a common unit variance.

The standard scores, shown in Table 3, indicate a slightly greater range (2.41 *versus* 2.22 standard units) in the average quality of British as against Danish sons of the five origin classes. We also note that the highest origin class stands well above the others in Britain though not in Denmark, but that differences in the location of the three lower classes are greater in

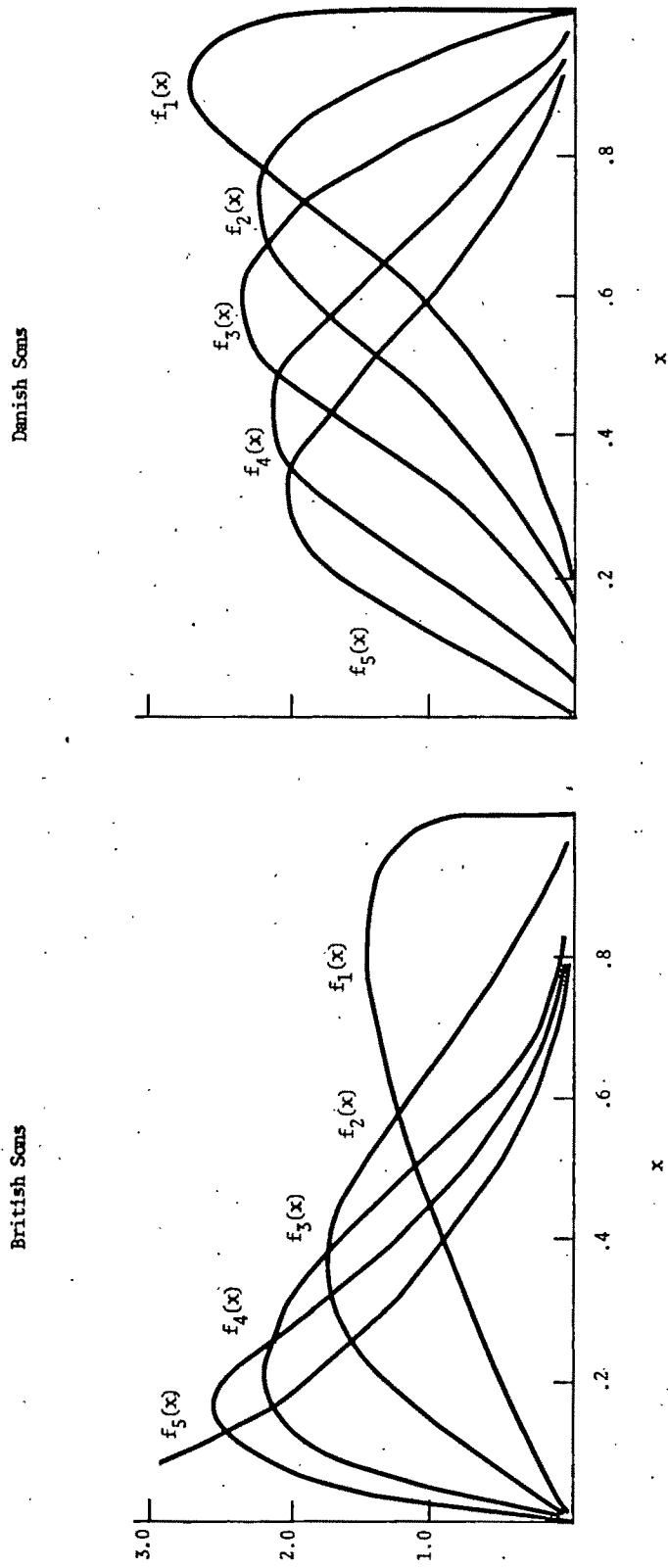


Figure 2. Estimated Within-Class Densities of Status-Related Advantages for British and Danish Sons

Table 3. Selected Parameters of the Within-Class Densities of Status-Related Advantages for British and Danish Sons

	British Sons Origin Status (i)					Danish Sons Origin Status (i)				
	1	2	3	4	5	1	2	3	4	5
Beta Parameters										
a_i	.82	.92	.83	.64	-.06	3.35	3.79	4.09	2.52	1.42
b_i	.14	1.75	3.11	3.87	2.86	.34	1.38	2.51	2.80	2.84
Derived Parameters										
μ_i	.615	.411	.308	.252	.196	.764	.668	.592	.481	.387
σ_i	.244	.207	.175	.158	.165	.164	.165	.159	.173	.181
z_i	1.91	.74	.14	-.18	-.50	1.41	.85	.13	-.25	-.81
Pairwise Comparisons of Within-Class Densities										
Π_{11}		.734	.833	.878	.910		.672	.782	.878	.928
Π_{21}			.645	.725	.796			.634	.781	.867
Π_{31}				.593	.695				.681	.798
Π_{41}					.618					.647

Denmark than in Britain. Thus, while there is no great overall difference in the relative advantage of sons originating in the upper strata as against the lower strata of the two populations, a son's origins *within* the upper strata is of greater significance in Britain, whereas one's position *within* the lower strata is of greater significance in Denmark. Thus, if these rules of allocation operate in societies in which the larger number of sons derive from middle-class origins, the Danish rule will indicate more fluidity. If, as is in fact the case in both societies, sons are predominantly of lower-class origins, the British rule will imply less dependence of son's position on that of his father and, thus, greater fluidity.

The standard scores have the advantage of scaling the mean values of quality on a single dimension and of permitting comparisons to be expressed in a familiar statistical language. On the other hand, there is some coarseness to this approach in that the means are not taken from normal populations of similar variances as the procedure assumes. As a consequence, probabilistic statements based on normal theory cannot be made on the basis of these standard scores.

We have a simple summary measure which is free of this limitation if we consider the likelihood that a son from one specified origin class will exceed in quality a son of some other specified origin class. This measure is taken as

$$\Pi_{ij} = \int_0^1 f_j(x) \left[\int_x^1 f_i(x) dx \right] dx \quad (15)$$

and it may be calculated numerically if the densities, $f_i(x)$ and $f_j(x)$, are known. The measure requires no distributional assumptions and it is fully independent of the marginals. Also, since Π_{ij} is simply the probability that a son selected at random from the i^{th} origin class will exceed in quality a son selected at random from the j^{th} origin, it has clear substantive relevance.

Insofar as the estimated densities reflect the actual joint distribution of status-related advantages, the values of Π_{ij} furnish relatively refined information regarding the implications of social origins for occupa-

tional achievement. For example, the value of Π_{24} in the British table indicates that in Britain the probability that a son of the second origin class will exceed in quality a son of the fourth origin class is .725. The corresponding probability for Danish sons is .781. Thus, the advantage held by a son of class two over a son of class four is considerable in both populations and more strongly so in Denmark. An examination of the entries in these matrices indicates that the competitive advantages accruing to sons of the higher origin classes is substantial in both societies, with the likelihood that a son of a specified origin class will stand in a more favorable position than a son originating in the class just below—see the superdiagonal entries—being generally near two-thirds. Contrasting the extremes of origin status, we find that the probability that a son of the first origin class will exceed in quality a son of the lowest origin class to be .910 in Britain and .928 in Denmark. It should be noted that these advantages do not translate automatically into important differences in social position. Large differences in status occur when the status system is highly differentiated and, in general, differences in the characteristics of individuals in the society will not, in themselves, assure differentiation in the social system. In other words, status-related advantages will be parlayed into status itself only insofar as the requisite opportunities are available. However, in the comparison at hand, we have two highly differentiated societies in which the distribution of social rewards is strongly conditioned by occupational position.

A comparison of the two Π_{ij} matrices reveals that, in general, an entry in a particular cell of one matrix is near in value to the corresponding entry of the other matrix. Thus, there is no evidence of extreme differences in the populations in the distribution of status-related advantages between origin classes. On the other hand, there are some differences of sufficient numerical significance to warrant some comment. Taking comparisons among the three highest origin classes, we note that the implications of social origins for a son's

competitive standing are generally greater in Britain than in Denmark. This is due largely to the strong position of British sons of class one. Thus, Π_{12} and Π_{13} are .734 and .833, respectively, in Britain and they are .672 and .782 in Denmark. The discrepancy is small, however, with reference to the comparison of class two with class three, the value of Π_{23} being .645 for Britain and .634 for Denmark. A comparison of the three lower classes reveals that a son's origins here are of greater significance in Denmark than in Britain. Thus, the Danish values for Π_{34} , Π_{35} and Π_{45} all exceed the corresponding British values. In general, we find here, as we did in the comparison of standards scores, that origins within the upper classes are of greater significance in Britain whereas origins within the lower classes are of greater significance in Denmark.

The fit of the model to the data is imperfect, and so it is necessary to consider the implications of the deviations for the interpretation of these tables. In general, there are two factors which can account for the lack of fit. Thus, we will expect errors insofar as the beta family is not adequate to capturing the ordering of quality and we will expect errors to the extent that status-related advantages do not collapse to a single dimension. We will focus on the second of these factors because multidimensionality has important substantive implications and because there is evidence that it is present in those data.

Human mobility is a complex process involving a variety of motives and contingencies and it is, therefore, unlikely that any outcome will be fully accounted for in the rank value of sons and jobs. Thus, insofar as sons of a particular origin class have preferences and opportunities for a specific sort of work and these do not conform to the general ordering of jobs and labor quality, the assumptions of the present model will be violated and errors in fit should be anticipated. The existence of such effects does not undermine the basic thrust of our formulation, though it does indicate its limits. On the other hand, the present model has the advantage of providing a reasonable standard against which

one can test for the presence of other effects. As an example, consider the case of strict status inheritance. In both populations, the most obvious feature of the pattern of deviations is an excess of the observed over expected values in the diagonal cells. This suggests a tendency, net of considerations of quality, for sons to take on positions similar to those of their fathers. While this outcome is not surprising since status inheritance has been alluded to repeatedly in the analysis of mobility tables, the evidence of a strict affinity of sons' and fathers' statuses is more convincing when refracted through this model than it is in the context of most analyses.

Under typical procedures, ratios of the observed values in the diagonals to those expected under independence are constructed. Some measure of status inheritance is presumed to be present if, as is almost always the case, the ratios are in excess of unity. The weakness of this and other approaches based on a standard of statistical independence is that they contrast the outcome to the expectation which would hold if sons of all origin classes were similar with respect to their distributions of status-related advantages. If, as it is reasonable to assume and as our empirical results indicate, the quality of sons is positively related to the standing of their origin class, it will require no particular affinity of sons' and fathers' statuses for the observed diagonal values to exceed those expected under independence. Thus, in each of the three illustrative cases of Table 1, the "observed" diagonals exceed the expected values, though quality alone and not strict status inheritance is a consideration in the construction of the illustrations. On the other hand, the presence of strong positive deviations in the marginals, after considerations of quality are taken into account, as in Britain and Denmark, is evidence that the phenomenon of strict status inheritance is not spurious.

Structural Influences on Occupational Mobility

The relative competitive situations of sons of various origins is, under the present perspective, but one of two factors giving

rise to the mobility experience of a population. The outcome also is conditioned by the market within which competition takes place. Thus, as our introductory illustrations indicated, the marginal distributions of statuses in the two generations as well as the differential allocation of status-related advantages figure into the observed outcome. A quite similar distinction, viz., "structural" as against "exchange" mobility, is currently routine in the literature. Structural mobility is defined as the minimum number of interclass moves which must take place given the observed intergenerational shift in the composition of the labor force and operationally this is equivalent to the minimum number of sons who will be located off the diagonal if the marginals are to agree with the data. Exchange or pure mobility is simply the amount of mobility which is in excess of the structural component and it is measured as the difference in the sum of the off-diagonal values and the value of structural mobility (see Yasuda, 1964; Boudon, 1973: 17-39).

The classical formulation is not theoretically satisfying. Thus, structural mobility is defined as number of moves which would occur if mobility in the society were due to structural factors alone or if exchange mobility did not occur. If, as is always reported, there is mobility in excess of the structural component, some less radical rule of status inheritance must, in fact, be operating. Also, since exchange mobility is estimated by subtracting structural from observed mobility, it involves a mixing of the prevailing rule of allocation with an extreme standard. Accordingly, a logically consistent estimate of how much mobility was actually induced (or averted) by changes in the composition of the labor force and how much would have occurred under static conditions is not furnished under these procedures.

If an estimate of the operating rule of labor allocation is available, a meaningful evaluation of the amount of mobility induced by shifts in the occupational structure may be obtained by comparing the number of moves which occur in the face of the actual generational differences in labor force composition to the number of

moves that would be expected given the prevailing rule of allocation and a marginal distribution of sons' statuses similar to those of their fathers. In application, this involves moving the cutting points, x_j , to the positions which will yield a set of destination classes similar in size to the set of origin classes. The operational procedure here is the same as that described above in reference to illustrative cases.

The expected cross-classification corresponding to the static case is displayed in the left-hand panel of Table 4. The sum of the off-diagonal entries in Table 2 is 2097.4 and in Table 4 this sum is 2087.1. In other words, assuming the prevailing rule of labor allocation is described by the estimated within-class densities of labor quality, the net effect of differences in the class distributions of British sons and fathers is to place about ten sons in statuses other than their fathers'. Our estimate is subject to qualification in that the derived, rather than the observed, cross-classification of statuses is put forth as the estimated allocation associated with existing labor requirements. This is a consistent approach in that the same rule is used to generate the two allocations which are compared, but it is limited in that the effects, under varying labor needs, of factors not accounted for by the estimated densities are neglected. However, we see no convincing reason why these factors should be expected to have any strong systematic influence.

Our result contrasts sharply with the 192 moves of British sons which, under classical methods, would be attributed to structural changes. This relatively large number is mainly a result of the fact that these methods include in the structural component any move that shifting labor requirements would require, and among these there are many which would have taken place under static conditions. Somewhat less obvious, but equally relevant, are those moves which would take place in a static context but which do not occur in the context of actual labor needs. We have, for example, an expected 438.7 British sons of origin class five in other destination classes given the generational change in the class distribution and an expected 485.4 in other

Table 4. Expected Cross-Classifications of Statuses Given Static Distributions: British and Danish Sons

Origin Status	Destination Status				
	British Sons				
	1	2	3	4	5
1	53.2	39.0	15.0	18.4	3.4
2	47.1	146.5	97.8	165.3	38.1
3	11.0	90.2	96.9	240.0	80.0
4	11.0	156.4	225.1	753.7	363.8
5	6.7	62.8	83.2	332.6	359.6
	Danish Sons				
	1	2	3	4	5
	1	2	3	4	5
1	15.2	20.6	14.0	6.0	1.2
2	24.9	97.5	114.8	66.2	14.5
3	12.3	123.6	261.7	216.9	57.5
4	3.7	58.3	201.4	308.9	205.7
5	.9	17.9	80.1	179.9	251.1

destination classes under static conditions. Thus, excepting the case in which a strict caste rule is present, mitigated only by changing labor needs, the classical measure of structural mobility is likely to be a gross overestimate of the net number of status changes arising as a result of structural shifts.

The net effect of structural influences on the mobility of Danish sons appears to be trivial. The expected number of status changes given the differing labor force distributions of the two generations is 1419.0 and under static conditions the expected number is 1420.6. In other words, an expected *deficit* of 1.6 status changes occurred as a consequence of the dissimilarity in the occupational distributions of fathers and sons. By contrast, an application of classical methods leads to the assignment of 73 moves to the structural component.

Conclusion

A table of social mobility describes the outcome of an allocation process. The rules which govern the process are not described in the table and so any interpretive statements relating to the outcome will require assumptions about the nature of the allocation process. The model employed here assumes that sons may be rank ordered on a single dimension and that they are allocated over a series of ordered destination

classes on the basis of this ranking. The distribution of sons along this dimension is expected to vary according to origin class and, under the present formulation, these distributional differences account for the observed variation in the proportionate allocation over destination classes of sons of differing origins. An *a priori* specification of the nature of the within-class distributions is an operational requirement of the model. In the present application, these within-class densities are assumed to be members of the beta family. However, the basic conceptual framework does not depend on the choice of the beta family.

This model bears some similarity to that proposed by Levine (1972) in that both approaches treat the status categories of the table as intervals on a status continuum. Levine's approach differs from that presented here in that his allocation is oriented in terms of the distance between the statuses of fathers and sons. The cell values of his reconstructed table are developed in terms of the distance (on an underlying status continuum) of the cell from the diagonal and the constraints of the marginal distributions. He applies his model to Glass's (1954) mobility table and obtains an expected table which fits the data more closely than the reconstruction obtained under the procedures developed here. Goodman (1972) proposes a series of models which also fit these data well. His models, like that of Levine, accord special weight to the diagonal values and, in varying ways, they weight the other cells in terms of their distance from the diagonal.

The tendency of sons to hold positions similar in status to those of their fathers is a longstanding empirical generalization. Levine's model and those proposed by Goodman explicitly incorporate this phenomenon and so, to some degree, their formulations are post hoc. The present model does not incorporate a principle of status inheritance or take account of the distance between origin and destination

classes. Nonetheless, a quite respectable reproduction of observed mobility tables, including their diagonal elements, is obtained. This suggests that much observed status inheritance can be explained without recourse to a principle whereby sons have an affinity for statuses similar to their fathers. A model is powerful insofar as it can be disproved, and models which cater to the most striking features of observed data lack this property. In summary, our results indicate that much, though not all, of the observed tendency of sons to take on positions similar to those of their fathers can be explained without taking explicit account of this tendency in the modeling of the data.

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SEX ROLES AND INTERPERSONAL BEHAVIOR IN TASK-ORIENTED GROUPS *

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In the past, sociologists have proposed that men and women approach situations in which they have to work with other people differently; that men are "task" or "instrumental" specialists, while women are "social" or "expressive" specialists. Subsequent advances in research on the social psychology of small groups, on families, and on personality has largely removed the theoretical and empirical supports for this proposition. On the other hand, researchers continue to observe sex differences in behavior in a variety of task-oriented situations. This paper suggests that sex roles may be seen as the result of status processes. Since men have higher status than women, men are expected to be more competent than women and it is expected that competitive or dominating behavior is legitimate for men but not for women. Empirical studies of sex roles as related to task appropriateness, group problem solving, conflict, dominating behavior and role expectations are reviewed in support of this theory.

The subject of this paper is sex role expectations as they affect interpersonal behavior in task-oriented group situations. For a number of years, the prevailing theory in sociology was that men are "instrumental" or "task" specialists, and women are "expressive" or "social" specialists. This hypothesis, which has found its way into introductory and family sociology textbooks and which has been used *post hoc* to explain unpredicted sex differences in empirical research, was derived from the more general theory of interpersonal behavior of the Bales tradition of research on small groups (Bales, 1949; Parsons et al., 1953; Slater, 1955).

According to the general theory, task behavior (which is primarily in the "attempted answers" categories of the Bales coding system, suggestions, opinions and evaluations) and positive social behavior (primarily in the "positive reactions" categories, solidarity, tension release and agreement) are incompatible, but both are essential to a viable small group. Initially,

the theory was that it is difficult for one person (male or female) to engage heavily both in task and in social activities. Since both activities are necessary, a pattern of *role differentiation*, in which a group has one "task leader" with higher rates of task behavior than other group members, and a different "social leader," who has higher rates of positive social behavior than other group members, was hypothesized to be a universal feature of viable small groups.

The *sex role differentiation hypothesis* was derived from this more general role differentiation hypothesis (Bales and Slater, 1955; Zelditch, 1955). The reasoning was that, since families are small groups and all small groups have role differentiation, families must have role differentiation. Because of the biological factors of age and sex, fathers are best suited to be task leaders and mothers are best suited to be social leaders. Furthermore, the task/social distinction is a fundamental aspect of personality, established through socialization processes such as identification with same-sex parent and differentiation from opposite-sex parent.

The sex role differentiation hypothesis is appealing because (1) it is derived from more general principles of human behavior (role differentiation) and (2) it makes

* Theoretical and empirical research papers by several students have greatly aided our thinking on the subject of sex roles and interpersonal behavior, although they may not all agree with our conclusions. These students are: Denise Abrams, Sol Fulero, Tom Patterson, Gary Peck and Roger Reitman.

predictions for human behavior at several levels: personality, behavior in families, behavior in non-family groups, and entry into occupations (women enter "people-oriented" or "helping" professions). Subsequent research, however, provides little support for either the general principle of behavior or the specific predictions. After a brief review of research directly related to the sex role differentiation hypothesis, we will propose an alternative theory and review in more detail some research relevant to it.

Evidence on Role Differentiation: Incompatibility of Task and Social Behavior in Small Groups Generally

Attempts to test the hypothesis that task/social role differentiation is a universal property of small groups have shown that, by no means, do all apparently viable small groups have the expected differentiation pattern. Lewis (1972) and Bonacich and Lewis (1973) have concluded that the data from neither the original Bales studies nor from a number of more recent studies provide strong support for the idea that task and social roles are incompatible. Davis (1961) found that role differentiation was unrelated to the viability of a sample of discussion groups, and other researchers have concluded that the appearance of a role differentiation pattern is related to such factors as the degree of task orientation of the group members (Mann, 1961) and the legitimacy of task activities (Burke, 1967; Turk, 1961).¹

¹ Later work by Bales (1970) revised the notion that task and social behaviors are mutually contradictory dimensions of social behavior and suggested instead three orthogonal dimensions. One reason for the revision was the difficulty of reconciling two "functions" of the social leader, both of which were implied by the original formulation. On the one hand, the social leader was hypothesized to provide to the rest of the group the social rewards necessary to keep them feeling part of and committed to the group; this function emphasizes equalitarian interaction. On the other hand, the social leader supports the task leader in his directive and controlling activities and mediates between him and the rest of the group; this function emphasizes hierarchical, status-differentiating behavior. Discussions of sex role differ-

Several types of research based on theories and methods different from the Bales tradition have reached similar conclusions; that is, that the types of behavior or social structures that create social satisfaction are incompatible with the behaviors and group structures that lead to task performance under some conditions but are compatible under others. One such body of research is the study of communication networks (see, for example, Shaw, 1964; Glanzer and Glaser, 1961; Moore et al., 1972). Another is the work of Feidler and his associates on leadership style and group effectiveness (Feidler, 1964; 1967). Both of these areas of research suggest, as did the studies derived from Bales' work, that factors such as the demands of the task and the legitimacy of the group's status structure influence the degree of compatibility of task and social behaviors and the actual distribution of these behaviors in a small group.

Role Differentiation in Families

Research on families indicates that sex role differentiation is no more universal than role differentiation in general. In an early study, Zelditch (1955) had presented evidence from the Human Relations Area Files that sex role differentiation in families occurs in a significant number of cultures. Aronoff and Crano (1975), however, have concluded that cross-cultural evidence shows that, in a large number of subsistence economies, women contribute as much or more than men to family productive activities. This casts some doubt on the ubiquity of male task specialization in families. In studies of American families, Strodbeck (1958) and Kenkel (1957) have reported observing sex role differentiation, while Levinger (1964) and Leik (1963) did not. Leik's (1963) study also provides some insight into the relationship between family and non-family groups. He observed mothers of college-aged daughters to have low

entiation generally have assumed that it is the former aspect of social specialization that characterizes the feminine role; we argue that for most situations it is the latter.

rates of task behavior and high rates of positive social behavior when they were interacting with strangers (in both same-sex and mixed-sex groups) but high rates of both task and social behavior with their own families. March (1953) observed that the role differentiation in a set of married couples varied with the discussion topic. Heiss (1962) reported more sex role differentiation in pairs of strangers than in dating and engaged couples; as in Leik's (1963) experiment, this was because the women had higher rates of task behavior in the more intimate groups. In at least one study (Straus, 1967), fathers were observed to be predominant in both instrumental and expressive spheres.

In general, research literature on families (see, for example, reviews of literature by Collins and Raven, 1968; Waxler and Mishler, 1970) show that dominance, instrumental and expressive behaviors in families are often more variable than in other groups, depending on such things as the task, conditions under which the family is observed, situational factors such as sex of child present, and stage of family cycle. Rather than being, as many sociologists have assumed, the ideal place to study sex roles, families (at least contemporary American families) seem to be one of the worst places to get information about sex role expectations and behavior.

Sex Role Differentiation in Non-Family Groups

When we look for direct tests of the sex role differentiation hypothesis in groups other than families, we find that this hypothesis generally has been invoked *post hoc* to explain unpredicted sex differences rather than tested explicitly. In an early and influential study, Strodbeck and Mann (1956) had applied the task/social role differentiation hypothesis to sex roles in mock jury discussions. They observed that men were more likely to be highly active, to be chosen as valuable to the group by other members and to initiate acts in the attempted answers categories. Women jurors who were active were more likely than men to initiate acts in the positive reac-

tions categories. The Strodbeck and Mann data also showed that contributions by men were more valued than those by women (that is, being highly active was associated with being named as valuable for men, but not for women).

Most subsequent studies of the sex role differentiation hypothesis have dealt with families rather than with non-family groups. One exception is a paper by Eskilson and Wiley (1975), who tested a role differentiation hypothesis in *ad hoc* three-person groups of college students, with leaders selected by the experimenters. They reported no overall differences between men and women leaders in task contributions but higher levels of positive social behavior for women. They also reported that leadership behavior of men and women varied differently with the sex composition of the group and the rationale for selecting the leader (on the basis of competence or randomly). There is a large body of research literature not based directly on the sex role differentiation hypothesis but which also documents sex differences in task-oriented groups. Some of this research is examined later in this paper.

Sex Differences in Personality

A recent review of personality research on sex differences (Maccoby and Jacklin, 1974) concluded that many previously reported sex differences are not reliable; they appear in some studies and are contradicted by others. Maccoby and Jacklin have concluded (1974; chs. 6, 7) that girls generally do not seem to be more motivated toward "social" behavior than boys, nor less motivated to "instrumental" types of behavior. Existing empirical studies do not show girls to be reliably more sensitive to social cues, more dependent, more affiliative, more nurturant, more altruistic or more empathetic than boys. Maccoby and Jacklin (1974:149) have also noted that "males do not appear to have generally greater achievement motivation, although they may show more arousal of this motivation under directly competitive conditions. The task-orientation vs. person ori-

entation distinction seems to be a poor one from the standpoint of understanding achievement motivation of the two sexes."

A further problem with acquiring information about sex roles from personality studies is that the relationship between personality, as measured by verbal tests, and actual interpersonal behavior is often different for men and women. Many personality tests seem to predict behavior of men better than behavior of women. Some specific examples of personality characteristics that are reported to predict behavior for men but not for women include Machiavellianism (Christie and Geis, 1968), self-esteem (Eisen, 1972) and various "needs" as measured by the TAT (Sechrist, 1968). McGuire (1968:1166) has commented that "this finding of lower correlations for women than for men is not peculiar to the personality-influenceability area. Part of the lore of laborers in the experimental-personality vineyard is that if one wants to find strong relationships between personality variables and behavioral measures in college sophomores, one is wise to use male rather than female subjects."

Sex Differences in Occupational Choice

Occupational choices and the reasons for making them sometimes are given as support for the task/social sex role differentiation hypothesis. In fact, women have been found to be more people-oriented and less oriented to material rewards in the values they attach to occupations (Davis, 1963). However, patterns of occupational choice are affected by perceptions of the alternatives available, and the occupations available to women have generally been those with less reward in terms of money and prestige than those available to men. Also, many women have been able to consider the option of not working at all. Thus, the major reason for a woman to choose an occupation would logically be that she does value the rewards it has to offer, i.e., people-oriented activities rather than pay or prestige. There is evidence (Turner, 1964) that women who place a high value on material ambi-

tion expect to satisfy that ambition through their husbands' occupations, not their own. Among the Los Angeles high school girls who answered Turner's (1964) questionnaire, it was the ones who were materially ambitious but did not expect to work, not the career-oriented girls, who agreed with statements endorsing individualistic and instrumental behavior (for example, showing people how to argue intelligently versus trying to smooth over disagreements or trying to get a group to do things my way versus being quick to go along with group). Since occupational choices are made from among different sets of alternatives for men and women, it is difficult to use such choices, or the reasons for them, to infer different personality or value structures. The fact that persons faced with different sets of alternatives made different choices cannot be used as evidence either for or against the existence of different preferences.

It appears, then, that the dimension of task versus social orientation is no longer justified as an explanation of sex differences in behavior in task groups. In the first place, there is no evidence that social behavior and task behavior are universally incompatible, although there are some types of tasks and status structures for which this is true. In the second place, there is no reliable evidence that women are more motivated than men to maintain harmonious social relationships.

On the other hand, differences in behavior between men and women have been observed. Some of the sex differences that have been documented in the studies just cited include: higher rates of task behavior by men than women in some settings but not in others (Leik, 1963; March, 1953; Heiss, 1962); higher rates of positive social behavior by women when their rates of task behavior are comparable to those of men (Leik, 1963; Eskilson and Wiley, 1976); apparent lower evaluation of women's than of men's task contributions by women as well as men (Strodtbeck and Mann, 1956). The circumstances that were associated with high rates of task behavior by women included being with family or fiancé as opposed to being with

strangers (Leik, 1964; Heiss, 1962) and being appointed to a leadership position by an outside authority (the experimenter) (Eskilson and Wiley, 1975). We suggest that these as well as other research results (reviewed later) can be explained by a theory of status processes rather than task/social role differentiation. The theory comes from two sources: performance expectation theory and status consistency theory.

Status Characteristics and Expectation States Theory

This theory, presented by Berger, Cohen and Zelditch (1973) and Berger, Conner and Fisek (1974), proposes that the power and prestige order of a task-oriented group, as observed by differentiation of the distribution of participation, prestige and influence, is determined by initial differences in external status. The mechanism through which external status affects internal status is the establishment of performance expectations. When group members perceive that external status differences exist, the higher status person is, in the absence of information to the contrary, assumed by self and others to be more competent at the group's task. This means that his task contributions are more valuable than others' task contributions. If the group members want good task performance, they want the most valuable task contributions they can get. Thus, the person for whom they hold higher performance expectations gets more opportunities to participate, initiates more actions, receives more positive reactions and has more influence. The establishment of performance expectations on the basis of external status characteristics takes place even when the status characteristics are not previously associated with ability at the specific task. The process will not operate only if information is provided to, and accepted by all group members that the status characteristic is not relevant. This last proposition is referred to as the "burden of proof" assumption, because it says that a lower status person must prove competence while a higher status person need not. Carefully controlled experimental studies, using a variety of status char-

acteristics, provide support for the assumptions of the theory and various predictions derived from it (Berger et al., 1974). The hypothesis that, if information that a lower status person is competent is provided, performance expectations are established on that information rather than external status has also received support (Cohen and Roper, 1971; Freese and Cohen, 1973). The theory has been applied to sex as a status characteristic by Lockheed and Hall (1975), who concluded that sex does operate like other status characteristics—men having higher status than women.

For our purposes, these assumptions can be summarized in the following:

- Statement I—A. Group members for whom there are higher performance expectations will receive and take more opportunities to make task contributions, will have more influence and more prestige and will receive more expressions of agreement and approval than those for whom there are lower performance expectations.
- B. In the absence of information to the contrary, group members will assign performance expectations to self and others on the basis of external status characteristics.
- C. Sex is a status characteristic; males have higher status than females.

Certainly competence is one of the primary factors affecting the legitimacy of task contributions in a group. Other factors also related to external status may be hypothesized to be important. The second part of the theory deals with some of these.

Status, Competition, and Legitimacy

This part of the theory, unlike the expectation states model, is not drawn from a precisely formulated and experimentally tested theory. It comes from a variety of sociological theories involving social exchange, status, distributive justice, and legitimacy. We may first note that even in a cooperative task-oriented group, there is a mixture of competitive and cooperative

elements. When one individual makes a valuable task contribution, he or she is simultaneously helping the other group members toward a goal they value and enhancing his or her own power and prestige at the expense of the other group members. Blau (1964, especially chapters 3 and 5) has developed this argument from an exchange theory point of view. Kelley and Thibaut (1968) proposed a similar argument based on considerations of interpersonal trust and the attribution of intentions. That is, in deciding whether to accept and encourage task contributions by another person, group members must decide not only whether the contribution is valuable to the group, but whether it is motivated by cooperative (group) or competitive (individual status) considerations.² Kelley and Thibaut (1968:38-41) also have cited a number of empirical studies that show that task contributions are less accepted when they are, either in fact or as perceived by group members, motivated by desire for competitive status enhancement rather than the good of the group. Hollander and Julian (1970) have reported that (male) subjects will accept leadership from an incompetent person who shows a sincere interest in the group's activity, even though competence is the most important source of legitimate authority.

This argument is closely related to status consistency theory (Homans, 1961: ch. 12; Adams, 1965; Sampson, 1963; 1969) which suggests that, in addition to establishing performance expectations, external status characteristics establish the legitimacy or "justice" of the power and prestige structure of a group. Persons with high status outside the group "deserve" higher status within the group and are allowed to earn status by making contributions which are accepted by others. A person with low

status, however, who attempts to make task contributions may be assumed by other group members to be acting from motives of competitive status enhancement, which is not legitimate since he or she does not "deserve" higher status. Applying this to sex role expectations means that before task contributions from a woman are expected or accepted, there must be evidence either that she is cooperatively motivated or that it is legitimate for her to enhance her own status. For men, the role expectation is that competitive status enhancement is legitimate. We hypothesize that these expectations are held by both men and women, for both same-sex and mixed-sex settings.

This part of the theory can be summarized as follows:

- Statement II—A. In the absence of information to the contrary, a task contribution by one member of a task-oriented group which is accepted by others will be assumed by both self and others to raise the status of the contributor relative to the status of the others.
- B. (1) Raising one's own status relative to the status of others is legitimate for persons with high external status but not for those with low external status.
- (2) Since men have higher status than women, raising one's status relative to status of others within a small group is legitimate for men but not for women.
- C. The "information to the contrary" referred to in Statement II—A may be (1) information that a person is motivated to help others in the group rather than to raise his/her own status or (2) that the person legitimately has been assigned a higher status than others (for example, been appointed leader by an outside authority).
- D. Acts that will result in raising one's own status relative to others will be expected from persons for whom it is legitimate to raise their

² This is just as rational as taking task competence into account, since a person with good intentions, even though not competent, may be assumed to be willing to take suggestions from people who are competent and to distribute the rewards fairly. A person who is motivated by competitive status considerations, even though competent, would be likely to keep others from maximizing their rewards from the group effort.

status relative to others; such acts will not be expected or accepted from persons for whom it is not legitimate.

- E. These statements hold independently of performance expectations.

This, in combination with Statement I, suggests that women must prove that they are both competent and well-intentioned before either they or others expect or accept high levels of task behavior from them, whereas men need prove at most one of the two. Statement I will help to explain why women in many mixed-sex settings have lower rates of task activity than men, are less influential and are less often perceived as making valuable contributions when they do contribute. It also may help explain some of the higher rates of positive social activity of women, since one of the features of a power and prestige order is that lower status persons do more agreeing and expressing of approval relative to the number of task contributions they make than higher status persons (see, for example, Hurwitz et al., 1968; Lippit et al., 1958). It may be that the relatively high rates of positive social behavior that have been observed for women are also related to the necessity (Statement II—C(1)) of proving good intentions. Through providing expressions of approval, agreement and encouragement to others, women can prove that their task contributions are motivated by concern for the group and thereby legitimate task behavior.

This set of statements also will help explain why it is that women do not have lower rates of task behavior than men in all settings. If the external status characteristic of sex can be made to appear irrelevant to the task, or if the particular women involved can be made to appear competent, performance expectations will not be affected by sex. If, at the same time, high rates of task behavior are legitimated for women, sex differences should disappear.

Review of Some Empirical Research

The remainder of this paper presents a brief review of some of the many recent

empirical studies of sex differences in task-oriented group settings. Although many of these have been explained by some version of a task/social role differentiation model, most of them seem more consistent with differences in expectations for task competence and legitimacy of competition for status.

(1) *Sex appropriateness of task.* It may be suggested that women will be assumed to be more competent and put out more effort relative to men when working on tasks that are culturally defined as feminine: for example, cooking rather than cars. While there is some evidence for this (for example, Milton, 1959; March, 1953), we should also note that "feminine" activities generally are considered less prestigious than "masculine" activities. For example, a sample of college students, both male and female, who were studied by Spence and Helmreich (1972) expressed greater liking for a (hypothetical) girl who had masculine interests than one with feminine interests. Also, both men and women preferred a girl who was competent over one who was incompetent. Seyfried and Hendrick (1973) found a general preference for masculine role attitudes in a hypothetical person, male or female as well as a preference for a person who had sex-appropriate interests. These studies provide evidence that sex is a general status characteristic. They also suggest that varying the stereotyped sex-appropriateness of a task may not provide an adequate test of hypotheses relating status and sex, since the greater expected competence of women in feminine-appropriate tasks must be balanced against lower motivation of both men and women in these tasks due to lower prestige and expected reward from them.

Further insight into the effects of sex role expectations on expectations of task competence comes from some of the research reviewed by Maccoby and Jacklin (1974:154-6), which seems to indicate that females tend to express less confidence than males in their own future performance, even on tasks where females characteristically do as well or better than males or where subjects are using their own past performance as a standard.

(2) *Conformity*. Conformity or influenceability is a variable that has been studied extensively in task-oriented situations. When conformity is measured in the Asch situation and its relatives, females typically are reported to conform more than males (for reviews of this literature see Allen, 1965; McGuire, 1968). On the other hand, when conformity or influenceability is measured in a variety of other ways, these sex differences do not appear consistently (Maccoby and Jacklin, 1974:265-74). For both males and females, confidence in one's own ability at a task is related to low levels of conformity, and having high expectations for the ability of others is related to high levels of conformity (Cohen and Lee, 1975). It would appear that when women conform more than men, this is related to lower confidence in task abilities rather than higher motivation to be agreeable or to promote group harmony.

(3) *Group problem solving and motivation*. A number of studies of group problem solving have reported sex differences. Hoffman (1965), who reviewed this area of research, concluded that all-female groups generally do less well at problem solving than all-male groups but that sometimes mixed-sex groups are superior to either. He attributed the lower level of performance of all-female groups primarily to motivational differences, suggesting that males generally are highly motivated to solve problems and that females are not. Working with other females, he suggested, reinforced the women's rejection of the task while working with males increased their motivation and, hence, the amount of effort they expended. Mixed-sex groups were superior to all-male groups in cases in which competition between men in the all-male groups interfered with coordination.

Of particular interest is an experiment by Carey (1958) who measured male and female subjects' attitudes toward problem solving before and after a mixed-sex problem-solving effort, a discussion designed to instill positive motivation toward the task, and a second problem-solving session. The women's task performance improved after the discussion, but the men showed no

change. As Statement II suggests, in group problem-solving situations many women will not put out their maximum effort unless told to do so. Although men are more likely to put out maximum effort, the results may be competitive rather than cooperative.

(4) *Conflict studies*. Another body of research that documents sex differences in task-oriented situations comes from studies of conflict, coalition formation, and bargaining. Vinacke (1959) compared male and female same-sex triads in a coalition experiment and concluded that the sexes showed the same general pattern in that the two weaker members of a triad tend to ally when they can overpower the stronger, but form few coalitions when they cannot win by joining forces. In addition, he reported a tendency for women to form coalitions when none is necessary, not to ally when it is rational to do so, to form triple alliances, and to divide the prize equally. Uesugi and Vinacke (1962) reported that this "feminine" strategy is enhanced when the game is given a female-appropriate content and explained it as a result of women's greater "social" orientation. Caplow (1968:31-4), however, has shown that the conclusion of both studies that there is a "feminine" strategy is invalid, since the supporting data presented do not show statistically significant differences. The women's behavior in these studies is similar to behavior of men in other coalition studies in which the subjects turned out to be confused about the rules and conditions of the game.

Bartos (1970) reviewed several studies of negotiation behavior. He concluded that men were "tougher" negotiators than women; that is, men made fewer concessions and more demands. Subjects generally were tougher when negotiating against women than against men. In addition to being male, the "tough" negotiators tended to be young, of minority race and "poorly adjusted" as measured by the California Psychological Inventory.

Two-person, mixed-motive games, in particular the Prisoner's Dilemma, have been studied intensively for several years, and sex is one of the variables that has

been used in combination with others. In a Prisoner's Dilemma experiment, each member of a pair of subjects is presented with a choice or series of choices between two alternatives. The Cooperative alternative, if chosen by both, results in mutual gain; the Defecting choice, if chosen by one when the other Cooperates, results in a large gain to the Defector and a large loss to the Cooperator. Mutual choice of Defection results in mutual loss. There is, thus, a constant temptation to Defect even though the result is not desirable to either person. In fact, subjects in these experiments do tend to "lock in" to the mutual loss pattern. The usual dependent variable is described as the number of cooperative choices, although there may be other motives than a desire to cooperate or to compete behind the choices that a subject makes. It is the difficulty of attributing motivation to subjects in this situation that has made sex differences in mixed-motive games puzzling. In one of the earlier studies, Rapoport and Chammah (1965) found that female dyads consistently were more likely than males to lock in to the mutual defection pattern, which was interpreted as meaning that women were more competitive than men. While a number of later studies have replicated this, others have found the reverse or no sex differences. As the use of the Prisoner's Dilemma as an experimental paradigm developed, a number of variables were found to affect the level of "cooperation." Most of these seemed to produce an interaction with sex—variations tending to increase cooperation for one sex, but not the other. In general, women tend to be less "rational"; they respond less to act-by-act variations in choices made by the other person and are less accurate predictors of the other's behavior (e.g., Pilisuk et al., 1968). Women are also less likely to find and maintain an optimal strategy for maximizing their own rewards (e.g., Halpin et al., 1970) but are more responsive to other cues such as promises (Tedeschi, Powell, Lindskold and Gahagan, 1969; see also a review of a number of studies by Tedeschi, 1973) and attractiveness of the other subject (Kahn et al., 1971). Women have been found to be less

likely than men to maximize contributions to a group score even in a cooperative game in which neither person gains anything by not cooperating (Meeker, forthcoming; Meeker and Hornung, 1976). Women appear to be neither more competitive nor more cooperative than men.

Tedeschi, Shlenker and Bonoma (1973), reviewing a large number of conflict experiments, concluded that women are more extreme in bargaining behavior than men, responding more cooperatively to cooperation but more vindictively to exploitation. Terhune (1973), who reviewed essentially the same body of literature, suggested, as an explanation of the apparently less rational and more extreme behavior of women, the hypothesis that women tend to be more affiliative and dependent while men are more dominant and aggressive. Since coordination, directing and leading are dominance behaviors and are also skills necessary to achieve cooperation in the Prisoner's Dilemma, women are less likely to achieve cooperation. Withdrawal is a dependent behavior and, "in the Prisoner's Dilemma the only withdrawal possible is to the defection tactic which is usually interpreted as a conflict maneuver. It seems, consequently, that the greater conflict among women in Prisoner's Dilemma reflects not competitive orientations but, rather, less ability to establish coordination in a complex problem-solving situation" (Terhune, 1973:215). Translated from personality variables into status differences, this is consistent with Statement II; that is, women assume it is not legitimate for them to make task contributions.

The idea that the observed differences in the behavior of men and women in mixed-motive games are due to differences in willingness to take the initiative in a situation requiring coordination rather than differences in competitiveness or cooperativeness is supported by a series of experiments reported by Kelley et al (1965). In these experiments, subjects were presented with a situation that has a logical structure like a mixed-motive game but is not defined as a competitive situation. The subjects had to take turns pressing buttons to escape from a noxious stimulus; all could

escape only if efforts were properly coordinated. Women generally were less likely to escape than men, except that sex differences disappeared in a condition in which a signal of confidence ("I will wait for you") was used by a confederate. For both sexes, failure to escape was positively associated with (among other factors) an orientation toward taking cues from others rather than deciding independently. In this situation, as in the group problem-solving studies, the apparent reluctance of women to take the initiative disappeared when there was an explicit statement that such behavior was expected and acceptable.

The conflict studies are particularly relevant to Statement II, since the values of individual acts in these experiments are assigned by the experimenter. Thus, differences in behavior cannot be explained readily by differentiation of performance expectations. The results of the conflict studies seem consistent with the idea that the "feminine" role requires avoiding the kinds of behavior associated with competitive status enhancement or dominance, rather than with low level of interest in the task or with high motivation for equality, harmony or "helping" others. Some further insight into the dynamics of this in a two-person game setting is provided by Wiley (1973), who has reported that when same-sex and mixed-sex dyads were given a Prisoner's Dilemma type of task with no verbal communication there were no sex differences in cooperation. When verbal communication was allowed, the mixed-sex dyads had higher levels of cooperation than the same-sex dyads. This seemed due to the fact that, in all but one of the mixed-sex dyads with communication, the man suggested a mutual cooperative strategy which the woman then agreed to and followed, while neither the all-male nor the all-female dyads with communication were able to organize themselves. Once again, the women were observed to contribute to a group product only after someone else (in this case, the other subject) had asked them to contribute. As in the group problem-solving studies, this situation is one in which all-male and all-female groups

had more difficulty coordinating than mixed-sex groups.

This theory suggests that contributions to a task may be viewed as a type of competition because of their implications for distribution rewards. We should expect that when task contributions and rewards can be separated operationally, women should demand less reward than men for the same level of contribution. It already has been mentioned that studies of negotiation have found women demanding less than men. Another piece of evidence comes from a study by Leventhal and Lane (1970) who had college students work with a (fictitious) same-sex partner, told them that their performance was either superior or inferior to that of the partner, and allowed them to allocate a monetary reward. Men took more than half the reward when their performance was superior and somewhat less than half when it was inferior; women divided the reward equally when their performance was superior and took much less than half when their performance was inferior.

(5) *Dominating behavior.* A study by Megargee (1969) supports the assumption that women try to avoid *appearing* to "lead" or "dominate" others. This experiment examined the relationship between dominance, as measured by the California Psychological Inventory, and actual leadership behavior in dyads composed of one high-Dominance and one low-Dominance person. When the subjects must decide who is to be leader, the general pattern is for the high-Dominance person to suggest him or herself, and to be the leader. However, in the dyads with a high-Dominance female and a low-Dominance male, the usual pattern is for the high-Dominance females to suggest that the male be leader, to which he usually agrees.

There is some evidence that women find having influence less rewarding than men do. Gray et al. (1968) and Richardson et al. (1973) reported that in three-person same-sex groups whose task was to form words out of randomly appearing letters, the women's initiation of task behavior seemed to be less contingent on their having had influence on the others in the past.

These two sets of experiments also found that the women's acceptance of influence from others was less contingent on the value of the suggestions than that for men.

The theory also suggests that dominating or aggressive behavior will be more accepted from men than from women, regardless of competence. Wahrman and Pugh (1974) partially tested this by presenting male subjects with a confederate who violated procedural rules established by a group for a problem-solving task. The confederate was also made to appear competent or incompetent at the task. The subjects accepted influence attempts by nonconforming men whether competent or incompetent, although incompetence somewhat reduced the influence. The competent nonconforming female had somewhat less influence than the competent nonconforming male, but the incompetent nonconforming female had virtually no influence. Apparently, as our theory would predict, men will accept obnoxious behavior from other men whether competent or not, accept it to a certain extent from competent women, but not accept it at all from incompetent women.

(6) *General sex role expectations.* If, as we have assumed, differences in the legitimacy of competitive behavior are part of generally held sex role expectations, we should be able to find evidence for this in people's descriptions of ideal behavior. Although there is much speculation that stereotypes for sex roles are changing among American middle-class youth, Komarovsky (1973) reported that a sample of college men still endorsed the ideals of masculine assertiveness, determination, decisiveness, courage, independence and aggressiveness. Almost half of the sample expressed some anxiety over failure to live up to those ideals in their relationships with women. Evidence that there are cultural and social class differences in legitimacy of assertive types of task behavior for women is presented by Strodbeck (1951), who compared couples from three different cultures, and Tallman and Miller (1974), who compared middle- and working-class American families. The latter study also indicates, as we might expect

from our review of the problem-solving and conflict studies, that differences in the acceptability of task contributions from women are associated with differences in the ability of families to coordinate effectively for solving a problem.

Conclusions

This approach has been not to look for universal sex differences but, rather, to try to explain the differences between situations in which sex role differences appear and those in which they do not. The argument is that status rather than task/social differentiation is the crucial concept, as status affects performance expectations and expectations for legitimacy of competitive or dominating behavior. Information, accepted by self and other, that the females present are at least as competent as the males establishes one kind of situation in which sex differences in task behavior are minimized. Information, accepted by both self and other, that task behavior by the women is not motivated by competitive status enhancement or that competitive status enhancement is legitimate in this particular case establishes another situation. A review of the literature suggests some ways in which the latter kind of information may be conveyed. These include a woman's: (1) having high rates of social activity; (2) being appointed to a leadership position by an outsider, such as an experimenter; (3) being requested to make task contributions by other group members (4) delegating formal leadership to a male; (5) occupying a role in which leadership is expected (for example, mother *vis à vis* teenage daughter in American middle-class families); (6) working on a task that is defined as appropriate for women. Whether these have similar effects, the ways they relate to perceived task competence, their interaction with such variables as task legitimacy, and the similarity or dissimilarity of these processes to processes involving status characteristics other than sex are questions that might be addressed by future research. In particular, advances in theory and research on the relationship of social and expressive be-

havior to status processes would be helpful.

Finally, we might note that in predicting task-related types of behavior of men and women, one should keep in mind that although a review of research shows that there are some conditions under which the amount of task performance of women is inhibited compared to task behavior of men, there is evidence that sex differences in rates of task behavior are highly affected by situations. In many cases, the rates of women and men are approximately equal, and both women and men have been observed to change their behavior as the situation changes.

Research also shows instances in which, while groups of women had difficulty with a task because no one wanted to take the lead in coordinating activities, groups of men had difficulty with the same task because everyone tried to take the lead. As with other types of roles, the expectations people have for their own and others' behavior may either facilitate or hinder coordination. It appears that when either men or women in a task-oriented group operate according to stereotyped sex role expectations, they are more likely to hinder than to help coordination.

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CONFLICT INTENSITY, MEDIA SENSITIVITY AND THE VALIDITY OF NEWSPAPER DATA*

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Although quantitative studies increasingly dominate analyses of conflict, issues of data validity have received little consideration. However, in a recent article, Danzger (1975) shows that differential location of wire service offices across U.S. cities biases newspaper data on racial conflict frequency and concludes that other studies (particularly of racial disorders) which rely on news sources are similarly invalid. We challenge Danzger's treatment and develop an alternative model of validity (which is defined strictly as the probability that events will be reported; we do not deal with the accuracy of published characteristics of events). Our model specifies validity as a function of event intensity and media sensitivity, each of which is dependent on several indicated factors. Intensity determines the relative probabilities that different forms of conflict behavior will be reported. However, precise reporting probabilities also depend on media sensitivity to conflict events. This model indicates the conditions under which news reports are valid sources of conflict frequency data and generates hypotheses—including no bias in racial disorder studies—opposite Danzger's. We report findings from two separate analyses: (1) racial disorders across 673 cities from 1965 to 1969 and (2) largely nonviolent collective protests occurring in 43 U.S. cities during 1968. Both sets of results contradict Danzger's conclusions and support our event intensity-media sensitivity model of validity. We indicate how this model is useful for assessing validity in other analyses of conflict and suggest more general implications for treatments of bias in newspaper data.

Over the past decade, quantitative studies have come to dominate analyses of social conflict, particularly in the areas of collective protest and violence. Although some investigations of these phenomena focus on characteristics of individual participants (Rudé, 1964; Caplan and Paige, 1968) or protest groups (Gamson, 1975), most empirical inquiries analyze conflict events aggregated over spatial and temporal units. These latter studies include analyses of (a)

cross-national patterns of conflict, both domestic (Feierabend and Feierabend, 1966; Gurr, 1968; Hibbs, 1973) and international (Rummel, 1963; Tanter, 1966); (b) time series of collective violence and industrial strike activity within particular countries (Kirkham et al., 1970; Snyder and Tilly, 1972; Hibbs, 1974; Snyder, 1975); (c) subnational ecological units (Spilerman, 1970; 1971; Morgan and Clark, 1973; Eisinger, 1973; Chirot and Ragin, 1975).

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Most of these attempts to isolate determinants of conflict and/or violence through ecological analyses of events rely on newspaper accounts as primary sources of data on episodes of the dependent variable. However, the assumption in such treatments, that news and other reports of events unobserved by the researcher adequately count and describe the behaviors of substantive interest, usually receives token if any consideration.¹

Such neglect of data quality issues is surprising in view of increasing evidence of bias in published news (White, 1950; Breed, 1955; 1958; Jacobs, 1967; Bagdikian, 1972; Tuchman, 1972; 1973; Sigelman, 1973; Molotch and Lester, 1974; 1975). More precisely, "bias" encompasses at least two discrete departures from some ideal standard of objectivity. One type concerns the *selection* of news, i.e., differential completeness of reporting across classes of events. Selection bears on validity in a statistical sense—the extent to which news derived data measure reporting practices instead of the desired population of events. A second form of bias is distortion in the *content* of news, including inaccurate reporting (Jacobs, 1967) and unbalanced interpretation of events (Bagdikian, 1972; Sigelman, 1973), which involves validity in a more closely journalistic sense. Although many treatments of media bias limit themselves to consideration of content issues (Bagdikian, 1972; Tuchman, 1972; 1973;

Sigelman, 1973), some address both types of validity (White, 1950; Breed, 1958; Molotch and Lester, 1974; 1975).² We stress this distinction between forms of bias because our substantive focus on conflict dictates a primary emphasis on *the probability that events are reported in newspaper sources*.³ Consequently, we restrict our analysis and use of the term "validity" to refer only to this type of bias.

Although analyses of conflict generally ignore work on media bias, in one recent paper Danzger (1975) challenges the validity of newspapers as sources of data on conflict events.⁴ Citing these other studies of biased news reporting as evidence, Danzger suggests that racial disturbances in the United States and reports of their occurrence in standard sources (*New York Times Index*, Congressional Quarterly's *Civil Disorder Chronology* and the Lemberg Center's *Riot Data Review*) do not conform to a straight line process of communication. Instead, he argues that the type of local reporting network—in particular, the pres-

² These studies all manifest a theoretical interest in the social construction of reality and account for bias in terms of the more or less subtle influences imposed by political inclinations of news gathering organizations and their personnel (Breed, 1955; Bagdikian, 1972; Sigelman, 1973), reporters' everyday job constraints (Tuchman, 1972; 1973) and/or broader loci of power in society (Breed, 1958; Molotch and Lester, 1974; 1975). Therefore, this approach is better suited, at least where overt censorship does not exist, to explain bias in the journalistic rather than in the statistical sense. Moreover, empirical evidence of journalistic bias is much stronger.

³ Some aggregate indices of conflict (e.g., numbers of participants) are based on the content of news reports. However, since every ecological study relies in part (and often solely) on frequency measures, this issue of counting accuracy involves more general implications for empirical analyses of conflict data. In addition, we suspect that substantial underreporting constitutes a larger component of error in content-based measures than does bias in the reports themselves.

⁴ We consider some general implications for treatments of media bias in our conclusions. However, the bulk of our discussion focuses heavily on Danzger's study because it directs hypotheses of bias explicitly to conflict and violence and, therefore, raises serious questions concerning previous empirical work in this entire substantive area.

¹ With few exceptions, e.g., archival checks in Tilly's (1968; 1975) French violence study, assertions of reporting validity, particularly in cross-national analyses, are based on zero-order correlations between measures of conflict and indices of press censorship (Gurr, 1968:1108n; Tanter, 1966:43-5). However, positive associations are not adequate evidence of validity for two reasons. First, since other determinants of conflict variation are not controlled, conflict may be reported less fully under conditions of press restrictions even if such places have greater mean levels. Second, such correlations do not address the following possibility. Assume a typical situation in which several measures of conflict are combined. Even though each indicator's validity may be constant across nations, summary scores will (assuming differential composition of event types by country) be biased to the extent that some measures are less completely reported than others.

ence of an Associated Press (AP) or United Press International (UPI) wire service office in a city—substantially increases the probability that accounts of an event will actually appear in the printed sources (especially the *Times Index*) used in ecological analyses. Consequently, the results of such analyses should be biased by variation across cities in these different reporting mechanisms.

Danzger's (1975:579) tests of this contamination hypothesis are based on regression analyses of "... 644 cities in the continental United States, having populations of 25,000 or more by the count of the 1960 U.S. Census." His operationalization of the dependent variable uses the *Times Index* reports of "... all conflict events, initiated from either side and by as few as one person, with the prime emphasis and concern being on civil rights demonstrations" (1975:571). Racial conflicts listed from 1955 to 1965 are aggregated over cities and regressed on number nonwhite, a South/non-South dummy variable, city age, number unemployed and an AP/UPI dummy variable.⁵ Since AP/UPI substantially increments the R^2 even after other factors are entered in stepwise regressions, Danzger concludes that reports of conflict events are indeed biased in the hypothesized fashion. However, absence of a wire service office will not necessarily lead to underreporting:

once the word gets out about a newsworthy story, the wire services ... will send their own teams of reporters to investigate the matter further. We therefore expect that if we compare cities with a "substantial" number of stories but without wire service offices to cities with wire service offices, we will find no difference in the number of conflicts reported. (Danzger, 1975:580)

This supplementary point is examined with

⁵ Danzger also includes a number of other control variables (e.g., numbers employed in public administration and educational service, foreign-born and foreign stock, employment in managerial occupations and a dummy variable for state capital cities), but finds these measures to be individually and cumulatively unimportant in explaining patterns of conflict or reducing the impact of AP/UPI. We exclude these additional indicators from our reported analyses.

analyses of 83 cities for which ten or more conflict events were reported. Danzger eliminates the association between AP/UPI and number of conflict reports by controlling for number nonwhite and a dummy variable for state capital cities and interprets these findings as support for the frequency-noncontamination hypothesis.

Although Danzger's definition of racial conflict is broad (more than ten thousand reported events qualified for his sample between 1955 and 1965), he takes the close correspondence between his previous results (Danzger, 1968) and those of studies (especially Spilerman, 1970) which examine spontaneous, collective racial violence during the 1960s to warrant the conclusion that analyses of racial violence (e.g., see also Downes, 1968; 1970; Jiobu, 1971; 1974; Lieberman and Silverman, 1965; Wanderer, 1969; McElroy and Singell, 1973) are similarly biased. Moreover, Danzger's (1975:573) criticisms are particularly serious in that he explicitly extends them to all investigations of conflict which ultimately depend on some wire service reporting mechanism.⁶

Danzger's raising of this largely neglected issue of validity in conflict data is welcome. However, his substantive treatment is misleading and seriously incomplete. Danzger mistakenly assumes that (given a particular type of communications network) all conflict events are homogeneous and, therefore, equally likely to be reported. Consequently, his model of validity considers that data contamination is solely a function of the reporting mechanism. As we later show, this shortcoming produces faulty empirical

⁶ Although the AP/UPI variable has a substantial net influence on conflict in Danzger's analyses, it does not alter the effects of major substantive importance (number nonwhite and Southern location). However, he properly suggests that other studies may not be so fortunate with respect to stability of conclusions after corrections for possible reporting biases. Moreover, Danzger's challenge of the validity of conflict analyses is potentially more serious than even he indicates. His criticisms logically apply not only to newspapers, but to all sources of conflict data in which media sensitivity (and, therefore, event-reporting probabilities) vary across temporal and spatial units.

predictions concerning the validity of other studies.

This is not to claim that all conflict data are unbiased—Danzger's own results dispute that hypothesis—but that the Danzger model does not provide a basis for discriminating between many valid studies versus those vitiated by reporting errors. In the present context, we develop an alternative model of validity as a function of the *interaction* between the *intensity* of conflict events and the *sensitivity* of reporting mechanisms (each of which is dependent on several specified factors). This alternative yields testable hypotheses concerning conditions under which events will be underreported. One such hypothesis is that data used in the analyses of collective racial violence, on which Danzger focuses criticism, are unbiased. As a partial test of our model versus Danzger's, we report analyses of data on (1) violent racial disorders across 673 U.S. cities from 1965 through 1969 and (2) collective protest events (racial disorders are explicitly excluded) occurring in 43 U.S. cities during 1968, from a study by Eisinger (1973). In both cases, the empirical findings provide strong support for our arguments and none for Danzger's. However, while these analyses focus on violence and protest in the urban U.S. during the 1960s, for reasons of data availability and direct relevance to Danzger's conclusions our treatment of these issues should be applicable to other times and places.

CONFLICT INTENSITY, MEDIA SENSITIVITY AND DATA VALIDITY

An overview of some longstanding issues in the definition and measurement of social conflict will aid us in developing certain central points in our interaction model of validity in conflict data. After discursive treatment of these issues, we present our arguments in more rigorous form.

Defining and Operationalizing Conflict

The proper definition and classification of "conflict" has long evidenced conceptual problems and disagreements (cf. Mack and Snyder, 1957; and especially Fink, 1968,

for an excellent review of such issues). For example, whether psychological antagonisms constitute an underlying determinant or conflict itself⁷ and whether conflict encompasses latent opposition of interests (competition) as well as manifest interactions are points of continuing definitional debate. Similar disputes occur in attempts to classify forms of conflict. Different schemes variously identify the social characteristics (as in racial or industrial conflict), level of complexity (individuals, organizations, states) or structural relations (intra- versus extrasystemic) of the parties involved as critical dimensions along which forms of conflict fundamentally differ (Fink, 1968).

All quantitative studies of conflict partially "resolve" these metatheoretical issues by restricting their analyses in necessary ways: i.e., to observable interactions, occurring at a specified level of analysis, and among parties with certain identifiable social or political characteristics (e.g., racial groups, national governments).⁸ Aside from these global limitations, however, empirical treatments employ either of two general strategies in their operationalization of conflict. The first of these is a "multiple-indicator" approach which assumes (a) the existence of some underlying dimension, which is substantively designated "conflict" (Rummel, 1963; Tanter, 1966; Danzger, 1975), "political instability" (Feierabend and Feierabend, 1966), "civil strife" (Gurr, 1968), etc. and (b) that combining (i.e., summing, usually after weighting) indicators of a variety of behavioral manifestations of conflict adequately captures the

⁷ In one extreme position on this issue, Shepard (1964) implies that psychological antagonisms constitute "conflict," whereas observable behaviors are merely forms of conflict resolution. Fink himself argues for a broad definition which includes both (unobservable) psychological hostilities and conflict behaviors.

⁸ The necessity of these strategic limitations in empirical analyses is admittedly obvious. Our point here (and throughout the paper) is that operational restrictions regularly involve sacrifices in generality *vis-à-vis* some widely held conceptions of conflict. We attempt to indicate the conditions under which accommodation to issues of data validity warrant these theoretical costs.

conflict or other specified theoretical variables. Although this multiple-indicator treatment is more easily recognized in studies which use factor analyses to weight conflict indicators (Rummel, 1963; Tanter, 1966), it also characterizes investigations (e.g., Danzger, 1975) which merely sum (i.e., weight equally) events of different form.⁹ Whatever the weighting technique, this approach assumes that different forms of conflict behavior are equally well measured and reflect similar causal processes—although there may be “substitution effects” among observed event types across time and space.

A second strategy for operationalizing conflict restricts empirical inquiry to relatively specific and uniform types of behavior (we label this the “form-specific” approach), such as industrial strike activity (Britt and Galle, 1972) or “instances of ‘spontaneous outbreaks’ . . . characterized by primarily Negro aggression” (Spilerman, 1971:427n). This form-specific approach considers that underlying causal processes vary across forms of conflict behavior (or that similarities should be verified rather than assumed). Moreover, studies in this line generally further limit phenomena under empirical investigation according to characteristics of events, such as minimum criteria of size, duration or violence. Examples of such operational boundaries are “thirty or more individuals” (Spilerman, 1970:630n, on racial disorders), “at least one group of fifty or more persons in the course of which someone seized or damaged persons or objects over resistance” (Snyder and Tilly, 1972:522, on collective violence in France) and “all strikes and lockouts which continue for 1 full day or shift or longer and . . . involve six workers or more” (U.S. Department of Labor, 1974:15, on its definition of work stoppages). Although these operational limitations are often imposed on theoretical

grounds,¹⁰ they almost always involve strategic assessments concerning equivalent “visibility” (probability of being reported) of all events which meet minimum criteria for inclusion.

In summary, these multiple-indicator and form-specific strategies differ in their assumptions concerning variation in causal processes across conflict behaviors.¹¹ Moreover, those who utilize the form-specific approach apparently are willing to sacrifice range of theoretical generalization for greater confidence that conclusions are adequately supported by the data. For example, there are no compelling reasons to treat racial disorders involving twenty persons or French collective violence with groups of forty participants as either definitionally or substantively different from those events which meet the minimum size criterion reported by Spilerman (1970) and Snyder and Tilly (1972). Consequently, attenuation of the range of conflict behavior to which results of these studies may be generalized is justified only if (a) various forms of conflict behavior have differential likelihoods of being reported and (b) the inclusion criteria employed effectively discriminate among these different levels of validity.

Although available evidence is sketchy, it suggests affirmative conclusions on both these points. For example, Tilly’s (1968) French violence study, which checked readings of two daily Paris newspapers against archival and secondary sources, indicates that violent disturbances meeting the size criterion were more likely to be (and were almost fully) reported. More important for

⁹ The “form” of conflict refers to (usually arbitrary) distinctions among event types according to their “contextual” features (e.g., social characteristics and/or complexity of involved parties, as in racial versus industrial and intra-versus international conflict).

¹⁰ For instance, “discontent” models of industrial conflict view individual absenteeism, sabotage, etc. and strike activity as indicators of the theoretical variable (e.g., Knowles, 1954). However, explanations which stress *organizational* mechanisms (Britt and Galle, 1972; Shorter and Tilly, 1974; Snyder, 1975) treat individual and collective protests as qualitatively different forms of industrial conflict.

¹¹ Space limitations preclude discussion of available evidence on such variation across forms of conflict. For good treatments of the causal assumptions implicit in various operational definitions of “conflict” and “violence,” see Nardin (1971), Jacobsen (1973) and Tilly (1975).

our inquiry, however, is to show that various forms of conflict behavior measured in multiple-indicator analyses have different reporting probabilities. For these purposes, we draw on two representative studies in this line (Feierabend and Feierabend, 1966; Danzger, 1975). In the Feierabends' (1966:251) cross-national investigation, "data on internal conflict behaviors were collected for eighty-four nations for a fifteen-year period, 1948-1962. The data derive from two sources: *Deadline Data on World Affairs* and the *Encyclopedia Britannica Yearbooks*." Some thirty conflict behaviors are included; these range in magnitude from exiles, arrests of a few insignificant persons and crisis within nongovernmental organizations to civil wars, *coups d'état* and mass arrests (Feierabend and Feierabend, 1966:255). It is likely that the latter set of event types is far more fully reported (for reasons of size, violence and "political significance") than the former set in these (as well as newspaper) sources. Moreover, underreporting of the less visible event types probably varies across units—i.e., a few arrests are less likely to be listed for Afghanistan than for the United States because of differences in international importance and locational proximity to the source compilers and consuming public. Although we consider these points highly plausible, we have independent evidence only in the following instance. Feierabend and Feierabend purport to include all strikes (though these are divided into "general," "macro" and "micro" strike categories) in their analyses. However, more strikes occurred in the United States alone during 1961 and 1962 combined (U.S. Department of Labor, 1974:368) than the total number of events of *all thirty types* in the Feierabends' data for 84 nations from 1948 through 1962. With these strike indicators, therefore, we can be certain of drastic underreporting bias. Moreover, it is extremely doubtful that such serious validity problems pertain to certain other measures (e.g., civil wars).

Analogous points may be raised concerning Danzger's racial conflict data, in which events "initiated from either side and by as few as one person," most of them

apparently nonviolent ("prime emphasis and concern . . . on civil rights demonstrations"), are included. Danzger does not explicitly define "conflict." However, if we take it to mean antagonistic interactions, then certain forms of racial conflict are subject to the same under- and differential reporting biases noted in the Feierabends' work. For example, Berk and Aldrich (1972) indicate that white retail merchants engage in a variety of antagonistic behaviors toward their black customers, and that such acts are apparently "meaningful" to ghetto residents because they help explain patterns of vandalism in civil disorders. It is at best unlikely that many of the large number of hostile merchant-customer interactions are reported for any city. To the extent that such events are listed in the *Times Index*, we agree that AP/UPI cities (especially New York) should be favored. Again, however, we doubt that larger and more violent conflicts (e.g., racial disorders) are subject to such underreporting (in fact, we suspect that disorders were equally well reported regardless of location in AP or UPI cities). Therefore, we challenge Danzger's argument that validity depends only on the reporting network. In the following section, we present a model of validity which depends *both* on reporting *and* the differential characteristics of conflict events indicated here.

Conflict Intensity and Media Sensitivity: An Interactive Model

We proceed from the final point developed above, that different forms of conflict behavior have differential probabilities of being reported in newspapers and other standard sources of event frequency data. We specify that these differential likelihoods vary positively with the *intensity* of conflict behavior. This intensity dimension is reflected (and operationally defined here) in three measurable characteristics of any observable conflict event: *size* (total number of persons ever participating), *violence* (extent of physical damage to persons and/or property) and *duration*.¹² Although these

¹² Although some analyses of conflict (e.g., Britt and Galle, 1972) make a distinction be-

components of intensity are empirically related (Snyder and Kelly, 1976), they are conceptually distinct and are expected to have partially independent (positive) effects on reporting. (We also recognize that other characteristics of conflict events, such as the nature of the participants, should affect reporting probabilities. However, these are not components of intensity and are considered subsequently.)

Therefore, given N discrete forms of conflict behavior (event types), E_1, E_2, \dots, E_n , and (hypothetical) information on their intensity components, we can (1) assign mean intensity scores, $\bar{I}_1, \bar{I}_2, \dots, \bar{I}_n$ to each and (2) *rank-order* each event type, $E_1 > E_2, \dots, E_{n-1} > E_n$, according to its greater or lesser likelihood of being reported. (For purposes of convenience, we will assume throughout that $\bar{I}_1 > \bar{I}_2, \dots, \bar{I}_{n-1} > \bar{I}_n$ and that intensity scores for each event type are normally distributed about their mean values.) We stress, however, that precise probabilities, $p(E_1), p(E_2), \dots, p(E_n)$, *cannot* be determined in the absence of information about the sensitivity of the reporting mechanism.

As Danziger (1975) shows, reporting mechanisms differ in their sensitivity to conflict events. We operationally define *media sensitivity* as the event intensity above

which the probability that conflicts will be reported exceeds a given criterion level (say .99). More sensitive media sources have lower thresholds and report larger ranges of conflict behaviors. Therefore, while the probability that an event type will be reported increases with its mean intensity, a given indicator of conflict behavior will achieve adequate validity only if the media source is sensitive enough to record at least 99 (or other arbitrary) percent of all occurrences. Moreover, where media sensitivity varies across spatial and/or temporal units of analysis (e.g., AP/UPI versus other cities; the U.S. versus Afghanistan in the Feierabends' study), conflict indicators are valid only if reporting for the *least* sensitive unit is greater than or equal to the specified criterion level.

Several corollary points, suggested in our earlier discussion, follow from this interactive model of validity in conflict data. First, given the sensitivity of newspaper and other standard sources, it is unlikely that *any* multiple-indicator analysis of conflict achieves adequate validity. These problems of biased data should be particularly serious in cross-national studies (Feierabend and Feierabend, 1966; Gurr, 1968; Hibbs, 1973; Rummel, 1963; Tanter, 1966), which analyze events of widely varying mean intensities *and* units (countries) across which media sensitivity differs considerably. Second, form-specific strategies for operationalizing conflict are more likely to be valid, particularly when the minimum inclusion criteria are set at relatively high levels of intensity (e.g., Spilerman, 1970; 1971). However, since most form-specific studies lack independent confirmation concerning validity, it remains plausible that their indicators of conflict behavior are, in some cases, partially contaminated and, in others, entirely valid but unnecessarily restrictive. In general, we specify that operationalization of "conflict" is optimal (i.e., the costs in permissible generalization due to *strategically* imposed inclusion standards are precisely warranted) when the probability that the least intense event included will be reported by the least sensitive reporting mechanisms exactly equals the validity criterion level. However, more restric-

tween size and "intensity" (which refers only to duration and elsewhere only to violence), Morgan and Clark (1973:614) and Spilerman (n.d.) provide some independent support for our operational treatment. They report results of factor analyses which indicate that number of participants, duration and measures of damage to persons and property in racial disorders cluster along a single ("severity") dimension. We also note that, while evidence is inconclusive, we suspect a rank-ordering of size, violence and duration in importance as determinants of reporting probabilities. Some support for this ordering is presented in our study of industrial violence in Italy (Snyder and Kelly, 1976). We find that rates of violence (as reported with accounts of each strike in an official source) are five times as great as those indicated by Shorter and Tilly (1971) in a comparable study of France (in which evidence of violence is based on two Paris newspapers). Since we are largely able to dismiss institutional sources of this difference, we attribute it to considerable newspaper underreporting of small work stoppages in France, even when these strikes were violent.

tive event inclusion limitations are, of course, warranted if they are imposed on the *theoretical* grounds of hypothesized variation in causal processes across forms of conflict behavior.

We now apply the general hypotheses developed here to analyses of racial conflict in U.S. cities. Figure 1 presents a heuristic diagram of our interactive model for three types of racial conflict and two levels of media sensitivity (AP/UPI versus other cities). For event types E_1 , E_2 , and E_3 , respectively we select collective racial violence (since Danzger explicitly focuses criticism on studies of these disorders), civil rights demonstrations (to which Danzger admittedly gives primary emphasis), and

the hostile merchant-customer interactions discussed earlier. Hypothesized mean intensities (\bar{I}_n) of these event types vary, as do their frequencies. We also allow some intersection of the intensity ranges of these event types—e.g., the most intense civil rights demonstrations will manifest greater intensity than the least intense racial disorders.

As previously suggested, we hypothesize that events which meet Spilerman's (and others') racial disorder inclusion criteria are intense enough to achieve reporting validity *even in the absence* of a more sensitive (AP/UPI) news network. This expectation is shown in Figure 1, which indicates complete reporting of racial disorders [$p(E_1) = 1.00$] for *both* levels (AP/UPI

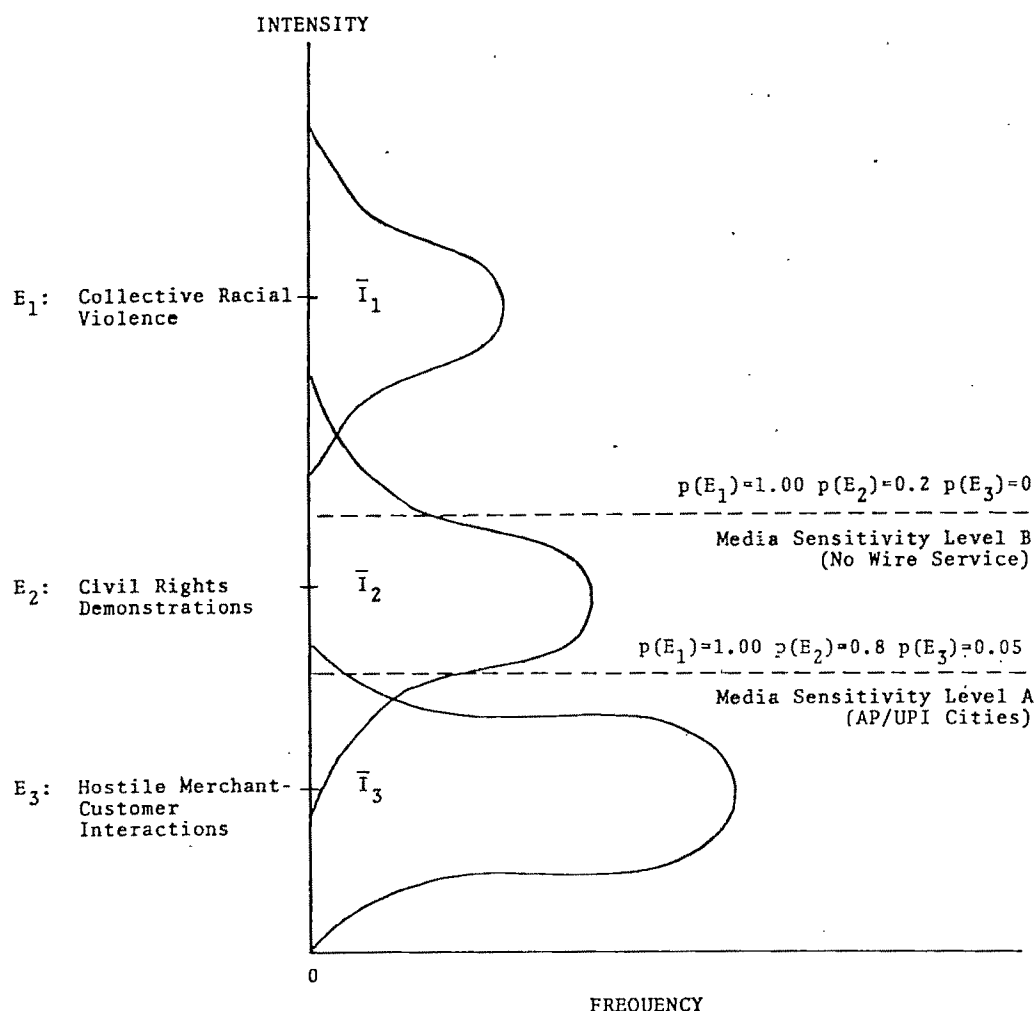


Figure 1. Heuristic Diagram of Conflict Intensity-Media Sensitivity Model of Validity, Applied to Three Types of Racial Conflict and Two Levels of Media Sensitivity

and other cities) of media sensitivity. However, the less intense event types (E_2 , E_3) should be reported with greater validity in wire service than other locations because their intensity distributions are intersected at different points by the differentially sensitive reporting networks. Therefore, we expect that analyses of racial *conflict* across U.S. cities (Danzger, 1975), but *not* studies of racial *violence* (Spilerman, 1970; Lieberman and Silverman, 1965; Downes, 1968; 1970; Jiobu, 1971; 1974; McElroy and Singell, 1973; Wanderer, 1969), will be biased. More generally, extension of the model shown in Figure 1 to N types of conflict behavior and M levels of media sensitivity is straightforward.

However, since we have not yet considered factors which affect media sensitivity (beyond the differential "efficiency" of communications networks across U.S. cities), we now pay brief attention to these issues. We anticipate that two broad classes of factors—"political climate" and contextual characteristics of conflict events themselves—determine media sensitivity to conflict behaviors. Important contextual characteristics (i.e., those which are not components of intensity) include frequency nature of participants involved and locational "visibility" *vis-à-vis* media sources. We expect a net negative relationship between frequency of an event type and its probability of being reported (although such a relationship may largely reflect the negative association between frequency and intensity). Small town newspapers are likely to report fistfights in local bars; those in major cities are not. Similarly, as industrial strike activity has evidenced secular increases in frequency and routinization in form (Shorter and Tilly, 1974), it has become a less newsworthy conflict behavior.¹³ Concerning racial disorders, this argument implies that, if any underreporting does

occur, it is more likely in cities with past disorder experience—i.e., opposite Danzger's (1975) indication that many previous conflicts sensitize the media to report such behaviors.¹⁴

Effects of other conflict characteristics on media sensitivity are more obvious, and we merely outline them here. Media sources are more likely to report events in which coercive forces are involved (since official statements on such incidents are usually forthcoming) and those in which participants are already public figures. Data validity also may be affected by the interaction of types of participants and political leanings of the reporting media¹⁵ and, as we have indicated, locational proximity to the relevant communications networks (including AP/UPI) is salient.

Finally, aspects of the "political climate" in which conflict occurs regularly affect the sensitivity of news sources of data. These effects often are manifested directly, as in governmental suppression of the press. However, they also may occur indirectly, for example when a single dramatic event raises public interest in an entire class of similar events. This last point suggests that, under some conditions, the rank-ordered reporting probabilities of conflict events which are based on their relative intensities will be rearranged. With few exceptions, however, we doubt that events of relatively low intensity remain newsworthy for long and, consequently, that these discontinuities in the linear intensity-reporting probability relationship are systematic.¹⁶

In summary, we have presented a model

¹⁴ We agree, however, that in the very short run rapid upsurges of previously infrequent forms of conflict may result in greater media sensitivity to such events. In the longer run, we expect our above arguments concerning frequency to take precedence, which implies a curvilinear (inverted U) relationship over time between frequency and these irregular event types.

¹⁵ For example, during the Weimar Republic in Germany, it appears that socialist newspapers were more likely to report attacks of left-wing groups, while rightist papers more regularly listed attacks on Nazis (personal communication, Professor James Diehl, Department of History, Indiana University).

¹⁶ This hypothesis is based on the assumption that intensity reflects the "importance" of conflict behavior in relatively invariant fashion.

¹³ We do not imply that these secular changes in frequency and routinization have undermined the completeness of strike reporting in official statistical compendia. However, in extreme cases, such as the French sitdown strike waves of 1936, unprecedented magnitudes of conflict can break down the reporting mechanism itself (Shorter and Tilly, 1974). Of course, this outcome is highly improbable with newspaper sources.

in which validity in conflict data derived from newspapers and other standard sources depends on the interaction between the intensity of conflict events and the sensitivity of reporting mechanisms. Although this model cannot *a priori* indicate levels of indicator validity with precision, it does provide clear guidelines for discriminating between those analyses of conflict behaviors which are likely based on valid versus contaminated data. Consequently, our interactive treatment differs considerably from Danzger's (1975) arguments, which (a) attribute bias solely to differential media sensitivity and (b) therefore conclude that empirical studies of conflict which utilize news reports are *all* invalid. In the following section, we report results of two sets of analyses which test these alternative arguments.

Analysis 1: Racial Disorders (1965-1969)

Data and method. For this analysis, information on the incidence of racial disorder was coded for 1965 through 1969 from reports issued by the Lemberg Center, the Senate Committee on Government Operations and the Congressional Quarterly.¹⁷ We have maintained separate files by year for the disorder data in order to examine (a) possible parameter shifts over time as a function of the changing form of racial disorders during this period¹⁸ and

¹⁷ Particular Lemberg Center Reports include: *Riot Data Review*, for May, 1968; August, 1968; February, 1969; *U.S. Race-Related Civil Disorders*, for January-June, 1969; July-December, 1969; and *Compilation of 1967 Disorders*. Our second source is the *Staff Study of Major Riots and Civil Disorders—1965 through July 31, 1968*, of the Permanent Subcommittee on Investigations of the Committee on Government Operations, 1968. We also used the Congressional Quarterly's *Civil Disorder Chronology*, September, 1967. All of these publications rely almost entirely on newspaper reports and thus conform to the sources employed by Danzger (1975) and Spilerman (1970; 1971). In some cases, our sources also communicated with city officials and local newspapers, generally to supplement other reports.

¹⁸ See Baskin et al. (1971) for a discussion of changes over time in the form and character of riots. For example, the typical precipitant changed from "random" encounters of black citizens with police in 1967 to consequences of the assassination of Martin Luther King in 1968.

(b) Danzger's arguments versus ours on the relationship between previous conflict and reporting bias. We exclude the years 1961 through 1964 because the very small number of disorders does not permit separate yearly analyses. Our operational definition of a disorder approximates that employed by Spilerman (1970; 1971), i.e., thirty or more blacks, doing damage to persons and/or property, and not occurring at or in school. However, spontaneity of the event was not a necessary criterion for inclusion because, in many cases, it was impossible to differentiate between spontaneous and nonspontaneous disorders with any confidence. Table 1 shows the distribution of disorder frequency in Spilerman's study and ours for the overlapping years (1965-1968). Despite somewhat different definitions, figures presented in Table 1 indicate that the two distributions are generally comparable.

Our independent variables come from two sources. Information on the location of wire service offices was obtained from interviews with officials of AP and UPI, who made available Bureau Directories. Although these directories were compiled for 1974, both Bureau officials assured us that additions since 1965 have been minimal. All other city characteristics are taken from the Alford-Aiken Governmental Units Analysis data set used by Spilerman. This file contains data on all 673 cities in the continental United States which had total populations greater than twenty-five thousand in 1960 (except for his missing data, these cities are identical to those in Danzger's study).

Our analyses of the effect of wire services on the reported frequency of racial disorders proceed (as do most of Danzger's) on the assumption that this relationship should be estimated and interpreted only *after* factors associated with both the dependent variable and the independent variable of interest (AP/UPI) are controlled.¹⁹ Failure to include these controls

¹⁹ This stepwise procedure is employed because other independent variables are *substantively* related to patterns of disorder frequency, while the wire service explanation is an *artifactual* one. Since wire service location is associated with sub-

Table 1. Distributions of Racial Disorders by Cities with N Disorders for 1965 through 1968: Spilerman and Snyder/Kelly

Disorders	Spilerman*					Snyder/Kelly				
	Cities with N Disorders					Cities with N Disorders				
	1965	1966	1967	1968	Disorders	1965	1966	1967	1968	Disorders
0	665	645	566	569	0	664	650	553	540	0
1	8	23	85	78	1	9	17	90	95	1
2		4	18	18	2		6	19	17	2
3		0	3	6	3			5	13	3
4		0	1	1	4			3	4	4
5		1		1	5			2	2	5
					6			0	0	6
					7			1	1	7
					11					11
	673	673	673	673		673	673	673	673	

* Adapted from Spilerman (1970:631).

greatly increases the danger of attributing substantive importance to spurious findings (Blalock, 1968). A related and equally important consideration is proper specification of the functional form of relationships between variables. If, in fact, particular independent variables exhibit nonlinear effects on the dependent variable (e.g., Spilerman, 1971:434-9), failure to specify them as such will misstate the true relationship and may again be a source of spuriousness.

Findings and discussion. Given these methodological points, we estimate the net contribution of wire service location to explained variance in disorder frequency with a model identical to that employed by Danzger (1975:582). Although we recognize that these disorders may differ from other racial conflicts, previous research on racial disturbances (Spilerman, 1970; 1971; Morgan and Clark, 1973) justifies the inclusion of the following independent variables: total nonwhite population in 1960 (N. Nonwhite); the year in which the city first attained a population of twenty-five thousand (City Age); a dummy variable scored one for those cities located in the Census South and zero for all others (South); the total number of unemployed persons in 1960 (N. Unemp); and a dummy variable equal to one for cities which have AP and/or UPI wire service offices and zero otherwise (AP/UPI). The dependent variables are the log transformed (X+1) total cumulated (Total) and yearly disorder frequencies (1965-1969).

Table 2 displays results of the regressions of these disorder measures on the independent variables described above; the latter enters the equations in the order presented in the table. Two preliminary conclusions emerge from this set of regressions. First, AP/UPI does not account for large increments in explained variance of disorder frequency. For total frequency, however, the

stantive sources of disorder (e.g., nonwhite population size, since AP and UPI offices are located in larger cities), if we entered the variables simultaneously in the equation, the variance explained in common would be apportioned among their partial regression coefficients (Gordon, 1968).

Table 2. Increments in Variance Explained by AP/UPI in Regressions of Log ($X + 1$) Yearly and Total Racial Disorder Frequency on City Characteristics *

	Disorder Frequency					
	1965 R ² Change	1966 R ² Change	1967 R ² Change	1968 R ² Change	1969 R ² Change	Total R ² Change
N Nonwhite	.355	.290	.256	.197	.319	.279
South	.004	.007	.009	.001	.000	.000
City Age	.002	.000	.087	.117	.026	.134
N Unemp	.001	.001	.006	.000	.010	.003
AP/UPI	.010*	.004	.015*	.011*	.024*	.028*
Cumulative R ²	.372	.302	.373	.326	.379	.444

* Results are based on 673 cities above 25,000 total population in 1960.

* Significant at .05 level (reported for AP/UPI only).

net influence of wire service reporting is small but not negligible (we estimate an R^2 change=.028 versus Danzger's .054 for racial conflict, although the two figures are not comparable).²⁰ Except for 1969, the impact of AP/UPI in the separate yearly equations is trivial (as we show later, proper specification of the nonwhite population variable eliminates even the weak evidence of wire service bias indicated here). After controlling for plausible sources of racial disorder, the effect of wire service location on reported frequency is at best of tenuous substantive importance. These results therefore tend to support our argument that event intensity is an important determinant of validity in conflict data and suggest that Danzger's conclusions concerning bias in previous studies of racial disturbances are largely mistaken.

Second, net of this general conclusion of minimal bias, the yearly equations indicate some temporal fluctuation in the relationship between wire service location and disorder frequency. Corresponding partial regression coefficients for AP/UPI (where all variables are entered simultaneously; results not shown here) range from small and *negative* for 1965 to increasingly positive

for subsequent years and the cumulative total.²¹ This pattern of increase is opposite that expected by Danzger; he argues that previous conflict sensitizes the media to report current episodes. More specifically, fully eighty percent of the cities which reported at least one racial disorder (by our definition) in 1969 had experienced a previous event during the period covered by our data. Over seventy percent of 1968 disorder cities, but less than forty-five percent in 1967 and fourteen percent in 1966 had reported earlier events. On Danzger's argument and this distribution of disorders, we should expect smaller effects of AP/UPI for the later years because larger percentages of cities were experiencing second, third, fourth, etc. events when media attention presumably was more focused on them. However, these findings are inconsistent with Danzger's expectations. Instead, our hypothesized negative relationship between frequency and reporting probabilities better explains these slight increases over time in the influence of AP/UPI.

In a second set of regression analyses, we address the specification issue raised earlier. In particular, Spilerman (1971) shows that the relationship between non-white population size and disorder frequency is clearly nonlinear, i.e., approaches a modified logistic curve. This functional form represents two substantive analogues:

²⁰ We report significance tests only for AP/UPI and only to avoid claims that we have withheld any information which might be favorable for Danzger's arguments. However, since these cities constitute a population rather than a sample, we stress that significance tests are not interpretable in conventional fashion. Moreover, given the large N, we prefer to rely on criteria of substantive significance (cf. Frideres and Taylor, 1972); we consider that increments of two to three percent marginally meet such criteria.

²¹ In this case, we use metric coefficients because explained variance measures are not comparable insofar as they depend on the total variances of the relevant variables (e.g., Cain and Watts, 1970).

Table 3. Increments in Variance Explained by AP/UIP in Regressions of Yearly and Total Racial Disorder Frequency on City Characteristics: Non-South Cities *

	Disorder Frequency					
	1965 R ² Change	1966 R ² Change	1967 R ² Change	1968 R ² Change	1969 R ² Change	Total R ² Change
N Nonwhite ^b	.167	.179	.426	.297	.277	.498
City Age	.001	.003	.013	.045	.000	.016
N Unemp	.190	.179	.044	.102	.192	.193
AP/UIP	.012*	.001	.000	.000	.001	.000
Cumulative R ²	.370	.362	.483	.444	.470	.707

* Results are based on 270 cities above 25,000 total population and 1000 nonwhite population in 1960.

^b This variable represented as $\log N + (\log N)^2$.

* Significant at .05 level (reported for AP/UIP only).

the "critical mass" (minimum nonwhite population below which disorder does not occur) and "ceiling" (diminishing rates of increase in disorder probabilities for very large nonwhite populations) effects. Spilerman (1971:436) approximates this form of relationship with the following ordinary least-squares equation: $D = a + b_1 \log N + b_2 (\log N)^2$, where D = disorder frequency and N = Negro (nonwhite) population size. Following Spilerman, we employ this equation for both South and non-South cities which had nonwhite populations greater than one thousand in 1960. Tables 3 and 4 report these nonlinear regression results for the non-South and South, respectively. This specification of nonwhite population is consistently superior to that employed in our first set of regressions (based on analyses of the comparable subset of cities), although this advantage is less pronounced in the year-specific equations. Of interest here, however, is the impact of the wire service variable. For Southern cities, the

increment in variance explained by AP/UIP is small though nontrivial in 1969 only, and negligible for other years and for total frequency. Moreover, the net effect of AP/UIP for the non-South is essentially zero in five of the six regressions; additionally, the regression coefficient of AP/UIP in the equation for 1965 is *negative*. Therefore, these results provide considerable support for our hypotheses concerning the importance of conflict intensity on reporting validity and no evidence for Danzger's argument of bias in studies of racial disorder. These findings also vitiate one of Danzger's (1975:582) major conclusions, that "a new assessment of the number required for a 'critical mass' or a 'ceiling effect' is required in light of the contamination of the data." A more appropriate conclusion is that, once these effects have been properly specified, there is no indication of such contamination. In general, we conclude, as expected, that previous analyses of racial disorders are valid.

Table 4. Increments in Variance Explained by AP/UIP in Regressions of Yearly and Total Racial Disorder Frequency on City Characteristics: Southern Cities *

	Disorder Frequency					
	1965 R ² Change	1966 R ² Change	1967 R ² Change	1968 R ² Change	1969 R ² Change	Total R ² Change
N Nonwhite ^b	.000	.192	.220	.125	.167	.290
City Age	.010	.001	.000	.023	.004	.007
N Unemp	.004	.010	.035	.026	.040	.052
AP/UIP	.000	.000	.001	.003	.027*	.008
Cumulative R ²	.014	.203	.256	.177	.238	.357

* Results are based on 139 cities above 25,000 total population and 1000 nonwhite population in 1960.

^b This variable represented as $\log N + (\log N)^2$.

* Significant at .05 level (reported for AP/UIP only).

*Analysis 2: Collective Protest
(May-October, 1968)*

Data and method. In this second analysis of wire service reporting bias, we employ data gathered by Eisinger (1973) on collective protest events occurring in 43 U.S. cities from May through October, 1968. Eisinger's data are of unique value for our purposes because information on protest events for these cities is based on *daily readings of local newspapers for each city* over this six-month period. This independent source of information therefore allows us to determine which protests were *also* reported in the *Times* according to differential characteristics of events (including their location in AP/UPI versus other cities).

Eisinger selected only cities which had 1968 populations between one hundred thousand and one million persons, in order to reduce distorting effects of size (cf. Eisinger, 1973:15, 28, for precise details on sampling framework and the list of included cities). Given this relatively large minimum size criterion, a high proportion of these cities (29 of 43 = .674) have AP and/or UPI offices. Inclusion of a protest event required some evidence of planning ("marches, sit-ins, demonstrations, pickets, protest meetings or rallies"). Since Eisinger was concerned with the relationship between urban political structure and protests against local government, inclusion criteria were further limited:

only those [events] carried out by two or more individuals and directed at local governmental agencies, officials or institutions were finally coded. The data do not include, then, campus protests or antiwar or anti-draft protests. No instances of rioting, ambushes, shootings, assaults, looting or threatened violence were recorded, even if the perpetrators or their victims attributed such behavior to political motivations. If, however, violence broke out at the site of a peaceful protest and was a direct result of it, this was recorded. (Eisinger, 1973:16)

However, only seven of the total of 120 protest events are violent.

In one analysis of these data, we examined the net effect of AP/UPI on variation across cities in protest frequency (as reported by the 43 local newspapers). This

first analysis by no means constitutes a test of our arguments versus Danzger's; both would expect an absence of reporting bias. However, a positive effect of AP/UPI under these conditions would suggest that Danzger's findings might not reflect differential reporting but, instead, some other factor uncontrolled in his analyses. Although there is a weak zero-order relationship between AP/UPI and protest frequency ($r=0.105$), it is entirely eliminated in every multivariate model estimated (which variously included measures of population size, form of city government, urban occupational structure and a dummy variable for Model Cities Grant during the 1960s). These results suggest that Danzger's findings that AP/UPI biases reports of racial conflict are not spurious.

Our second analysis of Eisinger's data, however, is the strongest test of our intensity-sensitivity model versus Danzger's argument of AP/UPI reporting bias. We originally attempted to use the *Times Index* to determine which of Eisinger's protest incidents also were reported in the *Times*. However, since information in the *Index* was too sketchy, assistants read through the *Times* itself for each day of the six-month period in order to "match" reports to Eisinger's events. After this matching procedure was completed, we could analyze reporting probability as a function of event intensity components (size, duration and violence from Eisinger's local newspaper data on each protest) and whether or not the incident occurred in an AP/UPI city. Specifically, we estimate the following equation: $NYT = a + b_1S + b_2D + b_3V + b_4W$, where NYT = a dummy variable scored one if an event was reported in the *Times* and zero otherwise; S = size (total number of participants, in hundreds); D = duration in days; V = a dummy variable for violence (1=yes; 0=no); and W = a dummy variable for wire service location (1=protest occurred in an AP/UPI city; 0=no wire service). We recognize the limitations in time, space and form of these protest events. However, these analyses are particularly important insofar as, to our knowledge, there are no previous empirical investigations of the de-

terminants of event-reporting probabilities.²²

Findings and discussion. One of our most important findings is the descriptive point that only 22 of Eisinger's 120 protests (18.3 percent) were also reported in the *Times*. Moreover, of all protests in which race of participants was identified (95 of the 120 cases), 73 percent involved either all black (52 percent) or mixed black and white (21 percent) participants, which would qualify them (had they occurred earlier) for Danzger's sample of racial conflicts. Since it is highly implausible that racial disorders were so underreported (i.e., only about one in five), this finding is consistent with our argument that differences in event intensity according to the form of conflict (protests versus violent disturbances) strongly affect reporting probabilities.²³

However, even within the limited (and relatively low) intensity range of the single form—protest behavior—we expect that intensity components will determine reporting validity. Equation (1) in Table 5 presents metric (b) and standardized (B, or beta) partial regression coefficients for the model described above.²⁴ In this case, the partial coefficients are preferable to the net increments to explained variance procedure used earlier because, since the occurrence of protest is now a given, there is no need to

control for substantive sources of frequency before entering AP/UIP. Raw coefficients (multiplied by 100) may be interpreted as the net percentage increase or decrease in event-reporting probabilities per unit increase in the independent variable. We also note that the regressions are based on 91 protests for which data on all variables are available.²⁵

The results shown in equation (1) of Table 5 are highly consistent with our arguments. Net of the three event intensity components (size, duration and violence), the effect of a protest's location in AP/UIP cities on its probability of being reported in the *Times* is very small and in the wrong direction.²⁶ Moreover, the coefficients of all three components of intensity are in the expected positive direction. In the case of size and duration, they are also of substantial magnitude (we attribute much of the weak impact of violence to its extremely small variance in these data). Consequently, these findings show the importance of event characteristics in determining their reporting probabilities (validity). The results also strongly challenge Danzger's emphasis on AP/UIP as the only important influence, as well as his (implicit) assumption that all conflict events are homogeneous with respect to reporting validity.

This is not to say, however, that the *Times* reports are entirely valid net of event intensity characteristics. We noticed that very few protests occurring in Western cities appeared in the *Times*, which suggests

²² In making this claim, we are well aware of previous studies by White (1950), Breed (1958) and Molotch and Lester (1975). However, the former two deal with selectivity of reports according to topics rather than event characteristics. The latter analyzes extent and type of coverage of aspects of a single event.

²³ Our previous analyses of racial disturbances strictly test only the hypothesis that disorders were not differentially reported across cities. However, we constructed that hypothesis on the assumption of complete reporting, on which the above statement is also based.

²⁴ The dummy dependent variable violates assumptions of disturbance homoscedasticity (Goldberger, 1964:249). Although the coefficients themselves are unbiased estimates (Ashenfelter, 1969:645), we do not report significance tests since standard errors would be biased under these conditions. However, our arguments do not depend on such tests; comparisons of the standardized regression coefficients are appropriate for this purpose.

²⁵ Eisinger (1973) reports the size of events in eleven categories. We recoded this variable as the midpoint of each of his size intervals, and assigned an arbitrary value of 2500 participants to the two cases which fell in the "2001 or more" category. We also relied on the *Times* reports for participants estimates in three cases where this variable was "undetermined" in Eisinger's data. Eisinger coded duration in five categories corresponding to one, two, three, four and five or more days, which slightly attenuates its variance and, probably, its effect on reporting bias in our analyses.

²⁶ Seymour Spilerman suggested (personal communication) an explanation for the slight, but counterintuitive, negative effect of AP/UIP in both this and the earlier analyses of disorders. Since AP/UIP offices and greater levels of conflict both are found disproportionately in larger communities, a given event should be marginally less newsworthy in wire service cities.

Table 5. Dummy Variable Regression Analyses of *New York Times* Protest Reporting Validity on Indicators of Event Intensity and Media Bias, May-October, 1968^a

Dependent Variable: Protest Reported in <i>Times</i> (1 = yes; 0 = no)	Size		Duration		Violence		AP/UPI		West		South	
	b	B	b	B	b	B	b	B	b	B	b	B
Equation (1)	.040	.336	.092	.246	.086	.050	-.032	-.033	-.250	-.291	-.030	-.023
Equation (2)	.028	.268	.076	.204	.025	.015	.004	.004				

^a See text for definition and measurement of variables.

a regional bias in coverage. Therefore, we created two dummy variables for regional location of cities: West (scored one for protests in cities west of the Mississippi River and zero otherwise) and South (1 = Census South; 0 = non-South). As the negative coefficients of these variables in equation (2) indicate, there is regional bias in the *Times* coverage, although underreporting is substantial only for protests occurring in Western cities.²⁷ Although inclusion of the regional dummy variables slightly reduces the effects of intensity components and AP/UPI from those presented in equation (1), our substantive conclusions concerning these indicators are not altered. These results show that validity depends upon both the intensity of events and the sensitivity of the reporting media, although in this case differential sensitivity is based on region rather than AP/UPI location. More generally, these findings provide strong support for the conflict intensity-media sensitivity model of validity in newspaper data developed in this paper.

SUMMARY AND CONCLUSIONS

We began this paper by challenging Danzger's (1975) conclusion, based on his analysis of racial conflict, that empirical studies which rely on newspaper data are biased. In particular, we doubted (1) Danzger's assumption that all forms of conflict are homogeneous and therefore equally likely (for any given type of communications network) to be reported; (2) his consequent treatment of validity as solely dependent on the reporting mechanism; (3) his claim that other analyses, especially those of disorder frequency, are invalid. Our findings in two discrete analyses—of

²⁷ Given the substantial effect on reporting of region in this analysis, we also included the West variable in our previous investigation of racial disorders, but found its influence to be negligible. This finding provides further support for our argument that racial disorders were intense enough to be reported even if they occurred in places for which media sources were relatively insensitive. We also tested for bias according to the race of participants and other characteristics of these protest events, but found these factors to be unimportant.

collective racial violence across 673 U.S. cities from 1965 to 1969 and of collective protest occurring in 43 cities during May-October, 1968—sharply contradict each of Danzger's points. Both analyses indicate that event characteristics are also important determinants of validity in news data, that conflict behaviors are differentially likely to be reported according to their intensity, and that previous studies of racial disorders are unbiased.

These results were expected from the alternative model of validity in conflict data developed in this paper. In this model, we specify validity as a function of event intensity and media sensitivity. Event intensity is dependent on size, duration and violence components. Intensity determines the relative probabilities that different forms of conflict behavior will be reported. Precise reporting probabilities, however, cannot be determined in the absence of information concerning media sensitivity to conflict events. Media sensitivity is dependent on "political climate" and on contextual characteristics of events (including their locational proximity to relevant reporting networks). Unbiased conflict data will be obtained when the probability that the least intense form of included behavior will be reported by the least sensitive media source exceeds a given validity criterion level. When forms of conflict behavior do not meet this validity criterion, they vitiate the theoretical advantage of generalization to a broader range of conflict which presumably follows from their inclusion. Our model specifies that the "multiple-indicator" strategies for measuring conflict which prevail in cross-national studies (e.g., Feierabend and Feierabend, 1966; Gurr, 1968; Hibbs, 1973; Rummel, 1963; Tanter, 1966) are particularly likely to be biased. Conversely, "form-specific" studies are generally valid, especially insofar as they limit consideration to behaviors of relatively high intensity.²⁸

²⁸ In some cases, multiple indicator studies also place limitations on event qualification criteria (e.g., Gurr, 1968, purports to restrict events to those with more than one thousand participants). Although this procedure reduces validity problems, it rarely eliminates them, given the range of included event types (e.g., Gurr includes

We recognize the major limitation of this conflict intensity-media sensitivity model of validity in news and other sources of conflict data. It cannot *a priori* specify precise levels of validity for different forms of conflict behavior. Consequently, we do not, in any sense, propose that our model is an adequate substitute for location of independent data sources and direct verification of validity (as we have done for collective protest events). However, such procedures are costly and, in many cases, impossible. Moreover, they are unlikely to be undertaken for the large number of data sets on which previous empirical analyses are based. Given these practical constraints, our model is particularly valuable insofar as it specifies clear guidelines for assessing the validity of past research and for designing future empirical analyses of conflict.

Finally, our findings are relevant to more general issues concerning bias in newspaper reports. Although we do not address work on validity in content, some analyses of media bias extend their treatment of printed news as social construction to selectivity issues. For example, White's (1950) case study of a wire editor's decisions presents strong evidence that the subject's personal feelings influence content, but erroneously concludes that they also determine his choice of topics.²⁹ Similarly, Molotch and Lester (1974:110) emphasize "... the fact that all events are socially constructed and their 'newsworthiness' is not contained in their objective features." Our analyses of conflict data challenge that view since our results clearly indicate the importance of objective features as determinants of reporting bias. Validity in newspaper data must be considered as a function of *both* media

localized rebellions, mutinies, plots and purges which are plausibly underreported for many locations). To some extent, explicit incorporation of assumptions concerning measurement error could reduce the validity problems in multiple-indicator analyses, but such techniques are consistently absent from empirical work in this area.

²⁹ The subject was instructed to save all wire copy during a seven-day period and note reasons for each item rejected. Despite White's conclusion of biased selection by topic, we compute a remarkably high correlation of .96 between the column inches of wire copy received and those actually published on thirteen topic areas (data from White, 1950:385).

bias and the objective characteristics of events themselves.

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WORK VALUES AND JOB REWARDS: A THEORY OF JOB SATISFACTION*

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This paper attempts to develop a theory of job satisfaction which incorporates differences in work values and perceived job characteristics as key explanatory variables. It empirically examines the relationship between job satisfaction and the work values and job rewards associated with six dimensions of work—intrinsic, convenience, financial, relations with co-workers, career opportunities and resource adequacy. It is found that work values have independent effects on job satisfaction. The extent to which workers are able to obtain perceived job rewards is conceptualized to be a function of their degree of control over their employment situations. The paper also seeks to develop a framework which links the variation in the job satisfactions of workers to the factors that influence the degree of their control over the attainment of job rewards in American society. The analyses in this paper are based on data from the 1972-73 Quality of Employment Survey.

The concept of job satisfaction traditionally has been of great interest to social scientists concerned with the problems of work in an industrial society. Many have been interested in job satisfaction, for example, as a result of a personal value system which assumes that work which enables satisfaction of one's needs furthers the dignity of the human individual, whereas work without these characteristics limits the development of personal potential and is, therefore, to be negatively valued. Thus, it is important to examine these issues in order, hopefully, to improve the work experiences of individuals as an end in itself. Other social scientists have been interested

in this concept because of evidence that has linked the degree of satisfaction with work to the quality of one's life outside the work role—especially one's physical and mental health. Still others were motivated to study job satisfaction out of a desire to improve productivity and organizational functioning by improving the quality of work experiences of employees. While these concerns have their bases in different perspectives, they share the recognition of the importance of the job in the total life experience of the individual and the desirability of a positive work experience.

Three types of explanations historically have been suggested to account for the variation in the job satisfactions of workers. The first has sought to explain this variation solely in terms of the personalities of individual workers and has attempted to establish a relationship between measures of adjustment or neuroticism and job satisfaction (see Vroom, 1964, for a discussion of this line of reasoning). While personality variables undoubtedly have some effect on job satisfaction, such explanations are inadequate because they ignore the association of job satisfaction with characteristics of the job.

A second explanation views variation in job satisfaction solely as a function of differences in the nature of jobs people perform. In the past, this has been the numer-

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ically dominant view and studies employing this type of reasoning generally deal with two sets of variables—one a measure of a work role characteristic(s), the other a measure of job satisfaction—and attempt to establish a causal relation from the former to the latter. There is wide variation in the types of work role characteristics that have been used. Some common ones include characteristics of the organizational structure such as span of control and size (see the summary by Porter and Lawler, 1965), job content factors such as degree of specialization (e.g., Shepard, 1970), economic factors (e.g., see Massie, 1964), social factors, promotional opportunities and hours of work. (Excellent reviews of these types of studies are provided by Vroom [1964] and Herzberg et al. [1957].) Generally, these investigations have found that job satisfaction varies, often considerably, with one or more of these variables. A widely tested theory of the determinants of job satisfaction that utilizes this type of explanation is Herzberg's "two-factor" theory (Herzberg et al., 1959). This type of explanation of the variation in job satisfaction may be characterized as a "structural" one, in that the attitudes of workers are seen as a direct, one-to-one reflection of the structure of the work place. This line of reasoning has had great practical utility since it has suggested to employers ways in which they could increase the satisfactions of workers by manipulating job characteristics that are frequently under their control.

Despite the practical utility of this second type of reasoning, this view raises important theoretical problems that question its usefulness for a thorough understanding of job satisfaction. In particular, it does not consider individual differences in the satisfactions experienced by people with the same job characteristics. Such differences arise not only because people evaluate similar "objective" job characteristics differently, but also from differences in what people seek to obtain from their work.

The view that the satisfaction an individual obtains from a job is a function not only of the objective properties of that job but also of the motives of the individual

was first suggested by Morse (1953). Since that time, the major line of development of this view in sociology has occurred through the writings of British industrial sociologists adhering to the "social action" approach.¹ Leading exponents of this view are Goldthorpe and his associates (1968), who reacted against the attempts of organizational social scientists to study issues of worker satisfaction by adhering to a closed-system model wherein organizations are seen as the relevant context for explaining these issues. They argued that the question of satisfaction from work cannot be thoroughly considered without a knowledge of the meanings that individuals impute to their work activity. Studies within this perspective (e.g., Goldthorpe et al., 1968; Beynon and Blackburn, 1972; Russell, 1975) have contributed to our knowledge of job satisfaction by attempting to establish empirically the ways in which the wants and expectations that people attach to their work activity shape the attitudinal and behavioral patterns of their working lives as a whole.

A social action frame of reference which directs attention to the variety of meanings that individuals impute to their work has great potential for advancing our knowledge of job satisfaction and the quality of work experience of individuals in general. At present, however, this potential may be regarded as only a promise. There has been no successful attempt to systematically and empirically establish the way in which meanings and the various satisfactions that work provides combine to determine job

¹ The classical origins of the social action approach may be traced to Max Weber. He rejected the assumption of any objective meaning attached to actions and wished to restrict the understanding and interpretation of meaning to the subjective intentions of the actor (Gerth and Mills, 1946:58). This approach starts from the principle that if people define situations as real, they are real in their consequences and thus it takes the actors' own definitions of the situations in which they are engaged as an initial basis for the explanation of their social behavior and relationships. Such an approach may be contrasted with those which start with some general and normative psychology (or philosophy) of individual needs.

satisfaction based on a heterogeneous and diverse sample of workers. Also, no adequate conceptualization exists which systematically links these considerations to the factors that affect the attainment of jobs. The present paper attempts to partially fill these gaps in our understanding of job satisfaction.

The objectives of this paper are to conceptualize and empirically examine: (1) the way work values and job rewards combine to influence job satisfaction and (2) the factors that determine the extent to which individuals are able to obtain job rewards. This paper thus seeks to develop a model which links the variation in the job satisfactions of individuals to the factors that influence their degree of control over the attainment of these rewards in American society.²

Data

The data that will form the empirical bases for the arguments to be developed in this paper come from the 1972-73 Quality of Employment Survey. This survey was conducted by the Institute for Social Research of the University of Michigan and is representative of the national employed civilian labor force meeting certain sample eligibility criteria (see Quinn and Shepard, 1974, for a description of the survey and sampling procedures). These data were obtained through personal interviews with 1,496 persons living in housing units within the continental United States and the District of Columbia and consist of the *perceptions* of these individuals regarding characteristics of themselves, of their jobs, etc. The sample appears to be representative in its design and selection procedures and

also in its representation of various subgroups (by sex, race, occupation, etc.) in the population (cf. Kalleberg, 1975:Appendix A).

These data permit the type of comparative analysis necessary for the elaboration of a theoretical model. In contrast to previous studies within the social action perspective (e.g., Goldthorpe et al., 1968; Beynon and Blackburn, 1972), which focus on a particular organizational setting, such data allow the consideration of a wide variety of occupational and organizational factors and thus permit the investigation of a large number of such conditions on workers' values, rewards and satisfactions.

Job Satisfaction

In order to empirically examine the process underlying the notion of job satisfaction, it is necessary first to state explicitly what is implied by this concept. Job satisfaction refers to an overall affective orientation on the part of individuals toward work roles which they are presently occupying. It must be distinguished from satisfaction with specific dimensions of those work roles. This conceptualization implies that job satisfaction is a unitary concept and that individuals may be characterized by some sort of vaguely defined attitude toward their total job situation. To say that job satisfaction is a unitary concept, however, does not imply that the causes of this overall attitude are not multidimensional. Obviously, a person may be satisfied with one dimension of the job and dissatisfied with another. The assumption underlying the present view is that it is possible for individuals to balance these specific satisfactions against the specific dissatisfactions and thus to arrive at a composite satisfaction with the job as a whole (cf. Hoppock, 1935). In line with these considerations, a measure of overall job satisfaction was developed based on the responses of workers to five questions concerning how satisfied they are with their jobs as a whole. These questions included such direct inquiries as "how satisfied are you with your job" as well as such indirect measures as whether the worker would recommend the job to a friend, whether the worker plans to look

² It is, therefore, an attempt to develop a "theory" of job satisfaction, i.e., a set of generalizations that explains the variation in this phenomenon on the basis of the conditions and processes that produce this variation (cf. Blau, 1965). There are two parts to the theory: a "psychological" part, which explains the variation in job satisfaction produced by the interplay between work values and job rewards and a "sociological" part, which relates the variation in job satisfaction to factors that affect one's degree of control over the attainment of job rewards.

for a new job within the next year, whether the worker would take the same job again if given a choice, and how the job measures up to the sort of job the worker wanted when he took it. The resultant scale of job satisfaction was computed as the mean of a sum of the responses to these questions and had a reliability (Cronbach's α) of .77.³ (A detailed discussion of this measure, as well as of all the measures described in this paper, may be found in Kalleberg, 1975.) The use of a multiple-item indicator of this construct overcomes many of the problems associated with single-item measures of job satisfaction (see Kalleberg, 1974).

A worker's level of job satisfaction is a function of the range of specific satisfactions and dissatisfactions that he/she experiences with respect to the various dimensions of work. It is thus "the pleasurable emotional state resulting from the appraisal of one's job as achieving or facilitating the achievement of one's job values" (Locke, 1969). This view of the process underlying the variation in job satisfaction implies that two types of factors are operative: perceived job characteristics, which represent the amount of satisfaction available from particular dimensions of work, and work values, which represent the meanings that individuals attach to these perceived job characteristics. In order to examine this process empirically, it is necessary to develop measures of these factors.

³ Evidence for the unidimensionality of the job satisfaction scale is provided by: (1) a principal components factor analysis of the correlation matrix for the five items comprising the scale, which produced a single-factor solution accounting for 52.6% of the total variation among the items and (2) a principal factor analysis of the correlation matrix for the five general job satisfaction items and the 34 job reward items (to be discussed later). A quartimax orthogonal rotation of the factor matrix produced by this analysis revealed that the five general job satisfaction items factored out separately from the job reward items, suggesting that general job satisfaction is a unitary phenomenon and is distinct from the satisfactions associated with specific dimensions of work. These results provide no support for any "two-factor" notions of job satisfaction (cf. Herzberg et al., 1959). These factor analyses are presented in Kalleberg, 1975.

Work Values

Work may have a variety of meanings for individuals in an industrial society. Work has no inherent meaning but, rather, individuals impute such meanings to their work activity. One way to understand the variety of these meanings is to specify the range of gratifications that are available from work in an industrial society and to assess the degree to which particular individuals value each of these dimensions.⁴ Information regarding the importance of each of 34 job characteristics to the worker was obtained from respondents in the Quality of Employment Survey.⁵ The intercorrelations among these items were factor-analyzed by means of a principal factor procedure, and the factor matrix produced by this analysis was rotated by means of a quartimax orthogonal rotation. Six dimensions of work that are differentially valued emerged from this analysis.⁶ These are:

⁴ In contrast, Goldthorpe et al. (1968) classify workers on the basis of several ideal-type "orientations to work," sets of consciously experienced expectations or goals which represent the priorities one holds with respect to the work activity. Such categorizations do not appear very useful for a comparative analysis since identifying workers as motivated by a single type of reward tends to neglect their valuation and experience with respect to other dimensions. The present view is more general than that suggested by Goldthorpe et al. in that it does not assume the existence of a dominant orientation but treats this as an empirical question. If such an orientation should, in fact, empirically characterize a group of individuals, this would be reflected by their valuing highly certain dimensions of work and not valuing others.

⁵ The worker was asked: "People differ a lot in terms of which of these things are more important to them. We'd like to know how important each of these is to you." The respondent then sorted 34 cards containing names of job characteristics (e.g., the work is interesting, the hours are good, the pay is good, promotions are handled fairly) into four piles, each pile corresponding to a response category (very important, somewhat important, a little important, not at all important). See Quinn and Shepard, 1974, for a discussion.

⁶ The analysis produced 16 factors, accounting for 44.7% of the total variation among the items; only the first six factors, accounting for 85.9% of the total factor variance and 38.4% of the total variance, were interpreted. The factors explained from 6.3 to 44.7 percent of the total

An *intrinsic* dimension, which refers to those characteristics associated with the task itself—whether it is interesting, allows the worker to develop and use his/her abilities, allows the worker to be self-directive and whether the worker can see the results of the work. Valuation of this dimension thus reflects the worker's desire to be stimulated and challenged by the job and to be able to exercise acquired skills at work.

A *convenience* dimension, which refers to job characteristics that provide solid creature comforts, i.e., a "soft" job. These include: convenient travel to and from work, good hours, freedom from conflicting demands, pleasant physical surroundings, no excessive amounts of work, enough time to do the work and an opportunity to forget about personal problems. This dimension may be viewed in opposition conceptually to the intrinsic dimension as it represents a valuation of facets external to the task itself.

While the convenience dimension refers to those aspects of work that are "extrinsic" to the task itself, it does not exhaust the range of extrinsic characteristics that are differentially valued. A second extrinsic dimension refers to the *financial* dimension and includes such items as the pay, fringe benefits and job security. Valuation of this dimension reflects a worker's desire to obtain present and future monetary rewards from a job.

A third extrinsic dimension refers to *relationships with co-workers* and includes such items as whether the job permits chances to make friends, whether co-workers are friendly and helpful and whether one's co-workers take a personal interest in him/her. Valuation of this dimension reflects a worker's desire for the satisfaction of social needs from the work activity.

factor variance and from 2.8 to 20 percent of the total variance. Further, the factor analysis suggested dimensions that replicate those defined by Quinn and Shepard (1974) by means of a cluster analysis of the same items. They labeled their dimensions: challenge, comfort, financial, relations with co-workers, promotion and resource adequacy. The items included in each of the six scales in the present study are the same as those included in the corresponding scales utilized by Quinn and Shepard.

A fourth extrinsic dimension is the opportunities the job provides for a *career*, a dimension that includes such items as whether the chances for promotion are good, whether promotions are handled fairly and whether the employer is concerned about giving everyone a chance to get ahead. Valuation of this dimension represents a worker's desire for advancement and recognition.

The final dimension of work that is differentially valued may be labeled *resource adequacy*. This dimension represents workers' wishes for adequate resources with which to do their jobs well and includes such items as whether the help, equipment, authority and information required for job performance are adequate, whether co-workers are competent and helpful, and whether the supervision is conducive to task completion. This dimension of work may be viewed as being different from the previous ones in that it doesn't refer to what workers "ultimately" want from their jobs. Yet in order to obtain such rewards as money, intrinsic gratifications, advancement, etc., workers must perform adequately in their jobs. Successful role performance is not only contingent upon the demands of a role and the characteristics of the person occupying it, it also may be dependent upon the amount of resources or facilities that are supplied to the role occupant. Thus, a worker may be concerned with and value not only the ultimate rewards provided by the job, but may be equally concerned with the more immediate problem of securing resources sufficient for adequate performance in the work role (cf. Quinn, 1972).

After identifying the basic dimensions underlying the intercorrelations among the 34 importance ratings, scales measuring each of these six dimensions were developed. Scales were constructed by taking the mean of the unweighted scores on component items, thus each scale has a range from 1.0 (low valuation) to 4.0 (high valuation), despite the fact that scales have different numbers of items. Missing data on a particular item were assigned the mean of the cases present on that item. The reliability estimates for these scales, the num-

Table 1. Correlation Matrix and Reliability Estimates (Cronbach's α) for the Work Value and Job Reward Scales: 1972-73 Quality of Employment Survey, N = 1496

Scale	1	2	3	4	5	6	7	8	9	10	11	12
1. Intrinsic valuation	1.000											
2. Convenience valuation	.248	1.000										
3. Financial valuation	.235	.447	1.000									
4. Co-workers valuation	.355	.423	.262	1.000								
5. Career valuation	.426	.241	.415	.308	1.000							
6. Resource adequacy valuation	.464	.551	.469	.557	.495	1.000						
7. Intrinsic rewards	.402	.023	.024	.167	.091	.185	1.000					
8. Convenience rewards	.165	.309	.134	.264	.114	.291	.405	1.000				
9. Financial rewards	.163	.030	.153	.090	.086	.111	.336	.347	1.000			
10. Co-workers rewards	.247	.178	.110	.456	.143	.326	.451	.456	.253	1.000		
11. Career rewards	.203	.024	.073	.164	.205	.160	.431	.334	.438	.390	1.000	
12. Resource adequacy rewards	.208	.198	.121	.298	.112	.380	.488	.586	.294	.621	.496	1.000
Cronbach's α	.72	.76	.75	.68	.72	.85	.83	.68	.68	.73	.75	.87
Number of items in scale	6	7	3	4	3	11	6	7	3	4	3	11

ber of items included in each scale and the intercorrelations among the scales are presented in Table 1.⁷

The concept of "work value" may be regarded as a special usage of the general concept of "value" and may be defined as the conceptions of what is desirable that individuals hold with respect to their work activity.⁸ Work values reflect the individual's awareness of the condition he seeks from the work situation, and they regulate his actions in pursuit of that condition. They thus refer to general attitudes regarding the meaning that an individual attaches to the work role as distinguished from his satisfaction with that role. The term should be distinguished further (but in common usage often is not) from related concepts that have been used previously in the literature for similar purposes. For example, values should be distinguished from expectations, which denote one's beliefs about what will occur in the future; but what is expected may not correspond to what is wanted and, conversely, what is valued may or may not correspond to what is expected. In addition, values should be distinguished from needs, which refer to the objective requirements of an organism's well-being. A value presupposes an awareness, at some level, of the object or condition sought while a need does not. The two concepts are closely related since individuals may value those factors associated with a job which satisfy their needs; but values also may be irrational and whether or not one's values correspond to his needs, it is his values which regulate his actions and determine his emotional responses. (For a

⁷ The intercorrelations among these six work-value scales were factor-analyzed (principal components factor analysis) in an attempt to discover any "higher-order" dimensions of work values that may account more parsimoniously for the covariation among them. A single factor solution was obtained, accounting for 49.2% of the total variation, suggesting that these dimensions are most usefully considered as constituting separate dimensions of work that are differentially valued.

⁸ The general definition of values from which this definition is adapted is discussed in Williams (1968). The present usage of the term "work values" corresponds to what Kohn (1969) has previously called "judgments about work" or "work orientations."

discussion of these and related issues, see Locke, 1969.)

Job Rewards

These dimensions of work that are differentially valued constitute potential sources of rewards to the worker. While valuation of particular job characteristics may, in certain cases, correspond to the availability of those characteristics, such correspondence is by no means certain in an industrial society. Given that the characteristics associated with jobs are determined relatively independently of individuals, the distribution of work values at a given point in time may not be assumed to match the distribution of opportunities for the satisfaction of these values. In order to understand the variation in workers' job satisfactions, it is necessary to consider not only the values that individuals have toward work but the types of rewards that are available as well.

Though a number of methods may be used to measure the types and amounts of rewards people receive from jobs, the relevant procedure in the present context is to ask the *worker* about the job. As Hackman and Lawler (1971) have argued, it is not the "objective" state of these characteristics that affects employee attitudes and behavior, but how they are experienced by the worker. Accordingly, information regarding "how true" each of the same 34 characteristics was for the respondent's job, which was also obtained in the Quality of Employment Survey,⁹ will be used to represent the perceived levels of the various job characteristics. The intercorrelations among these items were analyzed by means of a principal factor procedure, and the factor matrix produced by this analysis was ro-

tated by means of a quartimax orthogonal rotation.¹⁰ The six dimensions discussed previously also were found to underlie the correlations among the reward items.¹¹ Scales measuring the rewards¹² associated with each of these six dimensions were developed by taking the mean of the unweighted scores on component items; thus each scale has a range from 1.0 (low reward) to 4.0 (high reward), despite the fact that scales have different numbers of items. Missing data on a particular item were assigned the mean of the cases present on that item. The reliability estimates for these scales, the number of items included in each scale, the intercorrelations among the reward scales and the intercorrelations among the value and reward scales are presented in Table 1. It should be noted

¹⁰ The analysis produced 15 factors, accounting for 47.7% of the total variation among the items; only the first six factors, accounting for 88.4% of the total factor variance and 42.2% of the total variance, were interpreted. The factors explained from 5.4 to 42 percent of the total factor variance and from 2.6 to 20 percent of the total variance. The results with respect to the reward items also replicated the results obtained by Quinn and Shepard (see footnote 6).

¹¹ The intercorrelations among these six job reward scales were analyzed by means of a principal components factor analysis in an attempt to discover any "higher-order" dimensions of job rewards that may account more parsimoniously for the covariation among them. A single factor solution was obtained, accounting for 52.7% of the total variation, suggesting that these dimensions are most usefully considered as constituting separate dimensions of work that constitute sources of satisfaction.

¹² It may be argued that some "rewards" (i.e., convenience and resource adequacy) are most usefully considered as representing "costs" (with signs reversed). That is, high convenience and adequate resources represent, in a very real sense, decreased costs rather than increased rewards. This conceptual distinction does *not* imply the acceptance of a "two-factor" theory of satisfaction. Herzberg et al. (1959), for example, argue that rewards (job content) lead to job satisfaction while costs (job context) lead to job dissatisfaction. Similarly, Bradburn (1969) argues that avowed happiness is a function of one's positive rewards less his costs. These frameworks are based on the assumption that rewards and costs are independent of each other and functions of different sets of variables. In the present study, no evidence was found to suggest that the six "reward" variables represent two distinct dimensions (see footnote 11).

⁹ Near the end of the interview, the respondent was asked: "Here are some cards that describe different aspects of a person's job. I'd like you to put each white card below the pink card which best reflects *how true* you feel each is of *your* job." The respondent then sorted 34 cards containing the names of the same 34 job characteristics into four piles, each pile corresponding to a response category (very true, somewhat true, a little true, not at all true). See Quinn and Shepard, 1974.

further that the same job characteristics used in a particular valuation scale were used in the corresponding reward scale.

It should be clearly understood what these "job reward" measures do and do not represent. They are evaluative judgments on the part of respondents concerning features of their jobs (e.g., the pay is good, the work is interesting); in this sense, they may be regarded as representing measures of satisfaction with the various dimensions of jobs. They *do not* represent "objective" properties of jobs. The correspondence between actual and perceived properties of jobs constitutes a much-needed area of research, since it would shed light on the crucial policy-related question of how much of the variation in job satisfaction is produced by "objective" as opposed to "subjective" factors. While the present data preclude a serious examination of this issue, there is evidence to suggest that such ratings of job characteristics made by individuals do not appear unrealistic in light of our knowledge of occupational realities (see the discussion of this issue in Kalleberg, 1975:ch. 4).

One might argue that the value and reward dimensions are not independent, mainly because of instrument effect and weakness of measurement. Despite their spatial separation in the interview schedule, information regarding values and rewards was obtained from the same interview and these scales contain the same

items with different questions. One might also argue that values and rewards do not represent independent dimensions because people typically do not make a distinction between them. In order to assess the validity of these arguments, the intercorrelations among the six work-value and the six job-reward scales were factor-analyzed. The results of this analysis are presented in Table 2. Factor I consists of similarities among the job-reward measures, while Factor II involves the relationships among the work-value scales. While the analysis suggested that there may be a "methods bias" in the relationship between the intrinsic value and reward scales (Factor III) and between the co-workers scales (Factor IV), the loadings of these scales on these factors are relatively small, as is the amount of variation explained by these factors. The general conclusions suggested by Table 2 are that the reward and value scales represent independent constructs and that the amount of instrument effect is small.

Effects of Values and Rewards on Job Satisfaction

It has been argued that job satisfaction is a function of both work values and job rewards. Such a statement does not, by itself, constitute a great advance toward an understanding of the nature of the process underlying the concept of job satisfaction. A theoretical understanding of the nature

Table 2. Quartimax Rotation of Factor Matrix Produced by Principal Factor Analysis of the Correlation Matrix for the Work Value and Job Reward Scales: 1972-73 Quality of Employment Survey, N = 1496*

Scale/Factor	I	II	III	IV	V	VI	h ²
Intrinsic valuation	—	.454	.453	—	—	—	.481
Convenience valuation	—	.647	—	—	—	—	.511
Financial valuation	—	.610	—	—	—	—	.418
Co-workers valuation	—	.557	—	.329	—	—	.498
Career valuation	—	.576	—	—	—	—	.452
Resource adequacy valuation	—	.780	—	—	—	—	.682
Intrinsic rewards	.652	—	.314	—	—	—	.526
Convenience rewards	.630	—	—	—	—	—	.509
Financial rewards	.503	—	—	—	—	—	.346
Co-workers rewards	.656	—	—	.316	—	—	.574
Career rewards	.638	—	—	—	—	—	.473
Resource adequacy rewards	.764	—	—	—	—	—	.668
Percent Total Variance	22.5	19.8	3.3	3.2	2.0	.4	
Percent Total Factor Variance	43.9	38.6	6.5	6.3	3.8	.8	

* Factor loadings less than .3 are arbitrarily ignored.

Table 3. Coefficients Describing the Relationship between Job Satisfaction and Work Values and Job Rewards: 1972-73 Quality of Employment Survey, N = 1496

Dimension	(1) ^a	(2) ^b	(3) ^c	(4) ^d	(5) ^e
Intrinsic					.323**
Value	.057	-.169	-.245**	-.064**	
Reward	.493	.561	.486**	.692**	
Convenience					.059**
Value	-.038	-.158	-.069	-.019	
Reward	.339	.388	.091*	.146*	
Financial					.213**
Value	-.024	-.087	-.030	-.004	
Reward	.396	.410	.262**	.274**	
Co-workers					.033
Value	.054	-.119	-.035	-.006	
Reward	.323	.378	.058	.064	
Career					.088**
Value	-.054	-.135	-.094**	-.034**	
Reward	.364	.392	.077**	.190**	
Resource Adequacy					.139**
Value	.054	-.117	.044	.001	
Reward	.405	.450	.233**	.243**	

^a Zero-order correlations of values and rewards with job satisfaction.

^b Regressions of job satisfaction on values and rewards associated with a particular dimension separately (standardized coefficients). All coefficients are statistically significant at a p-value less than .001.

^c Unstandardized coefficients from the regression of job satisfaction on all values and rewards simultaneously (equation (1) in the text). $R^2 = .364$; constant = 1.680.

^d Unstandardized coefficients obtained from estimating equation (2) in the text. For each dimension, the "reward" coefficient corresponds to the R_i term in equation (2), while the "value" coefficient in the table corresponds to the coefficient for the $V_i R_i$ term. $R^2 = .353$; constant = .330.

^e Standardized coefficients from the regression of job satisfaction on the six "specific satisfaction" variables.

* $p \leq .05$.

** $p \leq .01$.

of this process requires a precise specification of the manner in which particular values and rewards combine to influence overall job satisfaction.

Clues regarding the interplay among values, rewards and job satisfaction are provided initially by an examination of their zero-order correlations. The correlations between job satisfaction and each value and reward are presented in the first column of Table 3. Rewards are positively and relatively highly correlated with job satisfaction, a result probably explained by the fact that the greater the perceived rewards one obtains from these specific dimensions of the job, the greater the satisfaction with the job in general. Values, on the other hand, are correlated near zero with job satisfaction. This might suggest that rewards may be "suppressing" the association between values and job satisfaction, which means it

is necessary to control for one of these variables when examining the effects of the other.

The second column of Table 3 presents the standardized coefficients obtained from regressing job satisfaction on both the values and rewards associated with a particular dimension of work. The pattern of these results supports the interpretation of a suppressor effect—namely, that by removing the positive association of values and job satisfaction produced by the positive correlation of values and rewards, the negative net effect of values on job satisfaction is revealed. When each dimension is considered separately, rewards have a large and positive effect on job satisfaction while values have a smaller but statistically significant (beyond $p = .001$) negative effect on job satisfaction.

In order to examine the net effects of

these values and rewards on job satisfaction, the latter was regressed on all values and rewards simultaneously. The results of this regression are presented in the third column of Table 3. These findings suggest that rewards generally have greater effects on job satisfaction than do values; rewards, furthermore, have positive net effects on job satisfaction while values generally have negative net effects. While some values do not have significant net effects on job satisfaction, a consideration of these factors constitutes an empirical as well as a theoretical improvement over a model which includes only rewards.¹³

The model underlying the coefficients in columns 2 and 3 is an *additive* one. The effect of an increase in the level of a perceived job characteristic is always positive and does not depend on the strength of one's desire for that reward, while the effect of an increase in valuation on job satisfaction is always negative and does not depend on the availability of the characteristic. Furthermore, it assumes that these effects are *linear*. This model (from Table 3, column 3) can be represented as:

$$JS = a + \sum_{i=1}^6 b_i R_i + \sum_{i=1}^6 c_i V_i + e \quad (1)$$

where JS is job satisfaction, R stands for a type of reward, V stands for the value or importance of that type of reward and *i* varies over the six types of rewards and values. In this model, the effects of rewards on job satisfaction (the b_i) are the same regardless of how important workers consider these rewards to be. However, for any given level of a reward, there will be variation in job satisfaction produced by the

variation in the valuation of that reward; these differences are represented by the c_i . The c_i are negative because for a given level of rewards (i.e., holding rewards constant), the more one values those rewards the more likely it is that these values are not fulfilled.¹⁴ The model implies that, in the aggregate, the highest levels of job satisfaction will be experienced by those workers with high rewards and low values, while the lowest levels of job satisfaction will be experienced by those workers with low rewards and high values.

While additive models constitute a major way that the relationship of job satisfaction to its components has been operationalized (Vroom, 1964), researchers holding this view typically have described this process by means of a difference score (e.g., Kuhlén, 1963). These models differ from equation (1) in that the effects of rewards and values on job satisfaction are assumed to be equal (though opposite in sign). This assumption amounts to a constraint on the coefficients of R and V which forces them to be equal, a constraint which has little theoretical or empirical justification. As the results in columns 2 and 3 of Table 3 show, rewards tend to have greater effects on job satisfaction than do values. Thus, it is not the correspondence of values to rewards that produces job satisfaction (if by "correspondence" we mean similarities in absolute levels of the variables); rather, both values and rewards have independent (but unequal) effects on job satisfaction.

One might argue that equation (1) is not an appropriate representation of the manner in which values and perceived job characteristics combine to influence overall job satisfaction. In particular, one might dispute the assumption that values have independent effects on job satisfaction and argue that a model in which values interact with rewards is more appropriate.¹⁵ Inter-

¹³ The R^2 for the present model is .364; the R^2 for a model containing only rewards as independent variables is .328. For some dimensions (i.e., financial and resource adequacy), rewards have a significant effect on job satisfaction while values do not. That is, workers who perceive they obtain greater financial rewards will tend to be more satisfied regardless of their values, and the absence of characteristics implied by resource adequacy such as role ambiguity, poor supervision and incompetent co-workers are oppressive job conditions for nearly everyone, regardless of their values.

¹⁴ Morse (1953:28) also suggested an additive view of the interplay among values, rewards and job satisfaction when she argued: "the greater the amount the individual gets, the greater his satisfaction and, at the same time, the more the individual still desires, the less his satisfaction."

¹⁵ One might also accept the additive form of the model, but argue that the assumption of

action models constitute the second major way that the relationship of job satisfaction to its components has been operationalized (e.g., Vroom, 1964; Schaffer, 1954; Hackman and Lawler, 1971; Wanous, 1974), although there has been no attempt in these studies to compare the explanatory power of the interaction models to that of additive models. While there are a large number of ways in which interactions between values and rewards in determining job satisfaction may be specified, a theoretically attractive version is represented by:

$$JS = a + \sum_{i=1}^6 b_i R_i + \sum_{i=1}^6 c_i V_i R_i + e. \quad (2)$$

Here the b_i represent the effects of the six perceived job characteristics on job satisfaction when the worker considers them relatively unimportant, and the c_i represent the additional effects of the same rewards as the value of the rewards to the worker increases. In contrast to equation (1), this model implies that the effects of job rewards on job satisfaction depend on the levels of valuation of the rewards.

The fourth column of Table 3 reports the coefficients obtained from estimating equation (2). Three factors suggest that equation (2) is less appropriate than equation (1) for representing the relationships among values, rewards and job satisfaction. First, equation (1) explains more of the variance in job satisfaction than equation

(2) (.364 versus .353). Second, the c_i in equation (2) are *negative*, whereas the reasoning underlying this model would have assumed the c_i to be positive. That is, an interaction model such as equation (2) assumes that highly valued rewards have greater effects on job satisfaction than rewards that are unimportant; equation (2) yields empirical results that contradict this assumption. Finally, equation (2) is also problematic in that the collinearity between the R_i and $V_i R_i$ terms for each dimension is very high (from .77 to .90); this makes it difficult to distinguish the respective effects of these terms. Therefore, equation (1) will be considered to be a more appropriate representation of the manner in which values and rewards combine to influence overall job satisfaction, at least in these data.¹⁶

The above analysis has suggested that work values have independent effects on job satisfaction. One might conclude from this that characteristics of jobs do not interact with work values to produce satisfaction; this would be incorrect. It is important to remember that the "job-reward" measures discussed in this paper are *perceptions* of job characteristics. The relationship between actual and perceived job characteristics constitutes a source of individual differences in satisfaction with specific dimensions of jobs that are not considered in this paper. The possibility remains very plausible that values (or related concepts, such as needs) interact with the "objective" job characteristics to produce evaluative judgments about those characteristics. For example, individuals are likely to develop judgments about or even to merely *notice* characteristics of their jobs to the extent to which the individuals value these char-

linearity implied by equation (1) is inappropriate. Equation (1) implies that having more than an adequate opportunity for the achievement of a particular value (i.e., cases where $J > V$) increases job satisfaction. One might argue that such opportunities would be frustrating to the individual low on the particular value if, along with the opportunity, colleagues or superiors expect or demand a high level of motivation of the particular type. The veracity of the linearity assumption within the context of an additive model was examined by comparing the R^2 values for models of the form of equation (1) with the R^2 values for two alternative specifications of an additive model which did not imply linearity (1) $JS = a + b|R - V| + e$ and (2) a model where the V and R for each dimension were divided into three categories each. Equation (1) yielded higher R^2 values in both cases. For an elaboration, see Kalleberg, 1975.

¹⁶ Additive models of the form of equation (1) are compared with a number of alternate specifications of models incorporating interactions between values and rewards in Kalleberg (1975). One such model combined the features of equations (1) and (2); i.e., $V_i R_i$ terms were added to equation (1). As was the case with equation (2), the very high collinearity between the R_i and $V_i R_i$ terms made it impossible to isolate their respective effects on job satisfaction in any meaningful way.

acteristics. Thus, the actual responses to the "rewards" questions are quite possibly the product of the "true" scores on these questions and the extent to which the respondent values the reward in question. Under these conditions, the job-reward measures *already* represent the interactions between the true job characteristic scores and values.¹⁷ An examination of this argument is not possible here, since the relevant information to test it consists of perceptions of individuals in the same or very similar jobs. While the perceptions of job characteristics are the appropriate data for understanding job satisfaction, a knowledge of how these relate to actual job characteristics is necessary for the formation of policy and for answering such questions as why job satisfaction is unequally distributed among the population.

A limitation on the model whose coefficients are presented in column 3 is that while it allows a ranking of the relative effects of the various values and rewards on job satisfaction, it does not permit a ranking of *dimensions*. Since job satisfaction is a function of the range of specific satisfactions and dissatisfactions that one experiences with respect to the six dimensions of work and since these "specific satisfactions" (i.e., the contribution of that dimension to overall job satisfaction) are determined by the combinations of the value and reward variables for these dimensions, it is of interest to assess the relative contribution of the six combinations to overall job satisfaction. How does one obtain a rank-ordering of dimensions? This is possible by representing the data from this model in a slightly different manner.

The strategy of this analysis is to define a new variable, say "specific satisfaction," which is weighted by the unstandardized coefficients in column 3. Satisfaction with the intrinsic dimension, for example, would be defined as: $.486 \times \text{reward} - .245 \times \text{value}$. A high value on the combination implies

high satisfaction with that dimension, a low value implies low satisfaction. (Note that in the cases where a reward has a significant effect on job satisfaction but the value does not—e.g., "financial," the specific satisfaction variable representing that dimension will be almost entirely a function of the level of reward associated with that dimension.) If overall job satisfaction is then regressed on the six specific satisfaction variables, the unstandardized coefficients will naturally be equal to 1.0 since these variables are just another way of combining the information from the value and reward variables. The relevant information from the regression of job satisfaction on the six new variables is contained in their *standardized* coefficients. These coefficients represent the standard deviations of the linear combination of values and rewards associated with each group divided by the standard deviation of the dependent variable; the relative sizes of these standardized coefficients thus indicate the relative effects of the values and rewards associated with the various dimensions on overall job satisfaction.

The standardized coefficients obtained from regressing job satisfaction on the six specific satisfaction variables are presented in column 5 of Table 3. These coefficients represent the relative effects of these six components on job satisfaction over the entire sample of workers.¹⁸ Whether workers obtain intrinsic satisfaction, for example, will have the greatest single effect on their satisfaction with their jobs as a whole. This supports recent speculation that it is the failure of workers to achieve their values with respect to the content of the task itself that is a prime cause of their dissatisfaction with their jobs (e.g., Com-

¹⁷ I am indebted to an anonymous reviewer for this observation. To argue that the values and rewards measures may be "contaminated" by each other is not an argument against equation (1), however, since this model allows for correlations among the V_i and R_i .

¹⁸ The present analysis was carried out on the total sample of workers. As always happens when individual-level data are aggregated, conclusions drawn on the basis of these aggregations may not be applicable to particular individuals. An interesting question for future research is whether the conclusions drawn regarding the relative effects of these components on job satisfaction need to be modified for particular subgroups within the population. This would imply a need for carrying out these analyses separately by sex, race, employment status, etc.

mittee on Labor and Public Welfare, 1972) and suggests the utility of recent attempts to "enlarge" and "enrich" jobs.

Whether or not workers obtain valued intrinsic rewards, however, they still may be relatively satisfied if they receive monetary compensation for their labors. The financial dimension has the second greatest effect on job satisfaction—a result which is not surprising since wages, fringe benefits and job security constitute the major sources of access to almost all goods and services as well as represent measures of the value of one's labor in a high consumption society such as the United States.

In contrast, convenience and relations with co-workers have relatively small net effects on job satisfaction for the total sample of workers. High satisfactions on these dimensions may be regarded as "bonuses" because having opportunities to attain their values with respect to these dimensions will increase workers' satisfactions with their jobs but are not the primary bases on which jobs are evaluated.

A possible alternative explanation for these results should be noted. It could be argued that the differences in the relative sizes of the coefficients reported in columns 3 and 5 of Table 3 are due to differences in the reliabilities of the various value and reward measures. It is unlikely that such an explanation is sufficient to account for these results. As a comparison of the reliabilities of these measures (Table 1) with their relative effects (column 3 of Table 3) shows, the relative effects of these measures do not correspond in any direct way to their relative reliabilities.¹⁹

These results suggest that factors associ-

ated with the intrinsic dimension have the greatest relative effects for producing overall job satisfaction. One might be tempted to conclude from this that such characteristics are most "important" for job satisfaction and that policy should be directed at improving such factors. These kinds of inferences may be misleading. The relative effects of these values and rewards on job satisfaction depend on their relative population-specific variances and, thus, these comparisons are historically specific. For example, a policy designed to improve intrinsic aspects of work could reduce the variation in intrinsic rewards; a subsequent identical study then would find that financial satisfaction has larger apparent "importance" because its variance has increased relatively, even though the causal processes remain unchanged. In fact, it may be that intrinsic work factors have become most important in producing overall job satisfaction in this country in recent years because of previous efforts to reduce the variation in financial satisfactions.

This section has examined the relationship between overall job satisfaction and the work values and perceived job characteristics associated with the various dimensions of work. The remainder of the paper will focus on the factors which influence the degree of satisfaction workers obtain from the six specific dimensions of work. A complete examination of the determinants of these types of specific satisfactions requires *both* an explanation of why people have different levels of job rewards and an explanation of why people with the same levels of rewards have different work values. The latter question will not be treated in this paper for two reasons. First, since specific satisfaction is primarily a function of the perceived level of reward associated with that dimension, it is more important for present purposes to understand why these are unequally distributed. Second, an adequate understanding of the variation in work values is very difficult to obtain with cross-sectional data, since they will change considerably as the person moves through the life cycle. (A discussion of the problems involved in studying the variation in work values with cross-sec-

¹⁹ One could, of course, examine this question directly by simply correcting the correlations among the job satisfaction, value, and reward scales for attenuation and recalculating equation (1). The measurement model underlying this procedure assumes that the errors in the items composing the scales are uncorrelated. This is a rather restrictive assumption, and it is unlikely that there may be some correlation among these items due to invalidity since these attitudinal items were obtained from the same interview. If there is any reason to suspect such correlated error, disattenuated correlations will be unstable and must be viewed with considerable caution.

tional data may be found in Kalleberg, 1975:ch. 3.)

Degree of Control and Job Rewards

The extent to which workers in an industrial society are able to obtain valued job characteristics is highly problematic. Factors associated with the industrial production process separate individuals from the forces that determine characteristics of jobs and, furthermore, such jobs are assigned on the basis of personal characteristics and are achieved by individuals competing for them. This situation is intensified in American society with its stress on economic individualism and the prevailing view that individuals have "inalienable rights" to enjoy a wide range of choice and to compete for better jobs in a free market (Wilensky and Lebeaux, 1965). Workers in such a society therefore may be assumed to seek jobs that conform to their values, though such jobs may not be available given the opportunity structure at a given point in time.

For Marx (1964), this problem of control over work was rooted in the fact that most people did not have control over the products and process of production and, thus, had little power to determine the rewards associated with their jobs. Variation in work experience was seen as a function of the economic and structural factors that determined the distribution of rewards to positions in the society, and it was assumed that rewards could be increased only by changes in these structural determinants. In line with this view, sociological theories of the quality of work experience have focused on comparative organizational and industrial contexts as the explanatory factors that account for the variation in such experience (e.g., Blauner, 1964).

While it is true that workers in an industrial society typically have little control over the distribution of rewards to positions, they do have a certain amount of control over their *attainment* of these positions. A sociological theory of the variation in the quality of work experienced by individuals should not only describe the structural factors that determine the distribution of rewards to positions; it should also explain

the factors that determine the distribution of these rewards to individuals. The latter view differs from the Marxian position in that it takes the structure of job characteristics as given and is concerned with how individuals are distributed within that structure. While a full development of this theory is clearly beyond the scope of this paper, an attempt will be made to suggest the types of factors that are operative.

At least two main sets of factors may be hypothesized to produce variation in the degree of control workers have over their attainment of job rewards in an industrial society. The first is the existing *demand* for the worker's services in the labor market. Workers with a relatively wide range of opportunities should be able to find jobs that provide greater rewards than workers whose range of choice is relatively restricted. In the present paper, this factor is measured by the unemployment rate for a worker's occupation at the time these data were collected (i.e., the first quarter of 1973).²⁰

A second factor affecting degree of control is the amount of *resources* available to the worker. Workers with more resources should have greater power with respect to obtaining job rewards than workers with fewer resources. Resources will be operationalized by four indicators: (1) the length of time the worker has been in the labor force, a proxy for the age of the worker as well as the general skills the worker has accumulated through work experience; (2) the educational attainment

²⁰ The rates for the first quarter of 1973 for the various occupational groups were: professional and technical, 2.2; managers and administrators, excluding farm, 1.5; sales workers, 3.6; clerical workers, 4.3; craftsmen and kindred workers, 3.8; operatives, 6.1; nonfarm laborers, 8.5; service workers, 5.9; farm workers, 2.4. (Source: *Employment and Earnings*, Vol. 20, January, 1974, Table A-45.) These rates are intended to measure a particular aspect of the occupational structure, namely, the structure of opportunities for employment in the various occupations. The unemployment rate is only a rough measure of this, since it includes such things as stability of employment and both voluntary and involuntary unemployment; ideally, one would want purer measures of involuntary unemployment.

of the worker, a measure of the credentials possessed by the worker and such cognitive and noncognitive characteristics related to job performance that are associated with education; (3) the worker's race, a proxy for the likelihood that the worker experienced discrimination in the labor market; (4) whether or not the worker belongs to a union or employee's association, a measure of the organizational resources available to workers to bargain for job rewards in their behalf.

The results of regressing each job-reward measure on the five degree of control measures are presented in Table 4. With respect to demand, the results in Table 4 indicate that the unemployment rate is negatively and significantly related to the attainment of intrinsic, financial and career rewards. The suggested interpretation for these negative relationships is that workers who have greater numbers of job opportunities (i.e., low unemployment) would be more likely than workers with fewer opportunities to have left jobs in which the rewards on these dimensions were perceived to be low.²¹

The relationships of the measures of resources to rewards are more complex. Workers with more experience in the labor force perceive that they have greater rewards than workers with less experience. (Years worked has no relationship to the

career dimension; older workers do not perceive their chances for a career to be greater than do younger workers.) Whites perceive that they attain higher rewards on all dimensions, except the convenience dimension, than nonwhites. Educational attainment, however, is significantly related to rewards only on the financial dimension, a result which might be explained in terms of returns on investment in human capital. That education does not increase workers' attainment of intrinsic rewards may account for why highly educated workers tend to be "over-trained," especially in the beginnings of their careers (cf. Kalleberg and Sørensen, 1973).

Workers who possess resources in the form of membership in a union or employee's association perceive that they have greater rewards with respect to the financial dimension and lower rewards with respect to the intrinsic and resource adequacy dimensions of work. Unions regulate the supply of individuals to jobs and give members more job security. They also bargain with employers for such "bread and butter" issues as wages and fringe benefits. In obtaining such rewards for their membership, however, union leaders traditionally have yielded to employers' control over certain areas of work. According to Salpukas (1974), these "management prerogatives" or nonnegotiable areas historically have included the manner in which the work is structured and the way the plant is run. These considerations may account for the lower rewards union members perceive they have on the intrinsic and resource adequacy dimensions.

The preceding is suggestive of the mechanisms involved in the relationship between degree of control and the attainment of job rewards. An elaboration of this type of reasoning may prove fruitful for the development of a sociological theory of the distribution of job rewards among various groups in the society. In the interest of the development of such a theory, it is instructive to point out the limitations of the preceding analysis. First, it has been assumed that people can maximize *all* six types of rewards. In reality, people do not attain rewards; they obtain *jobs*, which represent

²¹ A possible explanation for the lack of significant relationships between the unemployment rate and the convenience, relations with co-workers and resource adequacy dimensions may be that little information on these characteristics is available before the worker actually enters the job. Consequently, workers may not perceive that their chances for obtaining better relations with co-workers and avoiding the costs associated with inconvenience and inadequate resources are better elsewhere and would not be likely to take advantage of their alternatives for these characteristics. However, another interpretation is possible: given the way unemployment rates were assigned to occupations, it may be that the unemployment rate as utilized here simply reflects the relative availability of these rewards within the occupational structure. (The correlation between the unemployment rate and the Duncan SEI scores for the workers' occupations, for example, is $-.709$.) Thus, one could plausibly argue that the relationships observed simply reflect the fact that some rewards vary more among some occupations than among others.

Table 4. Regression Coefficients for the Effects of the Degree of Control Measures on the Six Types of Job Reward Measures: 1972-73 Quality of Employment Survey, N = 1496^a

Job Reward		Resources				Demand	
		Years Worked since 16	Education	Race ^b	Union ^c	Unemployment Rate	R ²
Intrinsic	3.5694	.178*	-.005	-.082*	-.072*	-.331*	.179
Convenience	2.9224	.0094*	-.0026	-.1783*	-.1065*	-.1215*	.025
		.150*	-.013	-.039	-.045	.017	
		.0065*	-.0049	-.0694	-.0550	.0051	
Financial	2.8192	.110*	.106*	-.115*	.150*	-.072*	.068
		.0066*	.0566*	-.2848*	.2511*	-.0296*	
		.077*	-.045	-.070*	-.031	-.043	
Co-workers	3.3901	.0036*	-.0188	-.1330*	-.0407	-.0137	.016
		.004	.046	-.116*	-.028	-.102*	
		.0002	.0263	-.3022*	-.0502	-.0449*	
Career	2.7543	.066*	-.032	-.071*	-.093*	-.020	.038
Resource Adequacy	3.4040	.0026*	-.0115	-.1170*	-.1034*	-.0056	.020

^a These values were obtained by regressing the reward measure for each dimension of work on the degree of control variables. The first row presents the standardized coefficients, and the second row presents the unstandardized coefficients (including the intercept) obtained from these regressions.

^b Coded: 0 = white; 1 = nonwhite.

^c Coded: 0 = not a union or employee's association member; 1 = member.

* p < .01.

"bundles" of job characteristics and one must often "trade-off" certain satisfactions (e.g., convenience) in order to obtain others. Second, the distinction between job characteristics and how they are perceived has been largely ignored in the above analysis. While this may be defensible in that it makes more sense to assume that people attempt to maximize their satisfaction on each dimension rather than particular job characteristics, it is clear that people attain the *characteristics* when they obtain a job. A key question for future research concerns the kinds of job characteristics that produce variation in these six dimensions of job rewards. The direct measurement of these characteristics will permit an assessment of how much of the variation in the relationship between job rewards and various social groups (defined by education, age, sex, race, etc.) is due to differences in the attainment of particular job characteristics and how much is due to differences in how these job characteristics are *evaluated*. Third, the results presented in Table 4 are only suggestive of the labor market and individual characteristics involved in the attainment of jobs. For example, labor market characteristics are only partly reflected in the level of unemployment. Such factors as the mechanisms by which jobs are distributed in various labor markets, rates of growth in particular occupations and industries, economic and business cycles, etc. are also important. Moreover, resources also include such factors as specific skills, membership in professional societies, level of information regarding job opportunities, occupational inheritance and familial factors involved in the transmission of status, etc. Such factors will be incorporated in future research designed to develop a comprehensive theory of the mechanisms which govern the matching of individuals to jobs in an industrial society.

The argument in this section has implied that rewards are *intervening* variables between workers' degree of control and their job satisfaction. To examine this argument empirically, job satisfaction was regressed first on the degree of control measures. The results of this regression are presented in the first column of Table 5. Next, job satis-

faction was regressed on the degree of control measures *and* the value and reward measures. These results are presented in column 2 of Table 5.

These results suggest that the direct effects of the degree of control measures on job satisfaction are substantially mediated by the reward variables.²² For example, while race, the unemployment rate and years in the labor force have significant effects on job satisfaction in column 1, only the latter has a significant direct effect on job satisfaction when values and rewards are controlled. It should be noted, moreover, that the coefficient for years in the labor force is sharply reduced when values and rewards are entered into the equation. That years in the labor force has a significant direct effect on job satisfaction after controlling for values and rewards may be explained by noting that this measure is highly correlated with age ($r = .90$). Older workers tend to be more satisfied with their lives in general (cf. Sheppard and Herrick, 1972) and since satisfaction with the job may reflect satisfactions experienced in other spheres of life as well,²³ this would account for this direct effect of age.

²² The causal model implied in this paper views job satisfaction as a function of values, rewards and degree of control, and rewards as functions of degree of control. While values and control may be correlated, their causal relationship is not examined here. The reduction in the effects of degree of control on job satisfaction when values and rewards are entered into the equation can best be interpreted as suggesting that rewards mediate the effects of degree of control on job satisfaction. While a portion of this reduction might also be attributed to the presence of values in the equation, the reduction due to values is minimal because: (1) values have relatively small effects on job satisfaction and (2) values and the degree of control measures are not substantially correlated (see Kalleberg, 1975).

²³ It needs to be mentioned that job satisfaction also may be a function of a whole set of nonwork-related factors unique to particular individuals, e.g., family relationships, health, etc. The present model is an attempt to explain as much of the variation in job satisfaction as can be attributed to work-related factors and is based on the assumption that one can understand the true nature of this attitude most effectively by examining such characteristics (cf. Hoppock, 1935). This focus also is useful from a policy stand-

Table 5. Coefficients Obtained from Regressions of Job Satisfaction on Degree of Control Measures and on All Work Values, Job Rewards and Degree of Control Measures Simultaneously: 1972-73 Quality of Employment Survey, N = 1496

Independent Variable	1 ^a	2 ^b
<i>Work Dimensions</i>		
Intrinsic		
Value		-.244**
Reward		.435**
Convenience		
Value		-.075
Reward		.080**
Financial		
Value		-.034
Reward		.252**
Co-workers		
Value		-.034
Reward		.056
Career		
Value		-.077*
Reward		.086**
Resource Adequacy		
Value		.043
Reward		.254**
<i>Degree of Control Measures</i>		
Years worked since 16	.199(.0145)**	.0073**
Education	.025(.0161)	-.0026
Union	-.003(-.0061)	.0078
Race	-.075(-.2254)**	-.0470
Unemployment Rate	-.146(-.0738)**	-.0150
R ²	.077	.374

^a Degree of control measures as independent variables. Standardized coefficients are presented with unstandardized coefficients in parentheses (intercept = 4.070).

^b Degree of control measures, work values and job rewards as independent variables. Unstandardized coefficients are presented (intercept = 1.725).

* $p < .05$.

** $p \leq .01$.

Conclusions

This paper had two major objectives. First, it examined the variation in job satisfaction in terms of both perceived job characteristics and differences in work values. It has been demonstrated that work values have independent and significant effects on job satisfaction. Second, it suggested a framework that links the variation in job satisfaction to the factors that influence workers' attainment of job rewards.

point, since it is the factors that are associated with the workplace that are most readily open to manipulation and change. Still, it must be recognized that such factors can never fully explain the variation in the job satisfactions of individuals.

Two major lines of research are necessary to increase our understanding of the determinants of job satisfaction. First, it is important to address the question of what kinds of people have different values toward work. Variation in work values may be hypothesized to result from three major sets of social factors: (1) socialization and other types of life experiences which occur prior to the individual's entry into the labor force and which shape one's view of the importance of the various dimensions of work; (2) nonwork social roles which impose constraints and contingencies on the types of meanings that the individual can seek from the work activity; (3) work experiences which affect the mature worker's valuation of the potential rewards associ-

ated with work. For example, workers with greater numbers of dependents are more likely to value financial aspects of work, men place greater valuation on the intrinsic dimension of work than women do, etc. (see Kalleberg, 1975:ch. 3). It should be noted that an understanding of the relative effects of these determinants on the various types of work values ideally requires longitudinal panel data, since the values and their determinants may change considerably over time.

Second, it is important to understand the mechanisms by which job rewards are distributed among various social groups. This requires not only an explanation of the factors which govern the distribution of "objective" characteristics to positions, but also an understanding of the ways in which individuals attain these positions and evaluate these job characteristics. One focus of research on this issue that is needed to supplement the present analysis is a series of studies carried out within particular organizations, since the investigation of a fully elaborated theoretical model ideally should be comparative both within and between fully distinct employment situations. Data collected in this way may provide useful information on the role of individual differences in the evaluation of job characteristics, since it would be possible to compare the perceptions of workers in identical jobs. Finally, the relationship of job characteristics, job rewards and job satisfaction to occupational categories and occupational ranking systems (e.g., prestige and socioeconomic scales) needs to be examined. An understanding of why occupations are related to job satisfaction would link the study of the variation in job satisfaction to the vast literature on occupational achievement.

In conclusion, it should be noted that an understanding of the relationship of people to their work can never be accomplished using only the conceptual tools provided by sociological thought. Sociology needs to be supplemented by psychological theory in order to understand the reactions of individuals to their jobs and by economic theory to understand the structural labor market factors that govern the matching

process. Disciplinary barriers historically have constituted major barriers to a comprehensive understanding of these issues; they need to be lowered before the relationship of the individual to the job can be fully understood.

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KEEPING THE FAITH OR PURSUING THE GOOD LIFE: A STUDY OF THE CONSEQUENCES OF PARTICIPATION IN THE CIVIL RIGHTS MOVEMENT *

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This study examines the long-range political consequences of the earliest phase of the student movement—the civil rights movement before 1965. The research focused on a relatively homogeneous group of black and white activists who participated in demonstrations in one of the leading centers of the civil rights movement. A theoretical causal model was developed and tested using nine exogenous and intervening variables to explain adult radical political attitudes and leftist behavior. The model accounts for 45.6 percent of the variance in radical attitudes and 22.2 percent of the variance in leftist behavior. Racial differences in adult politics are explained. Tentative conclusions concerning the consequences of participation in left-wing student politics are presented.

Because of the unpredictable development and the potential for change that social movements generate, the detailed, systematic research on contemporary social movements focuses on the individual and societal antecedents in order to explain what generates discontent and what characteristics distinguish movement members from nonmembers. Although there are a few general works on the effects of movements on their external environment by Gusfield (1963), Parkin (1968) and Hobsbawm (1959) as well as unsystematic studies of the internal development of movements by Bell (1967), Holtzman (1963), Newfield (1966), Zablocki (1971) and Zinn (1964), Wilson (1973) argues there is a serious shortage of data on the consequences of social movements. This shortage is acute when attempting to understand what happens to committed members of social movements.

Killian (1964) and Sherif and Sherif

(1969) argue that what happens to a movement's members is vastly more important than the reasons they first joined a movement. The intense experience of participating in a social movement dramatically alters the subsequent social and political orientation of activists. Not only are activists personally changed, but they try to work for specific social change within society. McCarthy and Zald (1973) suggest that committed members of social movements develop careers as social change organizational intellectuals who have visions of and commitment to societal or institutional change. Similarly, Flacks (1971) states the New Left of the 1960s will begin the long march through the institutions of society as a post-industrial new working class concentrating in the knowledge and human service industries. The two major objectives of this long-range movement are: (1) to challenge the structure of power, prestige and decision making within institutions and professions and (2) to challenge the domination of institutions and professions by corporate, militarist and conservative interests and, instead, compel institutions to serve the interests of the people.

The long-range consequences that McCarthy and Zald (1973) and Flacks (1971) predict are based on the assumption of radicals' highly developed ideologi-

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cal commitments to restructuring the social order. Other movements rooted in tangible self-interest may have different long-range effects. In their separate historical analysis of social movements in the United States, Ash (1972) and Gamson (1975) report the more successful movements have limited reformist goals and are related to their participants' tangible self-interest. The long-range political commitments of participants in movements with limited ideological commitments and limited movement objectives may be less radical. For example, Lipset (1950) finds Canadian farmers are radical when their economic interests are threatened; however, once the movement is successful in protecting the farmers' basic interests, the socialistic program and ideology declines. Similarly, the relatively centralist political orientation of the U.S. labor movement can be attributed to the movement's success in providing a decent standard of living and some economic security.

This study examines the adult politics of former students who were part of the early "radical probe" of the student rebellion in the 1960s (Miles, 1973). The first adult political act of black and white students was direct action to destroy the system of racial segregation. Data were gathered from black and white male activists ten years after they participated in the civil rights movement in one of the movement's leading centers; e.g., Matthews and Prothro (1966) selected the city as one of four to do an in-depth study of black political participation. Militant action started two years after the 1955 Montgomery bus boycott, when black and white students began a bus boycott with identical demands. Black community leaders were among the first to file school desegregation suits. In 1960, black and white students sat-in at lunch counters shortly after the Greensboro sit-ins. In 1963, there were demonstrations led by the Congress of Racial Equality (CORE) to desegregate public facilities that involved numerous arrests and extensive legal battles, which resulted in one major Supreme Court decision (*State of Florida vs. Adgerley*). The extent of civil rights activity can be attributed to the early development

of a group of militant black leaders (Killian and Smith, 1960) and aroused college communities.

Originally, data were gathered by mailed questionnaires from disproportionate stratified samples of activists and nonactivists.¹ Significant differences were found in adult politics within racial groups of activists and nonactivists (Fendrich, 1976). Results reported here will focus on the variance to be explained among black and white activists. This approach has the advantages of a natural experiment, i.e., black and white activists participating in the same political events in one community and demanding, through their civil disobedience, the end of segregation in the South. The control reduces many sources of error that might account for differences in adult politics. Twenty-eight white and 72 black activists returned questionnaires. For both groups, activists were more likely to respond to the questionnaire than members of control groups.² Two sources of bias probably affect the results. The first is the quality of the sampling frames, i.e., the responses are biased toward those respondents for whom the university records were the most accurate.³ In the white sample,

¹ For blacks, questionnaires were sent to those arrested during demonstrations (50%), student government leaders (81%) and a systematic sample of undergraduates (11%). Due to a lack of research funds, 50 questionnaires were sent to each of three groups of white demonstrators, student government leaders and a random sample of university alumni. In cases of overlap between the different strata, those who demonstrated were considered activists.

² The response rates for the whites were: 73% for activists, 69% for student government and 72% for the random sample. The black response rates were: 38% for those arrested, 37% for student government and 36% for the systematic sample. The percentages for whites are substantially better and the percentages for blacks are about the same as a second-wave response rate of a well funded five-year follow-up of college students by El-Khawas and Bisconti (1974). The analysis was limited to 87 activists that provided information.

³ The major reason for the differences in black and white response rates is the quality of the sampling frames. At the predominantly white university, the alumni office was able to provide current addresses for over 80 percent of the former students. The black university did not

key leaders of the student protest were also less likely to respond.⁴

The major questions are: (1) To what extent have activists maintained their political commitments? (2) Are there major differences in the adult politics of black and white civil rights activists? (3) If there are differences, can the relative level of left-wing political participation be explained? In order to answer these questions, a recursive causal model is developed. The model is derived from existing theory, established empirical relationships and assumptions concerning the causal ordering of the variables.

SPECIFICATION OF THE THEORETICAL MODEL

A recursive model explains variance in the endogenous variables of political attitudes and behavior. Path analysis (Duncan, 1966) proves useful when studying processes of groups whose experiences are traced over time. Exogenous variables measuring pre-college, college and post-college experiences are expected to have direct and indirect effects on adult politics. The variables in the model are: (X_1) adult protest behavior; (X_2) adult radical political attitudes; (X_3) occupational values; (X_4) socioeconomic status; (X_5) family obligations; (X_6) career choice; (X_7) graduate education; (X_8) academic ability; (X_9) major in college; (X_{10}) parental socioeconomic status; (X_{11}) race. The simultaneous linear equations for the model are:

$$X_{11} = e_{11} \quad (1)$$

$$X_{10} = b_{10,11}X_{11} + e_{10} \quad (2)$$

$$X_9 = b_{9,11}X_{11} + b_{9,10}X_{10} + e_9 \quad (3)$$

$$X_8 = b_{8,11}X_{11} + b_{8,10}X_{10} + e_8 \quad (4)$$

$$X_7 = b_{7,11}X_{11} + b_{7,10}X_{10} + b_{79}X_9 + b_{78}X_8 + e_7 \quad (5)$$

have the same resources for keeping good records. The majority of addresses for the black sample were obtained from the registrar's office files. Particularly for the black sample, the responses probably are biased toward the less geographically mobile.

⁴Key leaders were determined through interviews with faculty and staff members who had observed the student protest closely. Four out of seven white student leaders did not respond. Six of eleven key black leaders did not respond.

$$X_6 = b_{6,11}X_{11} + b_{69}X_9 + b_{67}X_7 + e_6 \quad (6)$$

$$X_5 = b_{5,11}X_{11} + b_{59}X_9 + b_{57}X_7 + e_5 \quad (7)$$

$$X_4 = b_{4,11}X_{11} + b_{4,10}X_{10} + b_{47}X_7 + b_{46}X_6 + e_4 \quad (8)$$

$$X_3 = b_{3,11}X_{11} + b_{39}X_9 + b_{37}X_7 + b_{36}X_6 + b_{34}X_4 + e_3 \quad (9)$$

$$X_2 = b_{2,11}X_{11} + b_{29}X_9 + b_{27}X_7 + b_{26}X_6 + b_{25}X_5 + b_{24}X_4 + b_{23}X_3 + e_2 \quad (10)$$

$$X_1 = b_{1,11}X_{11} + b_{19}X_9 + b_{17}X_7 + b_{16}X_6 + b_{15}X_5 + b_{14}X_4 + b_{13}X_3 + b_{12}X_2 + e_1 \quad (11)$$

Since the theory is primarily concerned with explaining adult politics, the theoretical discussion will begin with equations (10) and (11). The same direct causal links are expected for both variables, except that radical attitudes also are considered an exogenous variable for leftist behavior.

Race is considered an indirect indicator of ideological commitment among civil rights activists. While blacks and whites were idealistic and committed to the movement (Demerath et al., 1971; Orum, 1973; Warren, 1965), important differences separated them. The self and collective interests of aspiring middle-class blacks were involved (Miles, 1973). They expected to benefit directly from changes the civil rights movement achieved. The Geschwenders (1973) found that Southern black students were motivated by particular types of relative deprivation to participate in the civil rights movement. Relative deprivation is defined as the discrepancy between value expectations (the goals and conditions of life to which one feels entitled) and value capabilities (the goods and conditions one feels capable of getting and keeping). Three types of relative deprivation were found to be related to levels of student participation in civil rights protest—collective black aspirational deprivation, anticipated future aspirational deprivation and reference group satisfaction. Blacks who were the most extensively involved in the civil rights protest felt the greatest discrepancies for blacks collectively at their current position and also felt that this discrepancy would exist in the future. When comparing themselves to other blacks, college activists considered themselves better off. In short, blacks were committed to participate in the

politics of disruption to reduce the discrepancy in value expectations and value capabilities.

The ideological commitment for whites to participate in the civil rights movement was different. They had nothing to gain materially, individually or collectively, from their participation. In contrast to the popular assumption that guilt feelings were the motivational base for white participation, Demerath et al. (1971) found a different pattern for whites who participated in the civil rights movement. Analyzing self-reports of white activists, they found a diffused motivational theme of other-oriented humanism. Comparing white activists to a control group of Wisconsin students who did not participate in the movement, they found no differences on attitudes toward civil rights; however, the white civil rights activists were distinctive in their tendency to be concerned about more political issues in a more left-wing direction than liberal Wisconsin students. They were primed to become involved in social movements that held some promise of moving the society in more equalitarian, democratic and socialistic directions. At that time, the civil rights movement was the only available outlet for their ideological commitments. As Demerath et al. (1971:36) commented, ". . . the civil rights movement offered a structured political identity to those with broad-ranging political concerns." The differences in ideological commitment can be summarized as the differences in interest group politics versus radical ideological commitments to restructure societal institutions.

There are additional causal links that explain adult politics either directly or indirectly through intervening paths. Studies have shown that students majoring in liberal arts, particularly the social sciences and humanities, are further to the left politically than other college majors (Braungart and Braungart, 1974; Flacks, 1967; Lipset, 1972). This difference is expected to persist into adulthood. Post-college experiences also are expected to affect adult politics. Although evidence of the influence of graduate training as a liberalizing or radicalizing experience is mixed (Meier and Orzen,

1971; Lipset, 1971; Kornberg and Brehm, 1971), it is considered an important opportunity to reinforce and develop earlier political commitments because of prolonged exposure to leftist campus politics during the 1960s and because of the further development of knowledge and skills, particularly in liberal arts programs, to critically assess societal institutions.

Research suggests that college graduates who have pursued careers in the public sector of the economy, especially the rapidly expanding knowledge and human service industries, are more likely to have leftist commitments than those who enter the private sector of the economy (Fendrich and Tarleau, 1973; Krauss, 1974). Although family obligations can be a constraining influence on protest politics (Almond and Verba, 1963), former activists can exercise the choice of remaining single or deciding not to have children, thereby not subjecting themselves to the constraining influence of family obligations. As a result, they will be more politically active in political protest as adults.

The higher socioeconomic status (SES) activists are expected to be more radical than lower-SES activists independent of the separate effects of graduate education and occupational prestige associated with jobs in the knowledge and human service industries. Because of their structural location in various institutional sectors, higher-SES activists are more strategically located to witness the "business as usual" orientation within institutions that fosters institutional racism, the dominance of elitist interest groups in policy considerations, and the entrenched unwillingness to change institutional objectives. Their achieved status does not moderate their politics, but it does contribute to their political alienation. For example, young higher-SES blacks are more militant and alienated than those with lower SES (Matthews and Prothro, 1966; Michigan Survey Research Center, 1973). For white activists, the effects are similar. Those with higher SES are in a better position to see the malfunctioning of decision making within established institutions.

The last variable expected to influence both political attitudes and behavior is oc-

cupational values. Rosenberg (1957) found that college students develop distinctive occupational values. He discovered three major value clusters associated with career expectations—self-expressive values, people-oriented values and extrinsic reward orientations, i.e., high value placed on money, prestige and security. McFalls and Gallagher (1975) found a strong association between occupational values and student left-wing politics, particularly the low value leftists placed on extrinsic rewards. This relationship was expected to continue into adulthood.⁵ Those activists furthest to the left as adults were expected to place a high value on helping people and self-expression and low value on extrinsic rewards.

The final variable expected to influence protest behavior directly is radical political attitudes. Studies ranging from *The American Voter* (Campbell et al., 1960) to Goldberg (1971) indicate that political attitudes are important determinants of behavior. Although the strength of the relationship between attitudes and behavior may be partly spurious—i.e., due to the indirect effects of previous variables—political attitudes are expected to have a separate direct effect on political behavior. Two variables not included in equations (10) and (11)—parental SES and academic ability—are considered to have only indirect effects on the endogenous variables through intervening causal linkages.

The remaining equations in the model concern the intervening variables. Because of past and current discrimination (Blauener, 1972), race is expected to be a major determinant of parental SES. Race and parental SES are expected to have direct effects on both academic major and academic ability. Whites and those from higher-SES families are expected to be more likely to major in the social sciences and liberal arts and to have higher grade point averages. Because of ideological com-

mitments, the importance of financial support, the necessity of advanced degrees for occupations in the knowledge and human service industries and demonstrated academic excellence, all four causal antecedents are expected to influence graduate education (Fichter, 1964).

Only three variables are considered to have direct causal effects on career choice. The decision to choose a career in the knowledge and human service industries is influenced by humanitarian or leftist political commitments, majoring in the social sciences and having attended graduate or professional schools. The choice of remaining single or marrying but not having children is influenced by the degree of ideological commitment to radically restructuring the social order. Alternatives to the nuclear family also are expected to be higher among those who majored in the social sciences or attended graduate school.

Four variables have direct effects on the activists' SES. Those with the strongest ideological commitment are not expected to pursue monetary success and prestige and, therefore, have lower SES. The socioeconomic status of the respondents is transmitted across generations with parental SES affecting the current status of the respondent (Blau and Duncan, 1967). Graduate education is one of the primary criteria for SES. Those who have chosen careers in the knowledge and human service industries are expected to have lower SES. Even though they are likely to hold advanced degrees, their occupations are not any more prestigious than many of those in private sector occupations and, more importantly, there are restrictive wage ceilings in the public sector occupations they have chosen.

Finally, four causal antecedents have direct effects on occupational values. White activists are expected to value extrinsic rewards the least and to emphasize humanitarian and self-expression values. Both the initial commitment and the intellectual exposure to the social sciences is related to the same clustering of occupational values. Socialization emphasizing professionalism in graduate education is expected to have an independent effect on occupational values.

⁵ This variable was the most difficult to order in the causal model. Because of the limitations of the after-only research design and the expected effects of post-college experiences on career values, it was considered an endogenous variable immediately prior to adult politics.

OPERATIONALIZATION OF THE MODEL

Protest Behavior (X_1)—Operationally, this variable measures the extent of participation in political acts demanding reform or social change. Originally a five-item scale was developed to measure political efficacy. Three of the five items were selected to measure protest behavior: (1) organizational work in political programs and campaigns;⁶ (2) participation in demonstrations; (3) participation in any form of illegal political activity. The responses ranged from "frequently" to "never." Exploratory factor analysis was used to analyze the five items for each sample. The three items above emerged as the principal component. The scores ranged from 3 to 12. The higher the score the greater the participation.

Radical Political Attitudes (X_2)—Conceptually, this refers to the left-wing political sentiments of the former activists. A 14-item radicalism-conservatism Likert scale developed by Nettler and Huffman (1957) measured radical attitudes. In general, the scale measured attitudes concerning socialist versus capitalist economic and political programs.⁷ The scale was found to be both reliable and valid by its authors. The responses ranged from "strongly agree" to "strongly disagree." The scores ranged from 14 to 70. The higher the score, the more radical the political attitudes.

Occupational Values (X_3)—Rosenberg (1957) developed a 10-item occupational value scale measuring three clusters of values: self-expression values, people-ori-

ented values and extrinsic reward values. Exploratory factor analysis was used to verify the three separate value clusters for each sample. Only two factors emerged, one consisting of the three extrinsic reward measures—money, prestige and security—and a second general factor that included the remaining items. Only the unambiguous extrinsic reward value cluster was used to measure occupational values. Each item had three responses: high, medium and low. The scores ranged from 3 to 9. The higher the score the greater the emphasis placed on extrinsic rewards.

Socioeconomic Status (X_4)—A composite measure of education, occupation and income scores was used to measure SES. The selected procedure was developed by Nam (1963) for the U.S. Census Bureau. The range of scores was from 0 to 100 with higher scores indicating higher SES.

Family Responsibilities (X_5)—Operationally, respondents were asked to indicate the extent of family obligations, varying from being single to being married and having children. The range of scores was from 1 to 3 with higher scores indicating more obligations.

Career Choice (X_6)—This refers to occupational pursuits after college. Occupations were classified along a continuum that represented those work organizations that emphasized the profit motive at one extreme and those work settings that were knowledge and human service oriented at the other extreme. Operationally, there were five categories: (1) proprietors, managers, officials and salesmen in the private sector of the economy; (2) private practice professionals, e.g., doctors, lawyers, dentists, etc.; (3) government workers; (4) academic professionals; (5) those in social service and creative occupations, i.e., writers, journalists, social workers, full-time activists, etc.⁸

⁶ This particular item, at face value, does not seem to be a good example of leftist behavior; however, inspection of the questionnaires revealed that former activists were supporting either the liberal left-wing of the Democratic Party or local leftist reform candidates and programs, e.g., New Democratic Party or community action leagues.

⁷ A few examples of items in the scale are: (1) In a socialist system the worker maintains his dignity and self-respect, while under capitalism he is just a tool or instrument to be exploited. (2) No one should be allowed to earn more than \$25,000 a year "take-home" income. (3) Profits of the great industries should be rigidly controlled by the federal government. (4) It is up to the government to make sure that everyone has a secure job and a good standard of living.

⁸ Career choice, as well as some other measures, must be considered ordinal rather than interval or ratio measures. Although interval measures are assumed in regression analysis, Reynolds (1974) has found that ordinal measures can be used successfully in causal analysis if there are an adequate number of categories,

Graduate Education (X_7)—This variable measures the amount of education beyond an undergraduate degree. The responses ranged from "not attending graduate school" to "receiving two advanced degrees." The range of scores was from 0 to 8. The higher the scores the more extensive was the graduate education.

Academic Ability (X_8)—This variable was measured by the self-reported undergraduate grade point average.

Academic Major (X_9)—Operationally this variable measures major in college. All majors were collapsed into three categories: (1) non-arts and science major; (2) arts and science major except for social science; (3) social science majors.

Parental Socioeconomic Status (X_{10})—This variable was measured the same as respondent SES.

Race (X_{11})—Operationally, this is a dichotomous variable that distinguishes between the black and white respondents. A score of 0 identified black respondents and 1 identified white respondents.

RESEARCH FINDINGS

A series of criteria are used to test the adequacy of the theoretical model and to develop a more parsimonious restrictive model from the data.⁹ The model is tested for its general goodness of fit using maximum likelihood estimation procedures outlined by Land (1973). The correlation matrix also is reconstructed from the path

the responses are not highly skewed and control variables are measured with as much refinement as the independent and dependent variables.

⁹ One of the reviewers of the manuscript encouraged the author to weaken the assumptions in the model and test alternatives that would allow reciprocal causation, particularly between extrinsic rewards, radical attitudes and protest behavior. Utilizing two-stage least squares, different sets of static simultaneous equation models were examined. In every case, the alternative causal paths did not meet the criterion of explaining at least one percent of the variance in endogenous variables. The predicted values of the second stage also produced extreme values of path coefficients. This later result was due to multicollinearity. The predicted values for the second stage were functions of the variable race and highly intercorrelated with each other.

model to discover whether there are significant deviations from the original correlations. Since the research focused primarily on attempting to explain variance in endogenous variables, each path coefficient was examined to determine if it explained, at a minimum, one percent of the variance in the endogenous variables.¹⁰ A restricted model was developed from a fully recursive model based on this last criterion and was tested also for its general goodness of fit and its ability to reproduce the original correlation matrix.

Table 1 reports the path coefficients and the coefficients of determination for both the theoretical and restricted models. The goodness-of-fit test revealed the theoretical model was an adequate model for the existing data. The chi-square value was 8.567. With 17 degrees of freedom, the relationship between observed and expected values was not significant indicating the model was consistent with the empirical relationships. The theoretical model was somewhat less successful in reproducing the original correlations. Small variations between actual and predicted correlations of less than .05 were attributed to measurement error and were not considered significant. The predicted correlation matrix reproduced 47 out of 55 correlations within this range. The discrepancies greater than .05 were:

$r_{2.10} = .089$	$r_{45} = .068$
$r_{35} = .076$	$r_{48} = .159$
$r_{38} = .051$	$r_{68} = .187$
$r_{3.10} = .089$	$r_{89} = .091$

The deviations between academic ability and respondent SES (r_{48}) and academic ability and career choice (r_{68}) are substantial.

Based on the criterion of explaining variance, the theoretical model was revised. Eleven paths were dropped from the model and one was added, creating a more parsimonious model.

¹⁰ This criterion is different from the criterion of statistical significance, e.g., the path coefficient being at least twice the standard error. Since the major objective of the research was to explain variation in adult politics, the criterion of variance explained was chosen.

Table 1. Path Coefficients and Coefficients of Determination for the Theoretical and Restricted Models

<i>Dependent Variable</i> <i>Independent Variable</i>	Theoretical Model	Restricted Model
<i>X₁₀ Parental SES</i>		
<i>X₁₁ Race</i>	.495	.495
Coefficient of Determination	(.245)	(.245)
<i>X₉ Major in College</i>		
<i>X₁₁ Race</i>	.417	.370
<i>X₁₀ Parental SES</i>	.093	—
Coefficient of Determination	(.143)	(.137)
<i>X₈ Academic Ability</i>		
<i>X₁₁ Race</i>	.099	.099
<i>X₁₀ Parental SES</i>	.192	.192
<i>X₉ Major in College</i>	—	—
Coefficient of Determination	(.065)	(.065)
<i>X₇ Graduate Education</i>		
<i>X₁₁ Race</i>	.300	.345
<i>X₁₀ Parental SES</i>	.189	.182
<i>X₉ Major in College</i>	.106	—
<i>X₈ Academic Ability</i>	.014	—
Coefficient of Determination	(.224)	(.214)
<i>X₆ Career Choice</i>		
<i>X₁₁ Race</i>	.257	.345
<i>X₁₀ Parental SES</i>	—	—
<i>X₉ Major in College</i>	-.069	—
<i>X₈ Academic Ability</i>	—	—
<i>X₇ Graduate Education</i>	.157	.150
Coefficient of Determination	(.113)	(.108)
<i>X₅ Family Obligations</i>		
<i>X₁₁ Race</i>	.339	.339
<i>X₁₀ Parental SES</i>	—	—
<i>X₉ Major in College</i>	-.122	-.122
<i>X₈ Academic Ability</i>	—	—
<i>X₇ Graduate Education</i>	.204	.204
<i>X₆ Career Choice</i>	—	—
Coefficient of Determination	(.093)	(.093)
<i>X₄ Respondent SES</i>		
<i>X₁₁ Race</i>	-.290	-.305
<i>X₁₀ Parental SES</i>	.150	.124
<i>X₉ Major in College</i>	—	—
<i>X₈ Academic Ability</i>	—	.138
<i>X₇ Graduate Education</i>	.489	.484
<i>X₆ Career Choice</i>	-.341	-.328
<i>X₅ Family Obligations</i>	—	—
Coefficient of Determination	(.303)	(.321)
<i>X₃ Extrinsic Rewards</i>		
<i>X₁₁ Race</i>	-.406	-.389
<i>X₁₀ Parental SES</i>	—	—
<i>X₉ Major in College</i>	.014	—
<i>X₈ Academic Ability</i>	—	—
<i>X₇ Graduate Education</i>	-.119	-.110
<i>X₆ Career Choice</i>	.052	—
<i>X₅ Family Obligations</i>	—	—
<i>X₄ Respondent SES</i>	—	—
Coefficient of Determination	(.203)	(.200)

Table 1. (continued)

Dependent Variable Independent Variable	Theoretical Model	Restricted Model
<i>X₃ Radical Attitudes</i>		
X ₁₁ Race	.407	.384
X ₁₀ Parental SES	—	—
X ₉ Major in College	.164	.158
X ₈ Academic Ability	—	—
X ₇ Graduate Education	-.067	—
X ₆ Career Choice	.180	.162
X ₅ Family Obligations	-.185	-.193
X ₄ Respondent SES	.260	.231
X ₃ Extrinsic Rewards	-.175	-.167
Coefficient of Determination	(.459)	(.456)
<i>X₁ Protest Behavior</i>		
X ₁₁ Race	.099	.070
X ₁₀ Parental SES	—	—
X ₉ Major in College	.128	.126
X ₈ Academic Ability	—	—
X ₇ Graduate Education	-.046	—
X ₆ Career Choice	-.022	—
X ₅ Family Obligations	-.115	-.110
X ₄ Respondent SES	.084	—
X ₃ Extrinsic Rewards	-.364	-.363
X ₂ Radical Attitudes	.006	—
Coefficient of Determination	(.230)	(.222)

monious model. The eleven paths dropped from the model were:

p9.11 = .093	p36 = .052
p79 = .106	p17 = -.046
p78 = .014	p16 = -.022
p69 = -.069	p14 = .084
p39 = .014	p12 = .006

The last four paths are related directly to the endogenous variable of protest behavior. The one path that was added was p48 = .138. Academic ability as reflected in self reports of college grade point averages has a direct effect on the respondents' socioeconomic status. The goodness of fit is slightly better for the restricted model. The chi-square is 7.135 with 27 degrees of freedom. Given the fewer number of predicted paths, it is more difficult to reconstruct the original correlations. Table 2 presents the observed correlations and the reconstructed correlation matrix. Forty-five of the predicted correlations were within .05 of the observed values. The deviations from the observed values, however, were smaller than the original theoretical model. The ten correlations that deviated by more than .05 were:

r15 = .057	r38 = .064
r2.10 = .097	r45 = .074
r35 = .082	r68 = .075
r3.10 = .059	r89 = .079
r3.11 = .060	r79 = .080

The restricted model is an improvement and has the added benefit of being more efficient or parsimonious because fewer path coefficients are needed to explain the same amount of variance in the endogenous variables. Since the primary research objective is to account for the variance in adult politics of the former black and white civil rights activists, the remaining discussion of the findings will focus on the direct paths to radical political attitudes and protest behavior in the restricted model.

Six paths are directly related to radical attitudes. Race (.384), major in college (.158), career choice (.162), family obligations (-.193), respondent SES (.231) and extrinsic rewards (.167) accounted for about 46 percent of the variance in socialist versus capitalist attitudes toward economic and political policies. Race by itself explains most of the variance—34 percent. The remaining five variables explain between one and three percent of the vari-

Table 2. Observed and Predicted Correlations for the Restricted Model (Observed above and Predicted below Diagonal)

X	X ₁₁	X ₁₀	X ₉	X ₈	X ₇	X ₆	X ₅	X ₄	X ₃	X ₂	X ₁
X ₁₁ Race	—										
X ₁₀ Parental SES	.495	—									
X ₉ Major in College	.370	.183	—								
X ₈ Academic Ability	.194	.241	.072	—							
X ₇ Graduate Education	.435	.353	.161	.111	—						
X ₆ Career Choice	.300	.169	.111	.062	.253	—					
X ₅ Family Obligations	-.205	-.074	.029	-.034	.076	-.037	—				
X ₄ Respondent SES	-.104	.121	-.039	.142	.327	-.268	.097	—			
X ₃ Extrinsic Rewards	-.377	-.202	-.140	-.076	-.253	-.127	.059	-.001	—		
X ₂ Radical Attitudes	.570	.323	.327	.148	.337	.262	-.261	.122	-.366	—	
X ₁ Protest Behavior	.276	.139	.199	.054	.134	.085	-.142	-.022	-.414	.242	—
\bar{X}	.28	52.48	2.21	2.07	3.67	3.23	2.21	91.46	6.43	42.77	5.60
S.D.	.45	22.37	.85	.85	1.55	1.42	.88	6.69	1.63	9.30	2.05

ance. Former white activists were the most radical as adults; however, those former activists who majored in the social sciences, chose careers in the knowledge and human service industries, were either single or married but without children, had higher SES and valued extrinsic rewards the least also held radical political attitudes.

The restricted model has four direct paths related to protest behavior. They are race (.070), major in college (.126), family obligations (— .110) and extrinsic rewards (— .363). Together, they explained 22 percent of the variance.¹¹ The two variables which have the largest direct effects are race and extrinsic rewards, which explain 9 and 11 percent of the variance, respectively. Those adults who are most likely to continue to engage in protest politics are whites, former social science majors, single or married activists without children and those who least value extrinsic rewards of money, prestige and security. These are strong findings given the various sources of measurement error, the necessity of using indirect indicators of ideological commitment, and the possible exclusion of significant variables contributing to the unexplained variance.

Besides the direct effects of the variables on adult politics, there are also the indirect effects. Space does not allow a presentation and discussion of the individual and total indirect effects which can be computed from the available data. In general, variables that are intercorrelated and have large direct effects also have large indirect effects, e.g., student activism, which has strong indirect effects on the dependent variables.

In a separate analysis (Fendrich, 1976), the same variables were used to explore differences among white activists and non-activists and among black activists and non-activists. The research objective was to try to explain the variance in adult politics

¹¹ The major reason why the model is not as successful in predicting protest politics is the low precision of the endogenous variable when compared to the attitude measure. More detailed questions were considered in the preparation of the questionnaire, but it was decided these questions would have a major effect in reducing the response rate.

among racially homogeneous yet politically heterogeneous groups. The variables explained substantially more variance among the politically heterogeneous whites. For whites, the coefficients of determination (R^2) were .600 for radical attitudes and .421 for protest behavior. The coefficients for blacks were .122 and .247, respectively. The path models were better at predicting adult politics because there was more variation to be explained in the dependent variables, i.e., whites who were not active in protest politics as students are much more moderate or conservative. Stated somewhat differently, the within-racial group variance in adult politics is greater than that between-racial group variance for activists. Nevertheless, the results reveal important differences among the activists.

The two variables that have the largest effects are the race of the respondent and the value placed on extrinsic rewards. The race variable has strong direct and indirect effects. Race is considered an indirect indicator of ideological commitment. This assumption is consistent with the Geschwenders' (1973) research on the same black students. They found black activists were motivated to participate in the civil rights protest by feelings of relative deprivation. Demerath et al. (1971) and Von Eschen et al. (1971) found that white students who participated in the early phase of the civil rights movement were motivated by a leftist political ideology and humanistic commitments. Thus, there is empirical support for using race as an indirect indicator of leftist ideological commitment. The assumption is supported also by the subsequent findings that former black activists place higher value on extrinsic rewards and are more likely than whites to seek careers in the private sector of the economy that offer the rewards of money, prestige and security.

The extent of the differences in adult politics is revealed in descriptive statistics. The former activists were asked to identify themselves politically on a continuum from right to left. For whites, 21 percent are political moderates, 25 percent are liberals and 54 percent are radicals. For blacks, 13 percent are political conservatives, 52 per-

cent are moderates, 25 percent are liberal and 10 percent are radicals. Moreover, 75 percent of the whites participated in political demonstrations as adults compared to 47 percent of the blacks; and 43 percent of the whites engage in politically illegal protest as adults compared to 19 percent of the blacks. Whites, as adults, are clearly further to the left ideologically and more willing to continue to engage in protest politics.

If race is a good indicator of the initial ideological commitment to join a social movement, then it would be incorrect to conclude that the political commitments of black civil rights activists have been radically altered. Put more bluntly, it would be incorrect to assume black civil rights activists were coopted as adults. Their commitments as adults are consistent with their earlier beliefs. They fought for the right to enter the mainstream of American society (Warren, 1966) and, because of the relative success of the movement for young college-educated blacks (Pettigrew, 1975), they are now able to reap the benefits of that struggle. They are still committed to the specific issue of civil rights. Like members of successful unions, they can be mobilized to support left-of-center candidates and issues when their own interests are at stake. In contrast, the majority of former white activists are self-professed radicals. Whites are further to the left ideologically and are actively trying to bring about basic structural changes through social movement organizations and as social change organizational intellectuals.

Extrinsic rewards independent of the effects of race also had a strong effect on adult politics. Those former activists who are free of strong commitments to extrinsic rewards can pursue their political objectives. On the other hand, activists who place a high value on money, prestige and security cannot afford to participate in leftist politics as adults because of the risk of negative sanctions from peers and employers. There is no good measure for the positive career values associated with adult leftist politics. Activists who have maintained their political commitments probably are motivated by career values that stress

change and modification of the social structure (McCarthy and Zald, 1973; Flacks, 1971), i.e., the adult activists want to work in organizations that stress moving the social institutions in a more democratic and socialist direction.

CONCLUSIONS

Three tentative conclusions can be drawn from the research findings. First, persons active in protest politics who were active when they were college students are further to the left politically and more active in protest politics as adults. This indicates that adults who sustain commitments to a political movement must develop such commitments in their youth. This finding is consistent with generational conflict theory (Fendrich, 1974). Second, for adults to sustain commitments to radical leftist movements, they must be motivated initially by strong ideological commitments that extend beyond interest-group politics. This finding is based on the black-white differences. If social movement participants are motivated primarily for personal and collective gain, e.g., relative deprivation, they can become more politically moderate as adults if the movement is successful in opening opportunities for upward mobility. Finally, for adults, a strong ideological commitment to social change in a leftist direction needs to be supported by a personal commitment not to pursue career objectives for the sake of extrinsic rewards of more money, prestige and security.

Thus, three conditions were discovered that help to explain adult involvement in left-wing political movements. Adults who are active developed a high level of political consciousness and participation in their youth. They were motivated by ideological rather than pragmatic values. They need to remain free of both objective and subjective constraints that can inhibit their adult politics. These findings and conclusions are based on a small fraction of the student activists in the civil rights movement. Before they can be considered valid generalizations to explain the persistence of left-wing movement participation, additional empirical research and theoretical development is necessary.

One final point concerns the use of the multivariate model. There is growing opposition to the use of multivariate analysis in sociology. Both the immediate past Presidents of the American Sociological Association, Coser (1975), and Lee (1975), have expressed doubts about the use of these techniques. While space is not available to do full justice to their position, they generally argue that multivariate models, while rigorous, are not very illuminating because such techniques direct attention away from important sociological issues. This criticism is not necessarily valid. Multivariate analysis can be a means to address politically and socially relevant questions. Indeed, radical economics which uses essentially the same techniques is gaining widely based scholarly respectability (Lekachman, 1976). It also should be mentioned that multivariate model building and analysis reveal the modest state of accumulated knowledge within the discipline more critically than previous, less rigorous procedures.

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A MODEST TEST OF AN IMMODEST THEORY: THE FUNCTIONAL THEORY OF STRATIFICATION *

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Two hypothesis relating responsibility, reward and performance were designed to test the Davis and Moore functional theory of stratification. Large companies in the private sector of the United States economy were selected as the source of empirical evidence to test the theory. The data base was thought to be favorable to positive findings. The responsibility variable was measured by company assets, reward was measured by total compensation of the chief executive officer, and performance was indexed by several measures of growth and profitability. Over 700 of the largest companies in the United States were grouped into sixteen relatively homogenous business activity types in order to control for (1) scarcities of qualified incumbents, (2) structural differences between industries and (3) market conditions. The results provide limited evidence of a relationship between magnitude of responsibility (functional importance) and executive compensation (reward). No support was found for a hypothesized relationship between company performance, however measured, and executive compensation. Taken as a whole, the results do not confirm the functional theory.

The so-called functional theory of stratification as formulated by Davis and/or Moore (Davis, 1942; 1948; 1953; Davis and Moore, 1945; Moore, 1953; 1963)

has drawn attention to important issues for the study of social stratification. The debate, which has stretched over a generation, has been characterized by a sense that the writers were dealing with matters worthy of argument, although the brief and abstract form in which the theory was presented originally made it susceptible to diverse interpretations (Wesolowski, 1962:

* We are grateful to Parker C. Fielder for guiding us to a relatively tractable data set. We thank the *ASR* reviewers for thoughtful and constructive comments.

28). Despite the liveliness (and sometimes the heat) of the controversy, there has been little progress towards specifying a test of the theory (see Huaco, 1966). It is tempting to say that the longevity of the theory is related to its inaccessibility to empirical verification.

The present paper is a modest attempt to test a falsifiable version of the theory. We do not propose to investigate generalizations that pretend to be good for all sectors of all societies for all time. If doubt is cast on the theory restated in an acceptably rigorous and verifiable form and tested in a limited context, it will be incumbent on the defendants of the theory to restate it in less vulnerable form or to show why the test is incompetent or irrelevant, i.e., a unique exception. On the other hand, if the findings support the theory, the theory is supported only for the situation under scrutiny, although its plausibility score should be improved and the requirements for a more powerful test might be made more specific.

The Davis and Moore Formulation

The Davis and Moore theory (Davis and Moore, 1945:243; Davis, 1953:394-5; Moore, 1963:15) is addressed to a deceptively simple question: Why do different social positions in a social system carry different degrees of prestige and reward? Their answer is also deceptively and anthropomorphically simple: "Social inequality is . . . an unconsciously evolved device by which societies insure that the most important positions are conscientiously filled by the most qualified persons" (Davis and Moore, 1945:243). Therefore, stratification is a universal necessity for system survival. The specification of *most* qualified as opposed to sufficiently qualified is a needless burden on the theory, but it is not taken seriously and need not detain the present discussion (Wesolowski, 1962:28-9).

It is important to note that the theory focuses on the social positions in a given social system and on the type of individuals who occupy those positions at any given time, not on specific persons. Since not all

social positions have equally pleasant tasks, or are equally important to societal survival, or require the same amount of talent or training for their competent performance, "it does make a great deal of difference who gets into which positions" (Davis and Moore, 1945:243). Thus, and again anthropomorphically, a society must "instill in the proper individuals the desire to fill certain positions, and, once in these positions, the desire to perform the duties attached to them" (1945:242). This is to be accomplished by a system of rewards as inducements. The rewards must be differentially distributed to social positions according to the importance of the positions *and* according to the scarcity of qualified personnel available for those positions in order to guarantee that "less essential positions do not compete successfully with more essential ones for competent personnel" (1945:243). Attempts to explore aspects of the theory empirically have found the measurement of functional importance problematic and the results have been equivocal (Abrahamson, 1973a; 1973b; 1974; Grandjean and Bean, 1975; Harris, 1967; Land, 1970; Leavy, 1974; Lopreato and Lewis, 1963; Stinchcombe, 1963; Stinchcombe and Harris, 1969; Vanfossen and Rhodes, 1974).

Toward a Test of the Davis and Moore Formulation

Davis and Moore imply a simple causal chain between the functional importance of social positions and the rewards, qualifications and contributions to system survival of incumbents in those positions. Since we cannot deal with relative uniqueness, the first clue Davis and Moore offer to operationalize functional importance, we accept their second clue and measure functional importance by "the degree to which other positions are dependent on the one in question" (Davis and Moore, 1945:244n). The scarcity of qualified incumbents influences both the relationship between functional importance and the distribution of rewards and the relationship between distribution of rewards and the relationship between distribution of re-

wards and the distribution of qualified personnel.

Although they intended their theory "to explain, in functional terms, the universal necessity which calls forth stratification in *any* social system" (Davis and Moore, 1945:242, our italics), they fixate on positions unique to societal survival in their discussion of what they mean by functional importance and performance. Non-survival represents ultimate failure, but how does one assess whether the role prescriptions of a unique position have been carried out in a way to ensure societal survival? The failed monarch or president or general might have performed uniquely well, but the society may have collapsed for reasons beyond the control of mortal persons. Or the performance might have been uniquely bad, but the society tottered on despite that fact.

It is difficult to see how to use the survival criterion to test the theory. At first glance, survival seems easy and obvious to verify—but survival for how long, in what form, and ascribed to what sequence of episodes and human actions? The histories of societies are interlacings of one-time events of inordinate complexity, and there can be no sufficient and necessary explanation for nonrecurrent phenomena. Since monarchs are dethroned, presidents untaped and generals cashiered for reasons both related and unrelated to their qualifications or performance, a collection of such unique positions and their occupants does not make a data set. If we shift emphasis from actual survival to potential for survival in order to expand the data set, how is relative potential survival to be measured: by gross national product, by morbidity and mortality rates (inverted), by literacy, by degree of industrialization, by cost of living (inverted), by standard of living, by energy resources (consumed or preserved), by a composite index of all or some of the above? The methodological difficulties that are inherent in the thesis when societal survival is a focus of interest may be surmountable, but we despair of making societal comparisons of rewards associated with functionally unique positions. Waiting for societies to expire may

hold out promise in the present world climate, but we prefer to seek less apocalyptic criteria for a test. We propose to substitute the testable notion of performance for the untestable notion of survival. This substitution would be acceptable to Moore:

The explanation presented here reiterates the thesis that "functional differentiation" of positions will inevitably entail unequal rewards—and adds the thesis that *differences in performance must be expected to be and will be differentially valued*. (Moore, 1963:15, our italics)

In discussing the determinants of positional rank, Davis and Moore come closest to a testable statement of the theory:

In general those positions convey the best reward, and hence have the highest rank, which (a) have the greatest importance for society and (b) require the greatest training or talent. (Davis and Moore 1945: 243)

It is clear that such a statement is intended not as a static description of the state of a system but as a dynamic statement of causal connections. A direct observed relationship between functional importance and reward would be explained in terms of the necessity for motivating qualified persons to fill and discharge important positions conscientiously. Even if the relevant variables could be specified and measured, cross-sectional analysis cannot directly test this dynamic causal principle.

Nevertheless, an indirect test of the causal principle is apparent. If, controlling for scarcity, the expected relationships (between the functional importance of positions and rewards, between rewards and qualifications, and between qualifications and performance) are not observed, at a given point in time, then the necessary preconditions for the functional explanation do not exist. The implications of such a result for the theory are obvious, and the test can be determined by cross-sectional analysis. However, since the causal mechanism, is not dealt with directly in a cross-sectional study, a positive result may add to the plausibility of the theory but does not rule out alternative explanations for why some positions are highly rewarded.

An exhaustive test of the theory would

require measures of differential functional importance, differential scarcity and distribution of qualified personnel, the differential distribution of rewards, and the differential performance of incumbents. We doubt that any single test can deal with all these variables simultaneously.

Criteria for Selecting Functional Areas and Units of Analysis

A straightforward empirical test of the theory requires the identification of one or more functional areas with reasonably clear criteria for and accessible indicators of responsibility, reward and performance. We do not know how to estimate relative functional importance of tasks performed in different spheres of human endeavor. The nature of the dependencies are different. Therefore, comparisons should be made within rather than between kinds of functions. Another reason for this restriction is that the reward side of the equation may be confounded if cases are drawn from diverse fields. For example, the rewards for the clergyman, artist, teacher and politician very well may be different in kind, and the comparisons necessary to test the theory could not be made even in the unlikely event that there was agreement about the relative functional importance of these several fields of endeavor.

If the problem of societal survival is not a profitable case and if unique positions are not available for analysis, what kind of data can be used to test the theory? The positions to be examined should carry high and measurable responsibility (i.e., they should be functionally important for something), there should be some way to assess performance and they should have variable rewards. Such positions should occur in the very kind of society that the adversaries of the functional theory grumble about, i.e., they should be positions gained in competition, and the positions should be rewarded for activities involving struggle for advantage. Senior management positions in capitalistic industrial organizations come as close to satisfying these criteria as any. It is hard to imagine a category of positions more likely to yield

support for the theory (Davis, 1948:369-70; Wesolowski, 1962:31).

We have elected to focus on a sample of large, publicly held companies within the private sector of the U.S. economic system. United States law requires such companies to make available to regulatory agencies, to stockholders and to other interested or curious persons, information that can be constructed into indicators of the magnitude of responsibility represented in the company, of the company's degree of success and of the kind and amount of rewards afforded senior executives. The data are presented in annual reports to stockholders and in requests for proxies at the time shareholder meetings are announced. We have not yet fully explored the possibility of securing similarly detailed information for companies in other countries, but we know that many countries do not require similar disclosure. It goes without saying that companies anywhere are disinclined to disclose publicly anything more than they must. Companies closely held by a few owners are not available for analysis because they are not required to expose themselves in this way and do not.

Research Design

Units of analysis. There are three possible kinds of scarcity: of personnel, of rewards and of positions, and we suggest that of the three only highly valued positions are truly scarce in mature industrial societies (Huaco, 1966:238). If there were a scarcity of executive talent, that scarcity would probably be constant within business activity areas in the sense that the pool of potential incumbents is the same for all companies in a particular business activity (e.g., paper making) and possibly in related activity types (e.g., production of building timber). However, the chairs of boards and the presidencies of companies rarely go unfilled for very long. We assume there is some kind of competitive market for top executives and that there are fewer top level opportunities than competent potential incumbents, but we are in no position to assess the true market for senior managerial types nor the depth of the pool

of eligibles. The logic of our approach to a test of the functional theory dictates that we look at companies that are homogeneous with respect to business activity in order to control for differentials in the scarcity of rewards and of qualified personnel. Hence, all comparisons are within, and not between business activity types. We also require variation within types with respect to size, since magnitude of responsibility is a principal indicator of relative functional importance.

The initial sample was taken from directories of large U.S. companies published annually in *Forbes* magazine.¹ The listed companies are reported with a number of indices that can be used to measure performance, responsibility and executive compensation. Companies are included in the *Forbes* directories if they rank among the top 500 U.S. companies on at least one of the following: assets, sales, total market value of common stock or net profits. The 859 companies included in the 1975 *Forbes* lists (containing 1974 data) constitute the main data source for the present study. Thirty-seven companies had too much missing data to be included in our sample. The number of companies available for analysis varies because data are incomplete for some cases. Homogeneity within business activity types was determined by classifying companies according to their principal activities as listed in the *Forbes* (1975) industry groups and as described in *Standard and Poor's Stock Guide* (1975).

Responsibility measures. The major responsibility for a company and its success or failure is assumed to rest with the top

management of a firm. Clearly, they are responsible to a variety of clients. They are obliged to protect the interests of creditors and shareholders, to maximize profit and growth. They also have obligations to the company's employees: to reward them for work competently performed, to provide them with a safe working environment and, in this day and age, to make working conditions decent and comfortable and to ensure a degree of job security, if not job satisfaction. In the present period of industrial organization, top management is not likely to escape these responsibilities to employees if it hopes to retain workers and maintain their productivity. The difficult task is to balance profitability against employee interests—to give what management must in hope of maximizing productivity and minimizing worker turnover, but to give no more than it must, lest the company become noncompetitive or unprofitable. This balancing act is reflected in the quality and price of the product or service provided for the third constituency, the consumer. Top management is obliged to see that the consumer gets, or thinks she or he gets, value for money. But the consumer must not get too much value for money if the company is to be competitive, if loans are to be secure, stockholders satisfied and employees safe and secure in their jobs and decently paid. The obvious functional requisite, probably the only obvious one, perhaps the only one, is that top management must enable the company to survive. *Pace, Davis and Moore!*

Two measures of the responsibility of top management are available from the *Forbes* directories: assets and number of employees. Stockholders' equity is another responsibility measure. Although it is not reported by *Forbes*, *Fortune* magazine publishes annual directories of selected companies which include stockholders' equity, allowing us to supplement the *Forbes* data for 607 companies (see Table 1).

These three measures are highly interrelated, particularly within activity types. In order to reduce ambiguity about the responsibility measure, only those types in which the intercorrelations among all three

¹ Although the *Forbes* directories have been published in various forms for nearly three decades, compensation data for chief executive officers are available only for the years since 1970. The time-series is further complicated, if not restricted, by a large amount of missing data for 1970 and 1971, possibly because of teething problems in developing procedures for compiling the compensation data. Once the series is sufficiently long to make a time-series analysis tempting, researchers will have to contend with the fact that companies in the lower ranks on the *Forbes* selection criteria tend to be unstable participants in the listings.

Table 1. Four Responsibility Measures by Business Activity Type

Business Activity Type	1974 Assets (\$'000,000)	1974 Stockholders' Equity (\$'000,000)	1974 Number of Employees ('00)	1970-74 Average Annual Percent Change in Assets
Types Included in Analysis	\bar{X} (N)	\bar{X} (N)	\bar{X} (N)	\bar{X} (N)
Banks	4372 (159)	378 (60)	4 (158)	9.0 (85)
Chain Stores (dept. & discount)	1881 (19)	844 (16)	87 (19)	4.0 (12)
Chemicals	1146 (27)	585 (27)	21 (27)	5.6 (19)
Conglomerates	2025 (57)	819 (53)	59 (57)	4.1 (55)
Drug Mfrs.	954 (13)	588 (13)	27 (13)	8.2 (13)
Food & Drink	637 (60)	350 (49)	20 (60)	7.7 (42)
Forest Products	983 (24)	491 (24)	21 (24)	1.4 (17)
High Technology	3239 (43)	1668 (35)	61 (43)	8.2 (30)
Insurance	2297 (40)	461 (19)	8 (40)	6.6 (31)
Iron & Steel Producers	1610 (17)	864 (17)	38 (17)	0.4 (14)
Manufacturing, n.e.c.	1296 (85)	641 (80)	44 (85)	4.4 (56)
Misc. Mining & Metals Producers	1128 (22)	613 (20)	15 (22)	6.3 (16)
Personal Products	780 (16)	448 (15)	23 (16)	14.4 (16)
Petroleum Producers	4197 (36)	2332 (33)	22 (36)	7.2 (22)
Supermarket Chains	401 (23)	166 (23)	31 (23)	5.5 (17)
Utilities	1974 (76)	962 (46)	7 (76)	6.1 (68)
All Companies	2326 (717)	772 (530)	26 (716)	6.4 (513)
Types Excluded from Analysis *				
Clothing	671 (14)	278 (14)	38 (14)	0.1 (12)
Construction	485 (12)	190 (1)	17 (12)	11.9 (5)
Diverse Financial Services	4172 (17)	486 (10)	13 (17)	8.6 (12)
Leisure	514 (21)	252 (18)	17 (21)	7.6 (13)
Misc. Distributors	282 (8)	212 (4)	19 (8)	37.6 (5)
Tobacco	1721 (6)	769 (6)	26 (6)	3.4 (6)
Transportation	1634 (27)	665 (24)	29 (27)	-0.7 (18)
All Companies	1463 (105)	453 (77)	23 (105)	6.5 (71)
Total	2212 (822)	731 (607)	26 (821)	6.4 (584)

* Business activity types were excluded from further analysis if the intercorrelations between 1974 assets, equity, and number of employees within a type were not all above .80.

responsibility measures exceeded .80 were used in the subsequent analysis. Eight business activity types containing 105 companies (12 percent of the initial sample) were excluded from analysis because they

did not meet this criterion. The average within-type product-moment intercorrelations are presented in Table 2. Since Assets has the least amount of missing data and the highest average correlation with the

Table 2. Average Within-Type Intercorrelations among Four Indicators of Company Responsibility *

	(1)	(2)	(3)	(4)
1974 Company Assets	(1)	1.00	.96	.93
1974 Stockholders' Equity	(2)	.04	1.00	.92
		(530)		
1974 Number of Employees	(3)	.04	1.00	-.03
		(716)	(530)	
1970-74 Average Annual Percent Change in Assets	(4)	.22	.23	1.00
		(513)	(410)	(512)

* Values above the diagonal are averages of product-moment correlations within the 16 business activity types weighted by the number of companies. Standard deviations for the weighted correlations and total number of cases (in parentheses) for which complete data are available are given below the main diagonal.

other two measures, it was selected to index responsibility, our operational measure of functional importance, i.e., the dependence criterion. Nevertheless, our procedure assures comparability on all three dimensions of responsibility.

Forbes data are relatively complete for 591 companies from 1970 through 1974. The data first were restated in constant 1970 dollars to control for inflation. Next, 1974 assets were correlated separately with each year 1970–1973 and with the five-year averages. All correlations were .98 or higher. Since assets are stable over the short run and since many of the smaller companies moved in and out of the *Forbes* lists over the years, use of the 1974 assets figures allows more companies to be retained for analysis than would a composite index and with no loss in rigor.

If, as we propose, assets are used to index the magnitude of responsibility, change in company assets implies a corresponding change in the responsibility carried by executives. Such changes in the magnitude of responsibility within a company are indicated by the average annual percent change in assets (in constant dollars) over the period, 1970–1974. It is clear from Table 2 (column 4) that this measure is independent of the three cross-sectional measures of responsibility.

One could also argue that changes in the assets of a company can be attributed to the success or failure of executive performance. On this line of reasoning, the measure should be interpreted as a performance rather than a responsibility indicator. No matter which interpretation is accepted, a positive correlation between this indicator and rewards is predicted by the functional theory.

Although Davis and Moore (1945) speak of prestige as a reward, when attention is focused on positions of very high responsibility, prestige defies measurement. How would one differentially rank the chief executives of IBM, Xerox and Kodak? One could consider measuring reward by semi-imponderables such as the perquisites that come with a key to the executive suite, but this would require more information about the life styles of

individual executives and the inner workings of individual organizations than may be knowable. In order to devise a test that will not bog down in debate over different rating schemes, we opt for tangible economic rewards which are confidently measurable. We accept that there is a great diversity of intangible rewards attached to any high status position, but none substitutes for money.

Three measures of remuneration are available from the *Forbes* directories:

- (1) total compensation of the chief executive officer (in \$000), including salary, bonus, deferred compensation and director's fees, but excluding stock options;
- (2) salary and bonuses (in \$000) of the chief executive officer;
- (3) average remuneration (in \$000) of all officers and directors.

Superannuation benefits and gains from exercising stock options are deferred rather than direct and immediate measures of reward. Although such information appears in annual calls for stockholder meetings as required by U.S. law, the information is not reported in *Forbes'* extensive record of U.S. companies. The facts can be secured on a case-by-case basis from company proxies and reports, and we have experimented with collating and interpreting such data from a subset of companies. For the purpose of this paper, however, we decided that the yield in improved evidence did not justify a considerable investment in data acquisition and collation time.

We do not present the detailed base data on reward as we did for the responsibility measures. However, Table 3 shows that the three reward measures are highly intercorrelated, and this fact allows us to simplify and clarify comparisons by restricting attention to the one position of ultimate responsibility, the chief executive officer.

Although total compensation (the reward measure) is not as robust a statistic as assets (the responsibility index), the 1974 reward figures intercorrelate well enough with each year separately and with the five-year average to permit use of the

Table 3. Average Within-Type Intercorrelations among Four Indicators of Executive Compensation*

		(1)	(2)	(3)	(4)
1974 Total Compensation of Chief Executive Officer	(1)	1.00	.92	.76	.33
1974 Salary and Bonuses of Chief Executive Officer	(2)	.06 (714)	1.00	.72	.31
1974 Average Remuneration of All Officers and Directors	(3)	.07 (716)	.07 (714)	1.00	.43
1970-74 Average Annual Percent Changes in Total Compensation of Chief Executive Officer	(4)	.17 (513)	.17 (510)	.11 (512)	1.00

* Values above the diagonal are averages of product-moment correlations within the 16 business activity types weighted by the number of companies. Standard deviations for the weighted correlations and total number of cases (in parentheses) for which complete data are available are given below the main diagonal.

1974 total compensation measure. This enables us to preserve the maximum number of companies in the sample. Change in compensation, indexed by the five-year average annual percent change in total compensation in constant dollars, does not correlate highly with the other reward measures. Hence, change in compensation is retained in the analysis.

Performance measures. Although our restatement of the theory does not require us to deal with societal survival, our data enable us to offer a clarifying comment on the issue. We arbitrarily identify four kinds of enterprises of relatively high societal value and four of less, or possibly negative, value. The assets of the two largest companies in each business and the executive compensation for those companies are reported in Table 4. Obviously, the findings are the reverse of the societal value postulate of the functional theory. Even though the "more essential" companies tend to be larger (twice as large on average) than "less essential" companies, their chief executives get less pay (\$58,000 less on average).

One brief word about a type of corporate service more or less essential to societal survival may be worth a thousand pictures.

Wells, Rich boasts blue chip clients and outstanding creativity. (It dreamed up the famous Alka Seltzer TV commercial—"I can't believe I ate the whole thing.") When the company went public in 1968, it had little trouble in attracting investors. The first issue of common stock sold out at

17½, and a second, in 1971, brought 21¾. This infusion of capital meant \$12.5 million for insiders (that is, people in or close to management) and \$696,000 for the corporation.

Founder and head of Wells, Rich is Mary Wells Lawrence, one of the brightest stars ever to sparkle in the advertising world. From the two public stock offerings, she received over \$3.5 million. In addition, she boasts a contract (signed in 1969 and modified in 1971), guaranteeing her a yearly salary of \$223,000 to 1981, deferred annual compensation of \$30,000 and an annual consulting fee of \$125,000 for a dozen years after her retirement. She also received "executive compensation awards" of \$185,505 in 1973 and \$168,248 in 1974. After the agency obtained what it wanted from public status—increased executive compensation, via stock options and higher salaries, and acquisition of another ad firm via an exchange of stock—Wells, Rich and Wall Street cooled toward each other. The shares faded to a skimpy 5½ in 1974. (Scheibla, 1975:9)

No further comment is needed. The reward to the creator of Alka Seltzer advertisements and the evidence in Table 4 speak clearly enough about societal survival and the immodest formulation of the functional theory.

Aside from bankruptcy as a negative indicator of company survival, *Forbes* publishes a variety of indices covering diverse dimensions of company performance. The incestuous relationship among many is obvious, and most are moderately intercorrelated in any given year and within the short time-series available. We make no

Table 4. Compensation of Chief Executive Officers and Assets of Selected Companies by Presumed Societal Value

	Compensation ^a (\$'000)	Assets ^a (\$'000,000)
Less Essential		
Beer		
Anheuser-Busch	305	654
Jos. Schlitz	238	384
Cosmetics		
Avon	342	537
Revlon	264	414
Soft Drinks		
Coca Cola	239	1124
Pepsi Cola	274	899
Tobacco		
American Brands	374	1888
R. J. Reynolds	278	2130
Average	289	1004
More Essential		
Ethical Drugs		
Eli Lilly	271	822
Merck	254	777
Food		
Borden	261	1237
General Foods	232	1439
Steel		
Bethlehem Steel	239	3389
U.S. Steel	283	6115
Textiles		
Burlington	210	1346
United Merchants & Manufacturers	101	800
Average	231	1991

^a Five-year averages (1970-74) in constant 1970 dollars as adjusted by Annual Consumer Price Index.

judgment as to which are more important to company survival or which combination of variables indicates whether one company is performing better than another. Six indicators are generally recognized to measure two important aspects of company performance. The first five indicators are described in *Forbes* (January 1, 1975: 38). We constructed the last indicator to minimize the missing data problem.

1. *Economic growth* is measured by:

(a) the 1970-1974 average annual percent change in sales, where the average sales for the years 1970-1974 are expressed as a percentage change from the period 1965-1970 in terms of a five-year annual compounded rate of growth;

(b) the 1970-1974 average annual percent change in earnings per share, where the average earnings per share for the

years 1970-1974 are expressed as a percentage change from the period 1965-1970 in terms of a five-year annual compounded rate of growth.

The weighted average of the intercorrelations for the sixteen activity types is only .43 (total companies=494, standard deviation=.24). Hence, both measures of economic growth are included in the analysis.

2. *Profitability* is measured by:

(a) the 1970-1974 average percent return on stockholders' equity, where the return is based on earnings per common share as a percent of the stockholders' equity per common share at the beginning of a given year;

(b) the 1970-1974 average percent return on total capital, where the return is based on net income after taxes and write-offs, but before the deduction of interest

on debts as a percentage of stockholders' equity plus the long term capital from other sources;

(c) the 1974 net profit margin which is the proportion of net income available for either stockholders' dividends or reinvestment in the company;

(d) the 1974 ratio of net profits to assets which, similar to the five-year average return on total capital, indicates the return on investment from all sources.

The 1970-1974 average return on total capital is sufficiently correlated with the 1970-1974 average return on stockholders' equity to be deleted from the analysis (Table 5). The intercorrelations among the other three measures of profitability are sufficiently modest to require their retention in the analysis.

In this paper, we do not deal with price performance on the stock market. Folklore suggests that stock price fluctuations are susceptible to factors more remote from the immediate control of management than are other indicators of company performance. Managers can strive to effect growth, cut costs, increase production and sales, and otherwise attempt to maximize profits and their actions may influence the market price of the stock. They may also try to manipulate but they cannot control the behavior of investors who ultimately determine the market price of shares. For the time being, we bow to folklore and have eliminated market price changes as a measure of company performance.

A Modest Test of an Immodest Theory

We have already mentioned that we doubt a shortage of potential incumbents exists for positions in mature, industrialized societies. There may be periods of lag in supply while educational institutions catch up with unanticipated demands or some efforts may be made to restrict supply (Tumin, 1953:389), but a shortage of positions seems to be more nearly the rule. In any case, the distributions of qualified personnel and positions within a given business activity type can be taken as constant over the short run of five years covered by our data set by restricting comparisons to within-type data. Holding constant the distribution of qualified personnel, only the key concepts of (a) functional importance, (b) reward and (c) performance need be operationalized and only two basic hypotheses need be subject to test.

Hypothesis 1. Total compensation of chief executive officers is positively related to company assets.

Compensation is positively related to company assets, and the *precondition* for a functional explanation is upheld (see column 2 of Table 6). In the most favorable case, petroleum products, 64 percent of variability (r^2) in total compensation is accounted for by assets. On average, however, only 29 percent of variability is accounted for by assets. Clearly, factors other

Table 5. Average Within-Type Intercorrelations among Four Indicators of Company Profitability *

		(1)	(2)	(3)	(4)
1970 Average Annual Percent Return on Stockholders' Equity	(1)	1.00	.87	.35	.58
1970-1974 Average Annual Percent Return on Total Capital	(2)	.08 (497)	1.00	.41	.65
1974 Profit Margin	(3)	.22 (494)	.24 (497)	1.00	.57
1974 Net Profit Return on Assets	(4)	.16 (498)	.16 (505)	.14 (502)	1.00

* Values above the diagonal are averages of product-moment correlations within the 16 business activity types weighted by the number of companies. Standard deviations for the weighted correlations and total number of cases (in parentheses) for which complete data are available are given below the main diagonal.

than size of company must influence the compensation of chief executive officers. In addition, changes in compensation and the responsibility index (column 3 of Table 6) are moderately related in only three business activity types: supermarket chains, forest products and the miscellaneous metals and mining producers. We conclude that there is moderate support for Hypothesis 1.

Hypothesis 2. Company performance is positively related to the total compensation of chief executive officers.

No matter what measure of company performance is selected, there is no support for Hypothesis 2. Detailed findings are not presented, but Table 7 summarizes the distribution of correlations between executive compensation and performance. A more complex statistical model than the linear assumption underlying this analysis might improve the fit between compensation and performance. However, it is unlikely that any strategy would reverse a

sufficient number of the negative correlation coefficients (54 percent) to alter the conclusion that this test of Hypothesis 2 fails to support the functional theory.

Conclusion

If we accept that organizations providing a product or service which commands a price have positions that are the loci of responsibility, then if the company is to perform its functions (that is, to do its work and survive), the responsibility inherent in such positions must be competently discharged. Rewards should be related to such competent performance so that the company can retain the service of the competent performer. It can also be argued that the managers of a large company have more responsibility and more people depending upon them than those who manage a small company in the same activity. A larger company has more employees, more stockholder equity, that is, more money at stake and probably more stockholders, and more assets to manage.

Table 6. Product-Moment Correlations between Responsibility and Reward Indicators by Business Activity Type

Business Activity Type	Total Compensation of Chief Executive by Assets (1974) ^a		Change in Executive Compensation by Change in Assets (1970-74) ^b	
	N	r	r	Percent of Cases Complete
Banks	159	.69	-.07	53
Chain Stores (Dept. & Discount)	19	.45	-.37	63
Chemicals	27	.68	.35	70
Conglomerates	57	.66	.39	96
Drug Manufacturers	13	.52	.10	100
Food and Drink	60	.71	.20	70
Forest Products	24	.65	.63	71
High Technology	43	.33	.23	70
Insurance	40	.35	.19	78
Iron & Steel Producers	17	.26	.14	82
Manufacturing, n.e.c.	85	.27	-.24	66
Misc. Mining & Metals Producers	22	.73	.51	73
Personal Products	16	.74	.26	100
Petroleum Products	36	.80	.05	61
Supermarket Chains	23	.36	.74	74
Utilities	76	.35	.11	89
All Types	717	.54 ^c	.14 ^c	72 ^c

^a Data complete for all cases.

^b Average annual percent change in total compensation of chief executive by average annual percent change in assets.

^c Weighted average.

Table 7. Weighted Average Product-Moment Correlations between Executive Compensation and Performance Indicators

Performance Indicator	Total Companies	Total Compensation of Chief Executive Officer (1974)		Change in Executive Compensation (1970-74) *	
		r	Standard Deviation	r	Standard Deviation
	(16 Types)				
1970-74 Average Annual Percent Change in Sales	511	-.11	.23	-.04	.23
1970-74 Average Annual Percent Change in Earnings Per Share	494	-.03	.29	.01	.29
1970-74 Average Annual Percent Return on Stockholders' Equity	498	-.01	.25	.01	.27
1974 Net Profit Margin	503	.01	.23	.06	.24
1974 Net Profit Return on Assets	717	-.05	.24	-.13	.23

* Average annual percent change in total compensation of chief executive officer.

Managerial actions in such a company affect more people, involve more investment decisions, more capital, more consumers and more suppliers. If such a company prospers or crashes, the stakes are higher and the reverberations are greater than is the case with a smaller firm with a smaller share of the market. A testable statement of the theory suggests that such managers should be rewarded in a manner bearing some relationship to the magnitude of their tasks, and that they should perform accordingly.

We find that, on the average, executives of large companies receive more pay than executives of small ones, but there is no evidence that large companies perform better, no matter how performance is measured. Thus, there is little or no support for a functional explanation.

Alternatively, it may be argued that market forces strongly condition performance. However, our comparisons are made within business activity types over a constant time period, and external influences, including market forces, should be similar for all companies of a type. If market conditions make a difference, they are external to the control of incumbents in important positions and do not enter into the functional theory.

How are differential rewards to be explained? A plausible answer is that large

companies can provide the highest compensation with the least visible impact on their balance sheets—and do so irrespective of their performance. The mechanism for this reward pattern is simple indeed: chief executive officers influence, when they do not determine, their own compensation (cf. Simpson, 1956: esp. 133-4).

A few years ago, ITT's Harold Geneen speculated to *Forbes* that in terms of the wealth he was creating for stockholders, he might be worth \$5 million a year instead of just short of \$800,000. When we asked him this year why he hadn't raised his salary, he replied: "No one moved up there, and I didn't dare to do it alone. I still feel underpaid."

In a way, the whole system makes no sense. . . . George Scharffenberger at ailing City Investing had himself paid \$440,000, Meshulam Riklis at fouled-up Rapid-American, \$425,000. But the bosses at highly successful S. S. Kresge and Bank-America got only \$265,000 and \$249,000, respectively. (*Forbes*, May 15, 1975:234)

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COMMENTS

A CRITICAL CELEBRATION OF GUSFIELD'S "THE LITERARY RHETORIC OF SCIENCE" *

(COMMENT ON GUSFIELD, ASR
FEBRUARY, 1976)

Gusfield (1976) wants to challenge the accepted view that scientific language is a neutral transmitter of knowledge. To do this, he constructs a metaphorical equation between literary and scientific discourse which allows him to use techniques of literary criticism on specimens of scientific prose. As a result of his analysis of one paper central to policy formation with respect to drinking drivers, he suggests that there is a fundamental "tenuousness or ambiguity of conclusions" (Gusfield, 1976:32) in some scientific writing.

This is an excellent pioneering essay. However, if we are to use such an approach to understanding aspects of the production of scientific knowledge (and I believe we should), it is crucial that we have the issues it raises clear at the start. Four of these issues lack clarity in Gusfield's paper:

1. What is the meaning of "knowledge" in this approach?
 2. Through what process is this scientific knowledge developed?
 3. Is Burke's critical framework the only technique of rhetorical analysis or are there alternatives?
 4. What is the relationship between rhetoric and morality in the process of knowledge construction and policy formation?
1. What is the meaning of "knowledge" in this approach?

"Rhetoric" is more than literary style. Gusfield (1976) recognizes this in quoting Aristotle's definition of this concept which directs our attention to "persuasion" as the essential element. To deal with the question of knowledge in this case, therefore, is to grasp how

* George Zollschan suggested this comment. Bart Stoodley provided a most helpful critique of an earlier draft. I remain responsible for its genesis and errors.

knowledge is related to persuasive discourse through an understanding of rhetoric as addressed speech.

The conclusion of a logical argument, if one accepts its premises, is self-evidently true irrespective of any audience's response to it. The conclusion of a rhetorical argument, on the other hand, cannot be treated as knowledge as it stands: its cognitive status has to be assessed in relation to the response of the intended audience. To maintain that scientific discourse is rhetorical, or more warily to treat it as if rhetorical (Gusfield, 1976: 17), is to formulate *scientific knowledge as the changing judgments of scientific audiences on the meaning of statements directed to them, where these judgments are understood to emerge through a process of persuasive argumentation.*

Although immanent in Gusfield's essay, this formulation is never presented in the explicit terms that clarity demands. This omission, in combination with his focus on written presentations and the rather "forced" conclusion might encourage readers to think that rhetoric in scientific presentations is but one more obstacle to scientific lucidity and, ultimately, logic. While the position of logical arguments is moot from a rhetorical viewpoint, such a conclusion would be alien to the interests that Gusfield (1976) pursued in his paper. The treatment of scientific knowledge as an emergent from a process of persuasive argumentation leads us far from the conventions of the positivisms.

2. Through what process is this scientific knowledge developed?

As far as one can tell from Gusfield's (1976) essay, he believes that scientific knowledge is found in written "scientific presentation," i.e., in the written presentation of "the theory, generalization or conclusion to which the work of the scientist has led" (p. 18). This will hardly do. The written presentation is only one stage in the construction of scientific knowledge. If we are to explore seriously a rhetorical analysis of scientific knowledge, it is critical to broaden Gusfield's presentation.

From a general rhetorical framework that

conceives speaker, argument, situation and audience as the essential aspects of persuasion, one may construct four analytic stages that are important to such an enterprise. (a) There has to be a creation of persons who are licensed to speak before a scientific audience and who can do so effectively and convincingly. (b) Such individuals need pertinent experience on which to base their discourse; they need "research." (c) This experience is reconstructed in some written presentation that seeks to make a particular conclusion plausible. (d) Finally, an authoritative audience responds to the written presentation indicating what status it has as knowledge.

(a) Without going into the whole question of the recruitment, training and certifying of scientists (restricting ourselves to written presentations), the context of the presentation (in books or journals), the author's credentials, academic affiliation and display of research support may all be taken as a presumptive guarantee (based on the gatekeeping activity of graduate faculties, peer review committees and editorial referees) of the writer's intention to speak scientifically and, hence, intention to speak scientifically and, hence, initially demarcate such presentations from nonscientific speech. Writers are tentatively licensed to speak scientifically, therefore, by a display of a variety of credentials in a rhetorical situation wherein a scientific audience expects to encounter scientific discourse. A view such as this needs to be added to Gusfield's (1976:20) useful but incomplete formulation of credentials as identifying authors by establishing their role.

(b) The second stage in the production of scientific knowledge is the most difficult to place in a rhetorical understanding of that process. After all, the research experience is only available in the reconstructions of the written scientific presentation. But can what researchers do be inferred from what they say they do, as Gusfield seems to think? Probably not. However, given our undeveloped grasp of this transformation, it must suffice here to point out the central element in this stage from a rhetorical stance. Scientists are not omnivorous in their hunger after experience; they limit their intellectual appetites to those kinds of experience which they believe can be presented plausibly in written form to a scientific audience. Some sociologists, for example, only seek out measurable experiences since they believe it difficult to speak persuasively about the unquantified to their intended audience.

(c) Rather than repeat myself, I shall deal with this third stage in the production of scientific knowledge later when considering alternatives to Kenneth Burke in the study of scientific argumentation.

(d) From the addressed character of rhetorical speech and the idea of "persuasion," it is obvious that to understand the emergence of scientific knowledge one must examine the kinds of responses *and* the kinds of audiences that give them in relation to presumptively scientific written presentations. Who responds and where? Are there few audiences or many? Of whom are audiences composed? Do these presentations and their conclusions become incorporated as "facts" in another scientist's presentation? Does that make the earlier work "knowledge"? Does the presentation become a matter of controversy, is it debated at length, or is it ignored? Over what length of time are such responses made and is the latest comment the "state of knowledge" of a particular presentation? One could continue. It is only if these kinds of questions are confronted that a rhetorical analysis can present the effect of audiences in the emergence of scientific knowledge.

3. *Is Burke's critical framework the only technique of rhetorical analysis or are there alternatives?*

There are other ways of dealing with the question of rhetorical argument in scientific discourse. It is definitely not the ambiguous genius of Burke's "pentad" or nothing. Rhetoricians have been no more willing than Gusfield (or myself) to take on the natural sciences; nor, with very significant exceptions, have they leapt to analyze discourse in the social sciences. Yet, given Gusfield's (1976) attention to sociological speech, it is crucial to mention the work of Perelman (especially 1969) wherein the sole modern treatise on argumentation that is intended to comprehend the discourse of social science is presented.

Although Perelman's sampling of proofs in the social sciences is hardly systematic, he does address himself to "the use of reason [non-logical argument] in directing our own actions in influencing those of others" (Perelman, 1969:3). It is this precise issue, cast in terms of "policy implications," that concerns the conclusion of Gusfield's (1976) essay. Valuable as are the Burkean and more diffuse perspectives employed in the paper, Gusfield has to go well beyond them to draw conclusions for scientific practise or policy formulation. To give a rhetorical approach a fair

chance of establishing itself as a viable analytic perspective (on sociological, if not all scientific discourse), it is important to begin this kind of analysis with a variety of rhetorical orientations. The systematic reflections of Perelman (1969) would be an essential addition here and might allow for the security of a reliable, empirical investigation of sociological discourse (inter-observer agreement on the interpretation of the rhetorical form of discourse) that is so difficult when using Burke's framework.

This is no place to outline Perelman's work, but it might give readers some sense of how analyses using his approach could proceed if I discuss the form of argument in Gusfield's (1976) essay from a perspective that is informed by a close reading of Perelman (1969). Briefly, I would want to ask and answer two questions. First, what is the general conclusion of the essay? Second, what rhetorical techniques are used to make that conclusion persuasive for his intended audience?

The best candidate for a general conclusion appears at the very end (as one often finds) and is contained in two short sentences:

What is at stake, however, is the necessity of the interpretation and the close connection between that interpretation and its form of presentation, its artistic element. It is in underlining the tenuousness and ambiguity of conclusions that I cannot blink at having called into question the certainty and stability of scientific interpretation. (Gusfield, 1976:32)

Readers will note the crucial switch in level of generality that occurs between these sentences. The first deals with Waller's (1967) essay, the second with all science. I submit that it is this second sentence which carries Gusfield's *general* conclusion. This leads me to my second question. What techniques are used to persuade us that this conclusion is plausible?

If I may skip a great deal of detailed analysis, it is reasonably clear (in part because Gusfield is highly moral in making his own rhetoric so transparent) that the Waller (1967) essay is used as an *example* through which is established a general rule that applies to all scientific discourse. It is through such an argument that one may establish a generalization from a particular case using rhetorical techniques (Perelman, 1969:350-62). Instances of this form of persuasion abound in sociological discourse; all samples are examples! What makes our belief in this generalization more credible is related to the character of the example and the techniques used to convey the plausibility of the conclusion *vis*

à vis that example. One approach to understanding how this is achieved would be to take Gusfield's orientation and apply it reflexively to his own essay. Another would involve some effort to adapt Perelman's general approach to the rhetorical analysis of sociological discourse (for example, Overington, 1975). Unfortunately, this is not the place for such an extended discussion.

4. *What is the relationship between rhetoric and morality in the process of knowledge construction and policy formation?*

So we turn to the final unclarity of Gusfield's (1976) essay. The difficulty here is well summarized in Perelman's (1969:59) note that "rhetorical proof is never compelling." If such proof is not compelling, how shall we decide on the status of conclusions in scientific or, more narrowly, sociological discourse? Gusfield clearly recognizes this problem in his observation on the "unnecessary" character of scientific interpretations (p. 32); but he skirts the issue.

His avoidance may well be rooted in the "Prologue" to the (1976) essay, where (however ironically) he calls rhetoric "the skill perfected by the Sophists" (p. 16). Indeed, Plato (1952: 11) asserts that the "sum and substance" of the Sophists' skill is that of "persuasion." But Sophists care only for persuasions that succeed. Is this how we must think about sociological argument? And if violence is the ultimate persuader, is power the basis of knowledge? Surely we must ask whether we will accept that the *techniques* of persuasion are the "sum and substance" of rhetoric (and thus the grounds of conviction in scientific argument), or whether we will persist with Plato (1952:106) in the view that "rhetoric, like every other practice, is always to be used to serve the ends of justice, and for that alone," even if we might want to substitute "truth" for "justice" in most cases?

The acceptance of a rhetorical approach to scientific (particularly sociological) speaking forces us to face fundamental difficulties in our practice. How are we to formulate truth and justice as ends for persuasive discourse? How does a conventional agreement produce True knowledge? If the "truth" of our knowledge is based on social conventions, what are they? Would it help us to see many of the more important debates in sociology as a struggle over such conventions? Can we really avoid an investigation of how social power operates in the process of knowledge construction? The questions mount.

The pioneering excellence of Gusfield's (1976) paper can only be assisted by a confrontation with the issues raised in this attempt to clarify the radical importance of a rhetorical approach to scientific knowledge.

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REPLY TO OVERINGTON

Since publishing the paper on literary rhetoric of science (Gusfield, 1976), I have had a good deal of correspondence from which I have profited both in understanding and in progress on my current book on knowledge and policy. Michael Overington's (1976) comments raise a number of questions. Some of these had not occurred to me, and some I have been meditating on for many years and expect to mediate on for several more. I am grateful to him as well as to others for these questions.

The most difficult is the problem of the meaning of knowledge in my article. I wish I had a good answer, not only one that would satisfy readers but would satisfy myself as well. I think there are two different problems: one is how is it that scientific findings take on significance or meaning? This involves the interpretation of data rather than the second question: the validity of those findings themselves. I view my own paper as oriented chiefly to the first of these questions because it is here that science moves toward action. This involves understanding how it is that

data become translated or transferred into conclusions and generalizations. My intention in the published paper was to raise the possibility that the use of literary techniques of analysis might help us in understanding how it is that data are developed and conclusions are derived. I recognized (indeed, the revisions on the original paper were a small effort in that direction) that the published paper raised and dodged as many questions as it attempted to solve. Had I waited to have all of these answered in my own mind, I doubt if the paper would ever have been finished. It is an effort to open up a mode of analysis that I hope the paper may be of worth.

As to how scientific knowledge itself is developed, I think Overington has put his finger on an important problem. My paper is by no means a substitute for a more thorough analysis of the relationships between audiences and scientists. It is an effort to try to understand the nature of presentation in science as against that of other modes of communication such as poetry or journalism. I certainly erred, and quite consciously as a rhetorical device, in referring to "the literary rhetoric of science." Since then I have made the acquaintance of Bruno Latour who is conducting field empirical studies of biological scientists. Like myself, he has found the study of these as rhetoric of considerable value. He suggests, however, that it may indeed be the case that social scientists and even medical researchers feel a stronger necessity to articulate a conception of "scientific method" than do workers in many other scientific fields whose credentials are more readily taken for granted. I certainly did not mean to suggest that Burke, Frye and Booth represent the best and only mode of literary criticism of science. I used the three because I knew of them. (Overington has led me to read Perelman.) I am not a literary critic and have not studied literary criticism in any formal fashion. Indeed, I have sent this paper to several colleagues in philosophy and literature and have been met by the stoniest of silence. It suggests to me that they view it as very old hat literary criticism and, consequently, not to be noticed. It may indeed be that; I don't know. But intellectual currents sometimes run even stronger than the river, and it is not terribly crucial for what I hope to do in the paper that I use this or that particular literary critic. Again, my aim in this paper was to say, let's see where we can get by examining scientific documents as if they were artistic ones. I have come to learn that both by my own temperament and

possibly by the sure difficulty of the problem, I need to be something of a "know nothing" who proceeds without being definitive about my philosophical and, sometimes, even theoretical assumption.

It is evident that I have written this letter in a style of "fake humility" about my ideas and my paper. Yet, as one examines the development of areas known as "the state of the art," they generally proceed as if each piece were a definite and finished product. In that sense, they can be contrasted with the also evident way in which one paper is a part of a dialogue and can be seen less as a definitive statement and more as the first round in a set of exchanges. This is what Latour seems to be finding, and it makes the necessary exchanges less matters of cross-examinations and defense than part of the essential character of the scientific community.

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SOCIOECONOMIC STATUS AND CRIMINAL SENTENCES: IS THERE AN ASSOCIATION?

(COMMENT ON CHIRICOS AND WALDO, ASR DECEMBER, 1975)

Chiricos and Waldo (1975) do well to remind us that the empirical evidence for the existence of a strong association between the socioeconomic status of defendants and the severity of the criminal sentences they receive is weak. Their own analysis of prison sentence lengths in Florida, North Carolina and South Carolina finds no evidence for such an association. In assessing the evidence presented on this question and its bearing on the conflict perspective, several points must be kept in mind:

(1) A finding that defendants of different SES receive sentences of equal length is only inconsistent with the claim that lower SES defendants receive more severe sanctions for the same behavior if the offenses committed

are of equal gravity. This may not have been the case, because there could have been class differences in rates of pretrial detention, in resources available to charge and plea bargaining (which in some jurisdictions involves the judge directly), in the presence of mitigating or aggravating circumstances, and in outright differential treatment on the basis of class at all earlier phases of case processing. Evidence for such differences has been reported in previous studies (see, for example, Nagel, 1969:81-112). If lower SES defendants were less culpable than their middle and upper SES counterparts, then the equal sentences they received would constitute evidence of *unequal* treatment. Moreover, the propositions Chiricos and Waldo cite as predicting unequal treatment concern all the dispositions meted out. Yet, as Chiricos and Waldo concede, their study was confined to prison sentences. As many of the dispositions for some of the offenses they studied presumably did not involve incarceration at all (e.g., probation), their study has not excluded class differences in the choice of sanction type.

(2) Some extralegal variables found to be important in other studies were not included in this analysis. One of particular interest is the status of the victim. From a conflict perspective, one might anticipate that middle- and upper-class decision makers would experience little alarm or threat from crimes in which the poor victimize the poor. If judges are drawn from middle and upper strata of the population, one might expect them to feel most threatened and to respond with more severe sentences when victims are most like themselves. Much of the evidence that exists on this point has concerned race rather than class. Wilson (1973:161-4) suggests that in some communities police do not take black complainants very seriously. Earlier studies of sentencing (Garfinkel, 1949; Green, 1964) report inconsistent findings as to the effect of victim's race on sentence; however, a recent study of death sentences for rape in Georgia (Wolfgang and Riedel, 1975) reports a very strong interaction effect involving the race of victim and race of offender. In a stepwise multiple discriminant function analysis, the interaction between race of victim and race of offender entered first and was highly significant in separating death penalty from non-death penalty decisions, while the race of victim alone and the race of defendant alone were not statistically significant. As far as class is concerned, a recent study of homicide sentences in a large northeastern U.S. city

found that when information about class of victim and defendant was included in a path analysis for sentence outcomes, sentences were higher when the victim was of higher SES and lower when the offender was of higher SES (Swigert, 1975). Failure to consider such effects could lead to a misleading conclusion that class was unimportant in sentencing.

In light of these deficiencies, it is fair to say that the data provide no support for the existence of class differences in sentence severity, but are also not incompatible with some patterns of class differences in sentencing.

(3) Chiricos and Waldo analyzed dispositions for the years 1969-1973. During the preceding decade, a number of events took place which may have tended to reduce class disparities in sentencing. One of these is the growth of black political power, manifested in such forms as increased black representation on jury rolls, on prosecutorial and public defender staffs, and on the bench, as well as increased voting strength. The impact of these changes is suggested by James Gibson's study of sentencing in Atlanta (cited by Nagel and Neef, 1976) in which it was found that white judges were giving black defendants harsher sentences than whites within a given offense category, while black judges were reversing this pattern. Failure to disaggregate sentences according to the race of the judge would, in such instances, mask patterns of discrimination. A recent study of the sentencing of California felony offenders (Pope, 1975) also is consistent with this suggestion. An analysis of lower court sentences in 12 California counties found substantial racial differences in disposition in rural areas when legally relevant variables were controlled, but not in urban areas. The black population is concentrated in cities, and it is there that black political strength would be greatest.

In the same period, the civil rights movement may have changed attitudes toward unequal treatment, as publicity concerning inequities in the criminal justice system also may have done. Even without changing attitudes, such publicity could lead to changes in sentencing patterns on the part of judges concerned with preserving the legitimacy of a system under attack. In a political system whose legitimating ideology places great emphasis on formal equality, one would least expect discrimination where decision making is exposed to public view, as in judicial sentencing. It would be more likely to appear at those levels where decisions have less visibility

(police, prosecutorial, parole) and in the legislative determination of the seriousness of offense categories.

In view of the association between race and class, such developments, if they did influence sentencing, would tend to reduce class differences even if they affected only black defendants. If they spilled over to help white defendants as well, the reduction would be even greater.¹ In light of these considerations, it would be of interest to see whether recent sentencing patterns represent a change from earlier periods.

In any event, from the point of view of theory, it is not by any means inherent in a conflict perspective to assume that low SES defendants will always be disadvantaged in court. The collective activities of socially disadvantaged groups can generate sufficient power to offset traditional disabilities. To contain conflicts in a socially unequal society, the upper class may at times need to operate according to universalistic criteria, especially in the most visible spheres of public life. When a class rules, it does not necessarily do so free from all constraints.

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¹ There is some evidence that judges with working class backgrounds are more sympathetic to lower-class defendants regardless of race (Levin, 1972). Although this study involved white judges, one might speculate that the same would hold true for black judges.

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IS THERE A CLASS BIAS IN CRIMINAL SENTENCING?

(COMMENT ON CHIRICOS AND WALDO, ASR DECEMBER, 1975)

Chiricos and Waldo claim to have tested one of the propositions of "conflict criminology" and found it wanting. But there are problems with their procedure, and I shall argue here that an alternative method of analysis is more appropriate.

The proposition in question, asserted in the first place by Chambliss (1969:86), is that "the lower class person . . . if found guilty, [is] more likely to receive harsh punishment than his middle or upper class counterpart." To test this, Chiricos and Waldo assemble data on a cohort of criminals sentenced to prison for varying periods. They compute a socioeconomic status score for each inmate and, treating SES as a continuous variable, correlate it with length of sentence. They conclude that generally there is no significant relationship between a defendant's SES and the length of his sentence.

The problem with this procedure arises from the investigators' decision to treat SES as a continuous variable. This may be, in part, a result of imprecision in the writings of conflict criminologists themselves; they do sometimes formulate their propositions in continuous terms. However, the proposition in question treats status as essentially dichotomous: it compares the "lower class person" with his "middle or upper class counterpart."

Moreover, there are good theoretical reasons for treating status as a dichotomy rather than as a continuous variable in this context. Studies have shown (see Warner, 1969:182)

that although people may be sensitive to quite minor status distinctions between themselves and others they are largely insensitive to minor differences among people whose status is very different from their own. Middle or upper class judges thus are unlikely to make fine status distinctions among the predominantly lower class defendants whom they sentence. They are unlikely, for example, to perceive any status difference between two unemployed men, one with eight years schooling and the other, seven. It seems more plausible to suggest that judges will make status discriminations as follows. *Either* the judge perceives the defendant as someone with whom he can identify, someone with a similar background to his own who is not inherently criminal and whose criminal behaviour is felt to be an unfortunate consequence of particular circumstances or social pressures, *or* he sees the defendant as belonging to a lower social class and, as such, prone to criminal behaviour. If judges, in fact, think this way, then an adequate test of the Chambliss proposition must treat status as a dichotomous variable.

Furthermore, one of the reasons that conflict theorists give for expecting more lenient treatment for high status defendants is that status is often an indicator of political power and that, perhaps inadvertently, the courts tend to deal more warily with the politically powerful in order to avoid adverse reaction (Chambliss and Seidman, 1971:475). But the great majority of defendants are quite powerless politically, and it is only the few middle and upper class defendants who might be expected to wield some degree of power. The theory thus requires a comparison between those who have some degree of power and those who have none—between, in other words, the lower class on the one hand and the middle and upper classes on the other.

The above arguments imply that there will be no significant relationship between SES score and length of sentence *within the lower class*. Yet because middle and upper class inmates constitute a tiny fraction of the cohort studied (as of course they will in any such research) it is effectively this "within-class" relationship which Chiricos and Waldo are measuring and which, not surprisingly, they find to be insignificant. A more appropriate test of the Chambliss proposition would be to separate upper and middle class inmates from lower class inmates and compare the average length of sentence for individuals in these two groups. Of course, the choice of just where to dichotomize the SES continuum

would be arbitrary to a degree, but it would need to be made in such a way that the great bulk of defendants fell within the lower class.

Broadening this discussion beyond the task which Chiricos and Waldo set themselves, I suspect that whatever method of analysis is used, the class bias involved in sentencing individuals who have committed the same type of crime will be found to be relatively slight. The real class bias in sentencing lies elsewhere. The key to it is that, as Chiricos and Waldo demonstrate, there are so few middle or upper class people convicted of or, for that matter, charged with the traditional crimes of murder, robbery, larceny and so on. This is simply not the way higher class people behave. The illegal activities of the middle and upper classes are typically such things as income tax evasion and, in the case of businessmen, price fixing, violation of pollution laws and misrepresentation in advertising. Moreover, the sentences for such crimes are normally very light, often amounting to no more than warnings not to offend again (injunctions). In the most celebrated case of white-collar crime in American history, a price-fixing conspiracy among 29 electrical equipment companies which is estimated to have cost the general public more money than is reported stolen by burglars in an entire year in the U.S. (President's Commission, 1968:24), the principal defendants received sentences of only 30 days. The fact is that illegal behaviour of the type typically committed by the middle and upper classes attracts light sentences while crimes typically engaged in by the lower classes attract much harsher penalties. This is the real nature of the class bias in sentencing.

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ON METHODOLOGY, THEORY AND IDEOLOGY *

(COMMENT ON CHIRICOS AND WALDO, ASR DECEMBER, 1975)

Chiricos and Waldo (1975) have recently presented research which purportedly tests a conflict theory proposition and provides rather conclusive evidence that the proposition is invalid and, thus, the theory is weakened. It is contended here that their inquiry fails to test adequately the proposition and, thus, the theory. Furthermore, even if such a test had been undertaken it may be irrelevant given possible differences in the ideology and methodology of functional and conflict perspectives.

Chiricos and Waldo purport to assess empirically the worth of the following two propositions from conflict criminology.

Chambliss (1969:86) proposes that a lower class person "is if found guilty, more likely to receive harsh punishment than his middle or upper class counterpart."

Chambliss and Seidman (1971:475) propose that "when sanctions are imposed, the most severe sanctions will be imposed on persons in the lowest social class."

Chiricos and Waldo note that the latter proposition is only one (number fifteen to be exact) of twenty-three propositions; however, they assert (1975:755, fn.5) that their research "is clearly addressing one of the two or three most important propositions in Chambliss and Seidman's theory" (emphasis added).

Therefore, the empirical question posed is the extent to which social class influences the harshness of sanction received by convicted persons.

Chiricos and Waldo use data on prisoners in three states to test the above propositions. Therefore, they are not testing the propositions as stated by Chambliss, and Chambliss and Seidman. The authors note this limitation.

It must be recognized that a more complete assessment of the sentencing process would include those sentences received by probationers

* I would like to acknowledge the helpful comments and suggestions by Colin Goff, John Klein and Arthur Frank on an earlier draft of this comment.

as well as by those remanded to prison. However, these data are unavailable at this time and the present research must be regarded as but a partial test of the Chambliss-Seidman proposition. (Chiricos and Waldo, 1975:758, emphasis added)

Thus, they have moved from "clearly addressing" the proposition to a "partial test." In fact, the analysis fails to test the propositions, and the data presented could easily be interpreted as supportive of the conflict proposition rather than negating that perspective.

First, they are addressing a population which has received the most severe possible sanction, i.e., incarceration. This population is actually but a sample of a much larger convicted population receiving probation, fines, suspended sentence, restitution and/or other available sanctions (the extent to which the prison population approximates those convicted is an unknown, but crucial factor). Therefore, Chiricos and Waldo have a small and select representation of convicted persons and actually are testing their own proposition which they believe is derived from conflict theory—among prison populations those with the lowest SES status will have received longer sentences for the same offense than those with higher SES status.

This is an excellent example of the data's inapplicability to the theory because of the nature of the confined population. The Chambliss, and Chambliss and Seidman propositions specifically use social class as the significant independent variable in the imposition of sanctions—"when sanctions are imposed, the most severe sanctions will be imposed on the persons in the lowest social class" (Chambliss and Seidman, 1971:475, emphasis added).

However, Chiricos and Waldo apparently only have "lower class" people in their population! Therefore, they are attempting to make "within-class" distinctions while the conflict theorists are talking about "between-class" differences.

While Chiricos and Waldo find "within-class" differences among inmates in SES scores, the significance of their initial question is largely lost. They note that there is sufficient "class" variation to provide appropriate testing (1975:759, fn.14). However, "sufficient" variations around means of 32.89, 31.86 and 36.59 on a scale of 100 appear "insufficient" to address the initial proposition. While these within-class differences might interest the researcher, they do not begin to address the initial question posed by conflict theorists.

Since a cursory look at any state prison

population provides ready evidence of the class nature of its inmates, Chiricos and Waldo's "discovery" of the low SES nature of their population should not be surprising. In fact, such data support the propositions being tested since incarceration is the most severe sanction meted out by courts. Furthermore, according to Chambliss and Seidman's (1971) theory of legal systems in complex societies, differences in the class basis of laws, their enforcement and administration would have the "cumulative effect" of leading to disproportionate representation of the lower class in prisons.

Chiricos and Waldo (p. 758) address this problem in their discussion of computing SES.

Given the backgrounds of most persons who filter through the criminal justice process to the prison level, it makes little sense to speak of "high," "middle" and "low" status in traditional terms.

From the conflict perspective, the question might be why there is such a disproportionate representation, rather than an attempt to assess within-class differences among the incarcerated. The issue is resolved for Chiricos and Waldo when they state (1975:759, fn. 14):

Unfortunately, since this is the nature of the "real world," this is the kind of population the researcher is forced to use if sentence length is to be the dependent variable.

Unfortunately, the nature of the "real world" as defined in the initial proposition is radically changed by largely eliminating the most significant variable, i.e., social class. While one obviously cannot "create" inmates of middle and upper classes, if those convicted offenders not incarcerated had been included, it would likely increase representation by other classes. The nature of the "real world" evidenced by the skewed class distribution of inmates can be interpreted as supporting the Chambliss proposition specifically, and conflict theory in general.

Chiricos and Waldo (p. 766) conclude that:

What the foregoing data suggest, rather conclusively, is that the socioeconomic status of convicted criminal offenders is unrelated to the severity of the state's official sanction, as reflected in the length of prison terms assigned by the courts. Such a conclusion, which strongly contradicts common folklore, the general expectation of conflict criminology and the specific proposition of Chambliss and Seidman . . . , is given added credence by the fact that it is true for a total of seventeen different criminal offenses and for three separate states.

It would be more appropriate to conclude that the foregoing data suggest that "within-

class" differences in SES scores among lower class inmates is unrelated to the severity of their sentences as reflected in the length of prison terms assigned by the courts. Such a conclusion, which may surprise some, fails to address the Chambliss and Seidman proposition specifically, and conflict theorists' concerns generally.¹

A larger and potentially more significant issue is raised by Chiricos and Waldo's attempt to test a conflict theory proposition. This brings us to the relationship between ideology, methodology and theory and the possible incompatibility of testing one perspective's theory from another perspective's methodology. While the authors note that conflict theory has ideological appeal as a "world view," they fail to take seriously this relationship between ideology, "world view" and the practice of science.

Chiricos and Waldo (p. 754), however, note that "conflict criminology begins with a set of domain assumptions that are radically different from those of functional criminology." What are the methodological ramifications of these differences? Even if one were to accept their judgment that there are no paradigms in sociology, it is arguable that these "competing theories" have different methodologies.²

¹ Purposefully, I have not addressed their specific findings because the other issues overshadow them and render their interpretation largely meaningless for the initial question. Nonetheless, it is interesting to note that most inmates in Florida are black (although racial data are not presented for the other two states—North Carolina and South Carolina—I would guess they would be similar to Florida) in great proportion to their population in the state (p. 763). One interpretation of such data (there obviously could be others) from conflict theory would be that this reflected the cumulative effect of racial oppression in the making of laws, their enforcement and the administration of justice.

It is of particular interest to note that only blacks commit forcible rape in Florida as defined by the lack of any whites incarcerated for this offense. Might this reflect the history of black-white relationships and their representation in the laws, their enforcement and administration? Of course, the above-noted data could be interpreted differently, which is the basis for the next section of this comment.

² While the thrust of this argument is not to make a case for paradigms in sociology, I would conclude that Kuhn's (1970) analysis, particularly his notion of scientific subcommunities having varying paradigms, is applicable to sociology. While the case for "normal" science in sociology is more difficult to make, it is increasingly being

While Chiricos and Waldo use Chambliss (1973) as a basis for differentiating functional and conflict theories, they fail to note that Chambliss identifies functional and conflict perspectives on the basis of their varying focus, methodologies and underlying ideologies. Using Durkheim and Marx as exemplars of the functionalist and conflict paradigms, respectively, Chambliss (1973:4) derives four fundamental issues separating them "(1) the role of history in sociological analysis, (2) centrality of a critical analysis of the existing social conditions, (3) the methodology appropriate to social sciences and (4) the meaning attached to the normative system." Chiricos and Waldo address the issues in (2) and (4), but leave (1) and (3) virtually untouched. Chambliss (1973:4) states that while both study history they do so differently.

Functionalists see society as a reality which is unconnected with a particular historical period and look for those social needs which all societies have. . . . The Marxian analysis begins with the observation that the needs, characteristics, ideologies, and institutions of a particular society are a reflection of that society's historical condition—especially the material conditions (the mode of production) at that particular historical moment.

Regarding appropriate methodology, Chambliss states the Marxian conflict perspective sees the social scientist as necessarily engaged in bringing about the changes implied by his analysis.

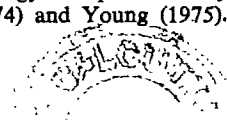
The ultimate test of a theory's utility is not its logical structure or its fit with empirical data but its ability to create workable recipes for changing the existing set of social conditions (both material conditions and the superstructure derived therefrom). (Chambliss, 1973:4, emphasis added)

Regarding the worth of conflict theory, Chiricos and Waldo (p. 754) state:

However persuasive the logic or appealing the scientific utility require that it be formulated as testable research hypotheses which can direct the accumulation of relevant empirical ideology of such an argument, its validity and evidence. It is on the basis of such evidence that the acceptability of a "conflict perspective" as an alternative world view in criminology specifically, and sociology more generally, will ultimately be determined. . . . (Emphasis added)

These two contrary positions on the purpose and nature of science reflect radically

asserted that sociology is a multiple paradigm science (Friedrichs, 1970; Ritzer, 1975) and that criminology also has competing paradigms (Taylor et al., 1973; Reasons, 1975). Recent discussions of conflict methodology are presented by Lehmann and Young (1974) and Young (1975).



different assumptions. Chambliss is presenting an activist theory of knowledge emphasizing the methodological differences between functional and conflict perspectives. This approach closely follows some of Horton's (1966) major distinctions between order (including functional) and conflict perspectives. In discussing the significant methodological differences between order and conflict theories, Horton identifies the order perspective with the natural science model, multiple causality and conditions of objectivity while the conflict perspective emphasizes the historical model, unicity and utility in terms of the observer's interests (objectivity discussed in the context of subjectivity). Chiricos and Waldo's analysis clearly fits the natural science model of objectivity and a passive/receptive theory of knowledge. Thus, while Chambliss rejects some of the major tenets of neo-positivists³ and emphasizes the utility of theory for social change strategies (like Horton), Chiricos and Waldo assume that the acceptance of a theory is based upon its fit with the "empirical world." This basic disagreement derives from differing perspectives on the role of ideology in the practice and development of science.

Chiricos and Waldo, like many others, tend to go about "normal science" within the confines of certain domain assumptions and directives without addressing the significance of ideology in the development of scientific theories in general and criminological ones

specifically (Radzinowicz, 1966; Miller, 1973; Quinney, 1972; Gordon, 1973; Reasons, 1975). The nature of one's "world view" and its developmental process is greatly influenced by "irrational" and "particularistic" factors as well as by "rational" and "universalistic" factors (Dolbeare and Dolbeare, 1973; Marchak, 1975). This is not to say that ideologies and world views never change among "laymen" and "scientists"; however, it entails much more than the "presentation of the facts" to make a person accept an alternative "world view." By definition, the concept of "world view" entails much more than an impassioned presentation of facts. As Kuhn (1970) notes, one filters the facts through a "world view" already established which predefines the way of doing science, including determining the worth of theories.

Thus, the differences between functional and conflict perspectives may include irreconcilable ideological, theoretical and methodological assumptions which may make it impossible for either perspective to test and judge the other within these assumptions. Therefore, the test of the conflict proposition by Chiricos and Waldo, even if "appropriately done," may not grasp the nature of the issues involved. As Kuhn (1970:146) notes:

If as I have already urged, there can be no scientifically neutral system of language or concepts, then the proposed construction of alternative tests and theories must proceed within one or another paradigm-based tradition. Thus restricted it would have no access to all possible experiences or to all possible theories. As a result, probabilistic theories disguise the verification situation as much as they illuminate it.

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³ The major characteristics of criminological positivism are that it (1) denies free will, (2) divorces science and law from morals, (3) proclaims the priority of science and believes in the existence of invariable social laws (4) emphasizes the unity of the scientific method for social and natural sciences, (5) emphasizes quantitative research, not qualitative, and (6) holds causality and determinism as paramount concerns to be pursued through observation (Mannheim, 1960). While noting that all social science is deterministic, Matza (1964) distinguishes between "hard" and "soft" determinism. The former has long been the major thrust of American criminological analysis and closely adheres to the characteristics of positivism noted above, while soft determinism allows for the incorporation of a degree of free will or choice in human behaviour. Matza (1969) later suggests that the positive school of criminology separated the study of crime from the workings and theory of the state and he advances a naturalist perspective of deviance, including crime. Of course, determinism and free will are not mutually exclusive but a matter of degree, i.e., some are more free than others (Wilkins, 1968; Pap, 1962).

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REPLY TO GREENBERG, HOPKINS, AND REASONS

A problem facing any scientific perspective is what to do with contrary evidence or "bad news" such as that implied for some aspects of conflict criminology by our recent sentencing research (Chiricos and Waldo, 1975) and other related studies (Hagan, 1974; Hewitt, 1975; Willick et al., 1975). The commentaries of Greenberg, Hopkins, and Reasons reflect contrasting options for dealing with the "bad news." The first option, essentially Greenberg's and partially Hopkins', considers the findings "real" (with some limitations) and contributes to the renewed growth of conflict theory by suggesting ways in which "no relationship" between SES and sentencing could make sense—even in conflict terms. The second option, essentially Reasons', denies the reality of the "bad news" or the relevance of such news for the conflict perspective, thereby preserving the integrity of the proposition in question. Our reply will underscore the limitations and dangers of the second option, while hopefully expanding the constructive input to conflict theory of the first option.

At the most general level, Reasons argues (with support from Hopkins on this issue) that our data "fit poorly" with conflict theory and should not be regarded as "bad news" at all. Reasons further argues that *any* empirical data may be irrelevant anyway, because the conflict perspective employs unique and distinctive standards of value in judging concepts and theories. The "poor fit" and "irrelevance" arguments are briefly summarized as follows:

Poor Fit: Studies such as ours, which use available populations of defendants, necessarily have skewed distributions of social class or SES, inasmuch as most higher status persons never penetrate the criminal justice system. Thus, our data presumably forces us into a *within-class* analysis of a *between-class* issue. *Irrelevant:* Studies which rely upon traditional empirical methodologies are not especially relevant to assessing the value of conflict propositions because of the latter's ideological emphasis upon praxis or action as the determinant of an idea's value.

We contend that the "poor fit" argument hinders the development of conflict criminology because it commits its proponents to a model of social control that is ambiguous, insufficient and, quite possibly, unnecessary on both empirical and theoretical grounds. The "irrelevance" argument hinders conflict theory because it commits its proponents to a kind of false consciousness which Gouldner

(1975) shows to follow from a misunderstanding of the relationship between ideology and objectivity.

On the Poor Fit between Theory and Data

When theory and data are poorly matched, the problem may lie, of course, either with the conceptualization or the measurement of reality. We contend that the problem of fit in this instance is more likely due to inadequacies with theory than with methodology, for reasons that are briefly noted here.

First, we note that it is difficult to determine when an adequate fit exists between theory and data because of considerable ambiguity in conceptualization, not only between conflict criminologists, but within the propositions of Chambliss and Seidman's (1971) theory. The problem is most acute when trying to determine the appropriate independent variables in conflict accounts of criminal justice outcomes. While Chambliss and Seidman employ "social class" as the primary determinant, the degree of consensus among conflict criminologists on this question is minimal. For example, Quinney (1970) relies upon "conflicts" between "pluralistic interests" while Turk (1969) employs "conflicts" between "authorities and subjects in imperatively coordinated associations." Davis (1975) comes closer to social class with her use of the "relative amount of resources held by the conflicting party." Still, for Taylor et al. (1973:273) none of these (presumably they would include Davis) is really adequate, for none shows how state sanctions are "intimately bound up with the structure of the political economy of the state." In short, the conceptual apparatus by which conflict theorists explain criminal sanction is highly varied and seldom relies upon a simple specification of social class discrimination.

A closer look at Chambliss and Seidman (1971) raises further doubts about what their theory intends by the concept of "social class." That theory not only employs several *different* concepts to reference the same phenomenon (presumably poverty) but conceives of that phenomenon in *both* discrete and continuous terms. The nature of the problem is readily seen in propositions #13, #14 and #15 from Chambliss and Seidman (1971: 475):

13. Therefore, those laws which prohibit certain types of behavior popular among lower-class persons are more likely to be enforced while laws restricting the behavior of middle- or upper-class persons are not likely to be enforced.

14. Where laws are so stated that people of all classes are equally likely to violate them, the lower the social position of an offender, the greater the likelihood that sanctions will be imposed on him.
15. When sanctions are imposed, the most severe sanctions will be imposed on persons in the lowest social class. (Emphases added)

From the foregoing, it is apparent that propositions #13 and #15 deal with "social class"—though the concept is not defined—while proposition #14 deals with "social position"—also undefined. More importantly, while #13 enjoins a comparison of "lower-class" defendants with "middle-" and "upper-class" defendants, propositions #14 and #15 are stated so that differences in social position or class can be understood in continuous terms. Indeed, the only consistent point of reference in those propositions is the "lower" or "lowest" end of the continuum. Thus, we would argue that considering all three propositions, the most relevant test would compare differences in sentencing outcome between the "lowest" status defendants and all others—a comparison which data such as ours (omitting upper status persons) obviously can make for the population at risk. In short, the unavailability of upper status defendants in our sample would not appear to disqualify the data from application to the Chambliss and Seidman (1971) theory.

Though we chose—for reasons not unrelated to Chambliss and Seidman's concepts—to treat SES as a continuous variable in our initial paper, we have pursued Hopkins' suggestion to handle the matter empirically in dichotomous terms. Thus, we have compared the *lowest* status defendants—somewhat analogous to "dangerous classes" (Silver, 1967)—to all of the remaining defendants in our Florida sample.

Cutting points for determining the "lowest social class" using an SES continuum must be, as Hopkins notes, an arbitrary matter. We have approached the task from three points of view. In the first instance, we have compared the mean sentence received by the bottom 5% (< 10) on the SES distribution with all others (≥ 10) as well as the bottom 12% (≤ 15) versus all others (> 15). The latter dichotomy renders a "lowest class" which closely approximates the bottom class identified by Willick et al. (1975) and by Laumann (1966) as unskilled and semi-skilled workers. We have also followed Hopkins' suggestion—though we think it more defensible to make the foregoing dichotomies

—that "the great bulk of defendants" fall within the lowest class. Toward that end, we have compared the bottom 90% (≤ 54.3) with the top 10% and the bottom 95% (≤ 63.5) with the top 5% of our sample. Our third dichotomy split the SES distribution at the median (30.0). It should be noted that, based upon the five social class categories employed by Willick et al and by Laumann, our sample extends well into the second highest class—referred to as "upper-middle" by the former and as "semiprofessional and middle level business" by the latter. Thus, it is difficult to sustain the argument that we consider only *within-class* differences.

The data, examined dichotomously with the use of t-tests, support our initial findings. In *no instance* did we find lower status defendants receiving significantly greater sentences than higher status defendants for the same offenses. Beyond the issue of statistical significance and excluding comparisons when fewer than ten cases were available for any status and offense category, we found that lower and higher status groups were almost equally likely to receive the stronger sanction. When the lowest class comprised basically unskilled and semiskilled workers (first dichotomy), it received higher sentences in nine of the seventeen available comparisons. When the lowest class included the "great bulk of defendants" (second dichotomy), it received the stronger sanction in six of eleven comparisons; and for the median split (third dichotomy), the lowest class received higher average sentences in six of thirteen comparisons.

Pursuing the question of "poor fit" in another direction, it seems reasonable to consider whether the lack of fit is due to the application of a faulty theory to the data at hand. As articulated by Chambliss and Seidman (1971), the theory is stated in simple and sufficient terms: social class is inversely related to sanction severity. The stark simplicity, involving but *one* determinant—social class—may be insufficient to the task of describing and explaining sentencing realities, even from within a conflict perspective. Indeed, this insufficiency is clearly recognized by both Hopkins and Greenberg, with the latter observing that "... it is not by any means inherent in a conflict perspective to assume that low SES defendants will always be disadvantaged in court."

We are suggesting that if the concept of social class has a viable role in conflict explanations of sentencing, it is likely not on

the basis of a simple model of class discrimination. To insist on such a model is to ignore the bulk of empirical evidence and the contributions of more recent conflict criminologists who specify additional contingencies for a conflict explanation.

Perhaps the most significant contingency is perceived *threat* to elite interests, a circumstance whose variation need not be isomorphic with variation in social class. This issue has been explicitly recognized by Spitzer (1975), Davis (1975) and Lofland (1969), and it is implicitly present in the discussions of conflict by Turk (1969) and Quinney (1970). Though Spitzer's analysis is not specific to the question of sentencing, he does argue that social control varies with the nature of threat posed by various "problem populations" within capitalist society. "Social junk" comprises non-productive citizens who are either too old, young, feeble, infirm or insane to warrant strongly punitive control measures. "Social dynamite," however, represents surplus labor that is young, able, angry, alienated and capable of revolutionary intention, thereby justifying harsher control sanctions.

The work of Turk (1969), Lofland (1969) and Davis (1975) suggests the relevance of additional contingencies of threat, such as the size, degree of sophistication and level of organization of threatening groups. Greenberg, in his commentary, made careful note of the victim's character and relationship to offender as consequential for threat that may be perceived and responded to by the courts. In short, threat is but one of the more frequently mentioned contingencies of social control that cause us to doubt the sufficiency of social class in a conflict theory of sentencing.

One might argue further that social class isn't even necessary for a conflict explanation of sentencing, or at least that conflict theory shouldn't be surprised if social class were inconsequential. This appears to be Greenberg's point when he observes that movements of the 1960s—such as the civil rights movement—may have increased the liberal self-consciousness of bourgeois justice, or at least the fear of appearing to violate one's legitimating ideology of egalitarianism. Hopkins, as well, argues that the inconsequential character of social class for sentencing should not be surprising to a conflict theorist, inasmuch as "the real class bias in sentencing lies elsewhere"—particularly in legislative decisions which attach stronger penalties to typically lower-class activities and lighter penalties to

white-collar behavior. The two arguments are readily combined. Given the conflict assumption that law and the definition of crime are part of the superstructure whereby ruling class interests are both protected and excluded from serious criminal sanction, then an ideology and even the practice of egalitarianism at the point of sentencing should pose no threat to elite class interests. In fact, such egalitarianism effectively screens critical attention from the ways in which class control is exercised within criminal justice. Ironically, if we persist in our belief that discrimination exists in sentencing, then we reduce the general credibility of the conflict position and, at the same time, we divert critical attention from the real issues of class dominance.

On the Irrelevance of Conventional Methodology for Conflict Propositions

Reasons argues that empirical findings generated by traditional positivist methodology—incredibly equated with functionalism—are not especially relevant for conflict propositions, because the latter are judged more appropriately by their action implications. This irrelevance, in short, derives from what are perceived to be basic and irreconcilable differences between conflict and positive methodologies, with the former stressing ideology and the latter objectivity.

In reply, we argue that ideology and objectivity are necessary components of *both* perspectives, and to assume that they are mutually exclusive is to misunderstand the nature of objectivity as described in Gouldner's (1975) recent essay and to risk a debilitating kind of false consciousness.

The utility of ideas for courses of action is hardly an evaluative criterion that is unique to conflict theory. The premise of scientific inquiry is that we explain in order that we might control, and it is not uncommon for basic essays on the nature of theory to include "pragmatic utility" as a criterion for evaluating theoretical positions (Reynolds, 1971; Schrag, 1967). This point has been strongly emphasized by Gouldner (1970:47-8), who contends that all theory is "normative" and has action imperatives:

every social theory facilitates the pursuit of some, but not all courses of *action*, and thus encourages us to change or to accept the world as it is, to say yea or nay to it. . . . In this sense, every theory and every theorist ideologizes social reality.

Thus, what distinguishes conflict from other perspectives is not that pragmatic utility is

relevant, but rather *what* pragmatic utilities are implied. Durkheim on social solidarity and Marx on social change are not distinguished as empirically relevant in the first instance and normatively relevant in the second. Rather, for both, action implications are manifest—even if the action is to consolidate order.

That empirical evidence is relevant for conflict analysis should be readily obvious to most. The science in Marx's "scientific socialism" is not limited to general theoretical laws, but involves, as well, a painstaking attention to empirical fact when available—as in the variety of *tables* employed to illustrate the "General Law of Capitalist Accumulation." The importance of empirical evidence is clearly implied in the formulation of "testable" propositions by Turk (1969), Chambliss and Seidman (1971) and Davis (1975). Chambliss (1973:2) has recently observed:

A theory . . . answers a specific question and, if it is to be scientifically useful, must be capable of being proved wrong. . . . We choose one theory or another because it has stood the test of empirical research. Data support the theories that stand the test of time and refute the theories that do not.

Finally, to assume the opposition of ideology and objectivity is to make an error that could prove disastrous, not only on scientific grounds but on purely tactical grounds as well. Gouldner (1975) contends that objectivity is "not neutrality" but *openness* in our cognitive processes so that we are able to "face the bad news" and "not exaggerate the good news." The tactical (action) implications of such openness or objectivity is concisely summarized by Gouldner (1975:5):

More specifically, objectivity has to do with the continued openness or access of persons to "hostile" reports; the lack or loss of objectivity is an underestimate of the negative implications of reports; it is an *under-estimate* of our *adversary's strengths* and an *over-estimate* of *ours*.

Thus, the ideology and worldly objectives of conflict theory, in no sense, remove the necessity of objectivity; indeed, it is the latter which serves the former by providing the bases for rational, self-interested, goal-directed behavior.

Essentially one must suppose that, for critical theory, emancipation and truth are not totally identical; one must suppose that truth serves emancipation and life, the part the whole. (Gouldner, 1975:7)

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dealing with the role of prior offense record and the nature of the offender-victim relationship in the adjudication of criminal defendants. RONALD A. FARRELL is Associate Professor of Sociology at the State University of New York, Albany. In addition to collaboration with Swigert in the study of differential legal treatment, his current research includes the development and empirical exploration of causal models of social deviance and analyses of the social psychological aspects of dream content. Swigert and Farrell also have collaborated on two recent books: *Social Deviance*, by Farrell and Swigert (Lippincott, 1975) and *Murder, Inequality and the Law*, by Swigert and Farrell (Lexington Books, Heath, 1977).

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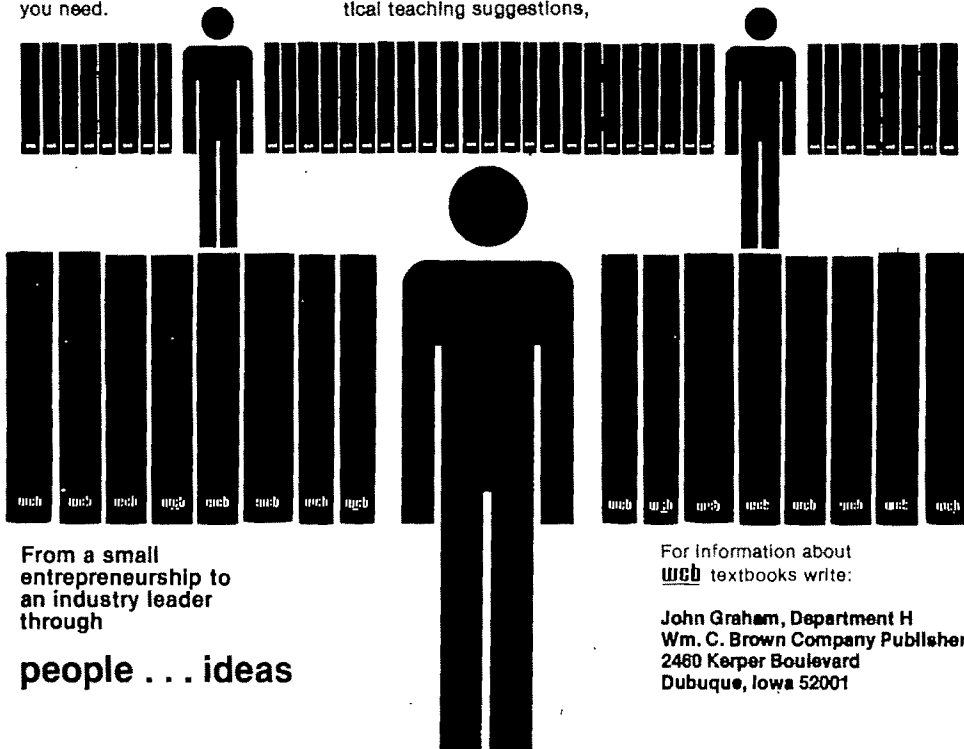
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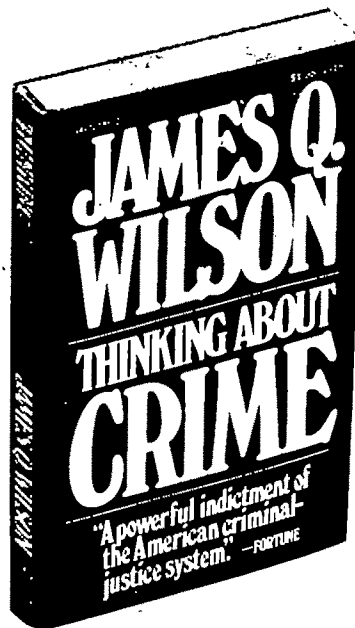
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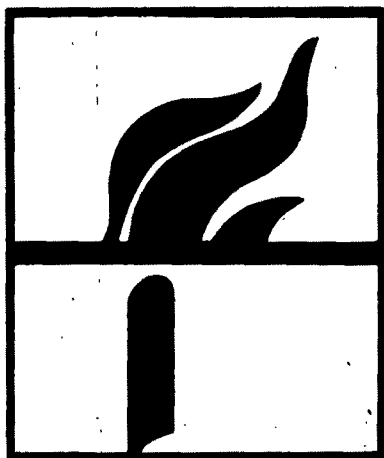
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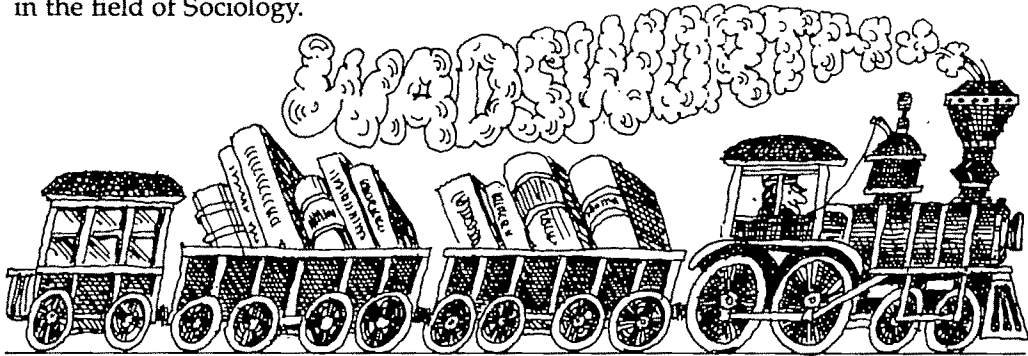
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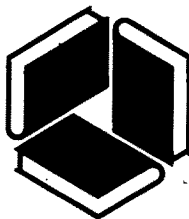


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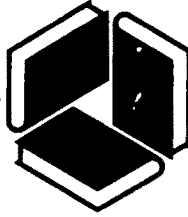
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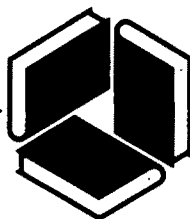
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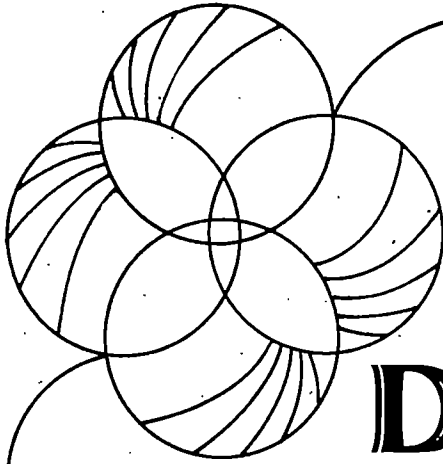
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Beginning July 1, 1977, send all manuscripts to:

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ITEMS

April, 1977

■ REYNOLDS FARLEY (Trends in Racial Inequalities) is an Associate Director of the Population Studies Center and Professor of Sociology at the University of Michigan. He has been analyzing demographic and survey data concerning the causes of racial residential segregation in the Detroit metropolitan area and is completing

a study of the impact of school desegregation on white flight.

■ DAVID H. KAMENS (Legitimizing Myths of Educational Organization) is Assistant Professor of Sociology at Northern Illinois University. In addition to current work on the role of schools in the status attainment process and an analysis of societal factors affecting the jurisdic-

(Continued on page 383)

TRENDS IN RACIAL INEQUALITIES: HAVE THE GAINS OF THE 1960s DISAPPEARED IN THE 1970s?

REYNOLDS FARLEY

University of Michigan, Ann Arbor

American Sociological Review 1977, Vol. 42 (April):189-208

Since the end of the Depression the black population has become urbanized, and black leaders increasingly have stressed civil rights grievances. Federal courts have overturned many segregationist practices, and Congress has enacted encompassing civil rights legislation. The nation's economy expanded rapidly in the 1960s, and economic growth may account for the improvements in the status of blacks registered in that decade. We would anticipate that the improvements of the 1960s would be negated in the recession of the 1970s. Investigation of recent trends in education, employment, occupations, family income and personal earnings shows that gains made in the 1960s did not disappear. Indeed, racial differences attenuated in the lean 1970s just as they did in the prosperous 1960s. The changes of the post-Depression period apparently mean that even during a pervasive recession blacks did not lose the gains they previously experienced. Despite these improvements, racial differences remain large and will not disappear soon.

Since the end of the Depression, four major trends in race relations are evident. First, blacks left the rural South and are now geographically dispersed. In 1940, 80 percent of the nation's blacks lived in the South and almost two-thirds of the southern blacks were in rural areas. By the mid-1970s, the proportion in the South decreased to 53 percent and a higher proportion of blacks than whites lived within metropolitan areas (U.S. Bureau of the Census, 1943a: Table 3; 1975a: Tables 3 and 4). There has been a concomitant change in occupations as the proportion of employed blacks working on farms fell from 32 percent to two percent (U.S. Bureau of the Census, 1943b: Table 62; 1975a: Tables 48 and 49).

Second, black demands for civil rights have increased. During the early 1940s, A. Philip Randolph led the struggle for equitable jobs. A decade later the NAACP won court battles concerning equal opportunities in employment, housing and schooling and, in the 1960s, Martin Luther King focused the nation's attention upon the civil rights grievances of southern blacks (Franklin, 1967:ch. XXIX).

Third, governmental actions to imple-

ment the Thirteenth, Fourteenth, and Fifteenth Amendments became more numerous. At the outset of World War II, President Roosevelt established a Fair Employment Practices Commission and, after the war, President Truman's Executive Order 9981 sought to integrate the armed forces. Supreme Court rulings in the 1950s overturned the separate-but-equal principle and outlawed many discriminatory practices. The Twenty-Fourth Amendment encouraged the participation of blacks in federal elections. The Civil Rights Act of 1964 forbade discrimination in employment and public accommodations, sought to end school segregation and created the Equal Employment Opportunity Commission. After the assassination of Dr. King, Congress enacted a civil rights law forbidding discrimination in the sale or rental of most housing. Many programs in the 1960s were designed to assist the poor—especially blacks—by providing federal monies for local schools, for urban renewal and for job training.

Finally, white attitudes and beliefs about blacks have become less prejudicial. Between the early 1940s and the

mid-1960s, the proportion of national samples of whites who believed that Negroes were as intelligent and educable as whites rose from about 40 percent to 80 percent (Hyman and Sheatsley, 1964:6). The proportion of whites who said they would not be disturbed if a Negro with the same income and education moved onto their block rose from 35 percent in 1942 to 84 percent in 1972. During the same span, the proportion of whites who claimed that children of both races should attend the same—not separate—schools increased from 30 to 84 percent (Sheatsley, 1966:219-22; National Opinion Research Center, 1972: questions 39 and 46).

Each of these trends should have reduced the traditional isolation of blacks and may have allowed them to compete more equitably with whites. At the macro level, changes are evident. Racial differences in educational attainment have gradually decreased, more than a few blacks now work at the occupations once reserved to whites and, since the end of World War II, the average income of black families has risen slightly more rapidly than that of white families (Levitan et al., 1975: chs. 2, 3 and 4).

These changes occurred during an era of general economic expansion. Thirty years ago, Myrdal (1962:401) noted the marginal status of blacks and stressed that opportunities for advancement depended upon national economic trends. Rose (1964:313) elaborating upon Myrdal's findings, observed that blacks made gains during World War II but he argued, "The real test of whether Negroes have been granted equal economic opportunity will come if and when America again faces mass unemployment." During the 1960s, economic growth rates were high and conditions favored the incorporation of marginal groups. The 1970s differ markedly. Not only has the unemployment rate risen (U.S. Bureau of Labor Statistics, 1976: Table A-1) but real family income has declined (U.S. Bureau of the Census, 1976c: Table 11). At the same time, support for civil rights legislation has waned, no new civil rights acts have been passed and anti-poverty programs have been dismantled.

If the 1940 to 1970 changes altered the

status of blacks and incorporated them into important positions in the nation's economic and political structure, we would expect that even during a recession blacks would maintain the gains made during the prosperous 1960s. On the other hand, if the gains of the 1960s were merely the consequence of economic growth and did not alter the marginal status of blacks, we would expect these gains to be negated.

This analysis investigates whether the black gains of the 1960s disappeared in the 1970s. Data concerning educational attainment, employment, occupational prestige, income and earnings will be examined.

TRENDS IN EDUCATIONAL ATTAINMENT

Throughout this century there has been a secular trend toward greater educational attainment. Although this involved both races, the years of schooling completed by blacks have always been fewer than those completed by whites (Folger and Nam, 1967: Table V-6; Hauser and Featherman, 1976: Table 8). Figure 1 indicates the educational attainment of the population age 25 and over. These data, and those analyzed in later sections, were gathered in decennial censuses or in the Census Bureau's monthly Current Population Survey. Wherever possible, data for the white and black population have been used. Prior to 1968, many tabulations were made for nonwhites rather than for blacks and, when necessary, these have been analyzed.

Figure 1 shows the first and third quartiles of educational attainment and a time series of indexes of educational dissimilarity calculated from educational distributions of eight categories. This measure of the overlap of the black and white distributions takes on its minimum value of zero when the distributions are identical. If, on the other hand, all whites had attainments greater than any blacks, the index would equal 100—meaning no overlap of the distributions and maximum dissimilarity.

Racial differences in attainment gradually diminished after the Depression. In 1940, the difference at the first quartile

TRENDS IN RACIAL INEQUALITIES

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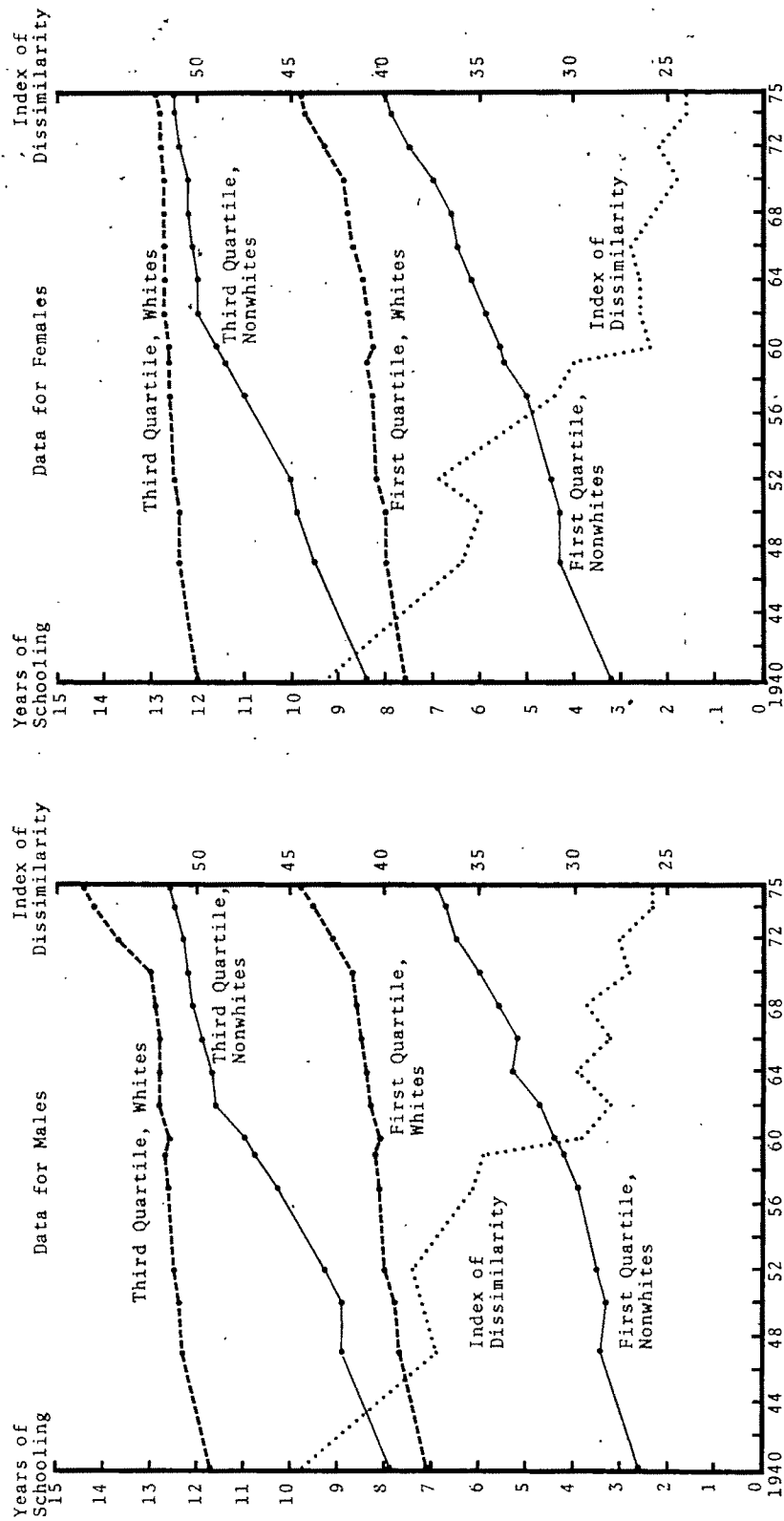


Figure 1. First and Third Quartiles of Educational Attainment and Indexes of Educational Dissimilarity, 1940 to 1975, Population Age 25 and Over

U.S. Bureau of Census, Census of Population: 1950, P-C1, Table 114; Census of Population: 1960, PC(1)-1D, Table 173; Current Population Reports, Series P-20, Nos. 45, 77, 99, 121, 138, 158, 169, 182, 194, 229, 243, 274 and 295.

point was about four and one-half years of schooling but, by the late 1960s, the difference was under three years. At the other extreme, the racial gap at the third quartile point—which was about four years in 1940—decreased to less than one year in the late 1960s for both sexes. The trend toward smaller racial differences in attainment persisted into the 1970s. The indexes of dissimilarity continued their decline and, at present, educational distributions overlap more than at previous dates.

Changes in the attainment of adults occur primarily because older individuals with few years of schooling are replaced by younger people who have greater attainment. Education trends can be studied further by analyzing the attainment of the birth cohorts recently completing schooling. Table 1 indicates attainment at ages 25 to 29 using the same measures as Figure 1.

Over time, racial differences as assessed by the index of dissimilarity declined, and the first quartile points of the black and white educational distributions are closer now than they were in the 1960s and very much closer than thirty years ago. This occurred primarily because of a racial convergence in secondary school completion. Shortly after World War II,

approximately two-thirds of the whites compared to one-third of the nonwhites finished twelve years of education (B. Duncan, 1968: Table 26). Blacks have pretty much "caught up" with whites at this level and, among those in their early twenties today, 85 percent of the whites and 72 percent of the blacks completed high school (U.S. Bureau of the Census, 1976a: Table 1).

The racial gap at the third quartile point has grown larger among both sexes. This occurred because, throughout most the post-World War II span, increases in college enrollment were greater among whites than among blacks. In 1969, 36 percent of white men 18 to 24 were enrolled in college compared to 16 percent of black men—a difference of twenty percentage points which was twice the difference recorded in the Census of 1950 (U.S. Bureau of the Census 1953: Tables 104 and 112; 1970a: Table 1). As a consequence, the racial difference among those completing college widened.

Future trends in educational attainment depend largely upon college enrollment rates. If black enrollment rates approach those of whites, the long-term trend toward educational convergence will continue. Between 1969 and 1975, college enrollment rates of blacks increased more

Table 1. First and Third Quartiles of Educational Attainment for Cohorts at Age 25 to 29 and Indexes of Dissimilarity

Years of Birth of Cohorts	Year Reaching Ages 25-29	First Quartile			Third Quartile			Index of Dissimilarity
		White	Black or Nonwhite	Racial Diff.	White	Black or Nonwhite	Racial Diff.	
Males								
1910-14	1940	8.3	3.7	4.6	12.5	8.8	3.7	48
1920-24	1950	9.0	5.6	3.4	12.8	11.5	1.3	38
1930-34	1960	10.0	7.8	2.2	13.8	12.4	1.4	26
1935-39	1965	11.4	9.4	2.0	14.1	12.6	1.5	23
1940-44	1970	11.9	9.8	2.1	15.2	12.7	2.5	20
1945-49	1975	12.2	11.5	.7	16.0	13.7	2.3	20
Females								
1910-14	1940	8.5	5.2	3.3	12.5	9.9	2.6	43
1920-24	1950	9.6	6.4	3.2	12.6	12.0	.6	33
1930-34	1960	10.5	8.6	1.9	12.8	12.5	.3	24
1935-39	1965	11.5	9.5	2.0	12.8	12.6	.2	23
1940-44	1970	11.9	10.0	1.9	13.7	12.7	1.0	20
1945-49	1975	12.1	11.3	.8	14.9	13.1	1.8	13

Sources: U.S. Bureau of the Census, Census of Population: 1950, P-C1, Table 115; Census of Population: 1960, PC(1)-1D, Table 173; Census of Population: 1970, PC(1)-D1, Table 199; Current Population Reports, Series P-20, No. 158, Table 4; No. 295, Table 1.

than those of whites, suggesting that racial difference in attainment may attenuate (U.S. Bureau of the Census, 1970a: Table 1; 1976b: Table 6). The social and economic processes which brought about a convergence of attainment at the secondary school level in the 1950s and 1960s may have similar effects on college attendance in the 1970s (Hauser and Featherman, 1976:110-6).

TRENDS IN UNEMPLOYMENT AND EMPLOYMENT

If blacks occupy a marginal position in the nation's economy, they would be the last hired and the first fired. In a period of economic contraction such as the 1970s, we would expect unemployment to rise more rapidly among blacks than among whites.

Trends in Unemployment

Unemployment rates are calculated for those who are in the labor force. If a person 16 or over worked for pay or was temporarily absent from a job because of illness, a vacation or a strike, that person was classified by the Census Bureau as employed. The unemployed population includes those persons who did not work but made specific efforts to find jobs, those waiting to be recalled from lay-offs and those awaiting the start of new jobs. Not all persons are in the labor force. Those who have given up the search for work, retirees, homemakers and many students are not members of the labor force (U.S. Bureau of Labor Statistics, 1976:130). Figures in this section refer to whites and nonwhites and exclude military personnel. This exclusion affects rates for young men since, in 1975, seven percent of the male population 16 to 24 were in the armed forces (U.S. Bureau of Labor Statistics, 1976: Table A-3). These data refer to annual averages except for 1976, when data for the first five months were used.

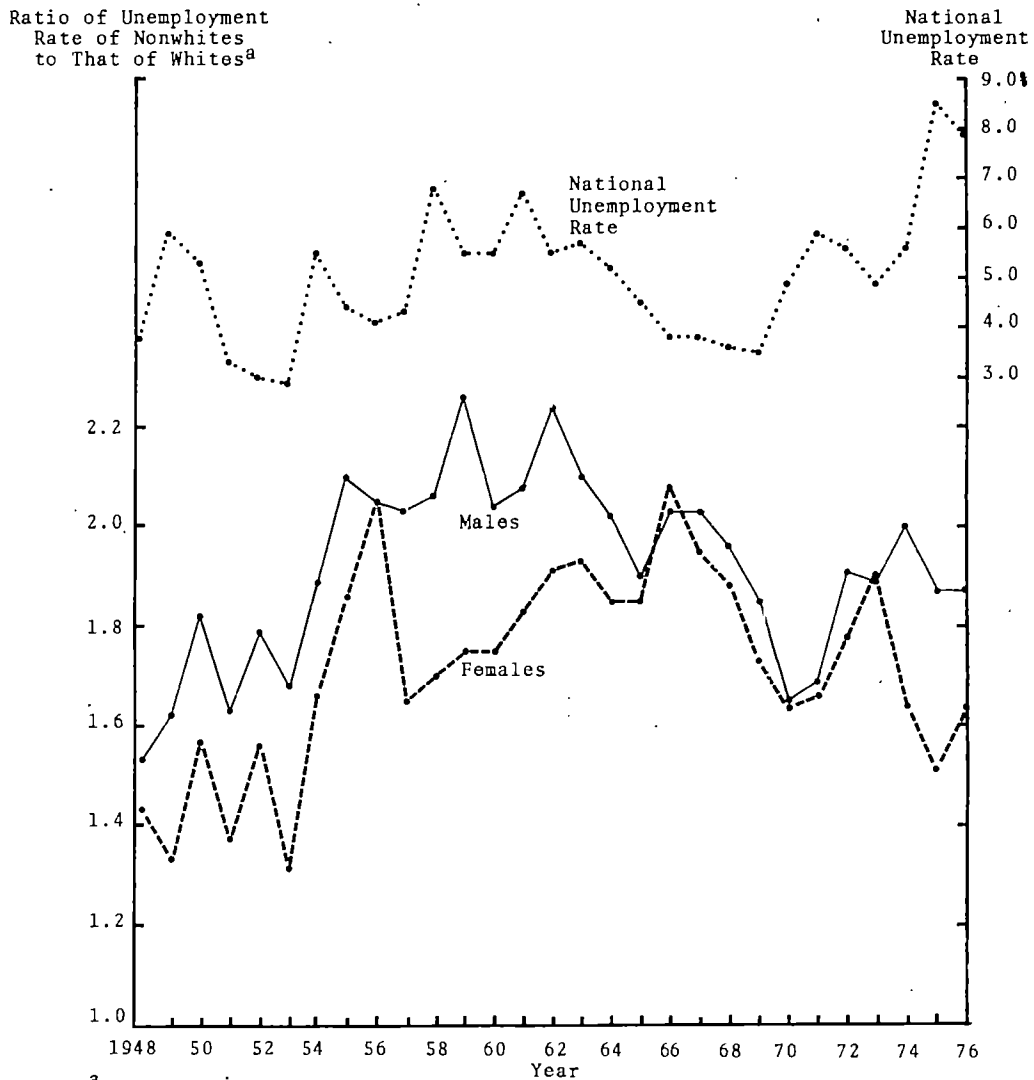
Data concerning unemployment are available from 1948 to the present. The nation's unemployment rates declined during the Korean and Vietnamese wars

but rose in other periods. (See Figure 2 which shows the percentage of the nation's total labor force unemployed for each year.) This rate climbed rapidly in 1974, and in 1975 it was at the highest level since the nation mobilized for World War II (U.S. Bureau of Labor Statistics, 1976: Table 1).

Descriptions of the employment status of nonwhites frequently focus upon high unemployment rates. Throughout much of the post-World War II era, the unemployment rate for nonwhite men has been twice as great as that for white men, as indicated in Figure 2. Since unemployment rates vary by age, these rates were standardized for age. Then the ratio of nonwhite to white unemployment rates was determined and these ratios are plotted in Figure 2.

From the mid-1950s to the late 1960s, the ratio of unemployment rates among men persisted at about two to one. At the end of the 1960s, the economy expanded and the ratio of unemployment rates decreased to the levels observed during the Korean War. During the 1970s, unemployment increased more rapidly among nonwhites than among whites. The proportion unemployed went up from 5 to 13 percent among nonwhite men between 1969 and 1975 while it increased from 3 to 7 percent among whites (U.S. Bureau of Labor Statistics, 1976: Table 1; 1970: Table A-1). The ratio of unemployment rates rose and much of the improvement registered in the late 1960s was negated. Nevertheless, this ratio in the mid-1970s is lower than it was around 1960 when the national unemployment rate was much smaller.

Racial differences in unemployment have been smaller among women than among men. The ratio of nonwhite to white unemployment rates has been less than two to one for most of the post-World War II period and, even during the recession of the 1970s, the rise in unemployment was no greater among nonwhite women than among white women. Apparently, black women have maintained their employment position *vis-à-vis* whites and, on this measure, the gains of the 1960s have not diminished.



^aThese data are standardized for age. See text.

Source: U.S. Department of Labor, Manpower Report of the President, 1975, Tables A-1 and A-20; U.S. Bureau of Labor Statistics, Employment and Earnings, Vol. 22, Nos. 7-12, Tables A-1 and 1.

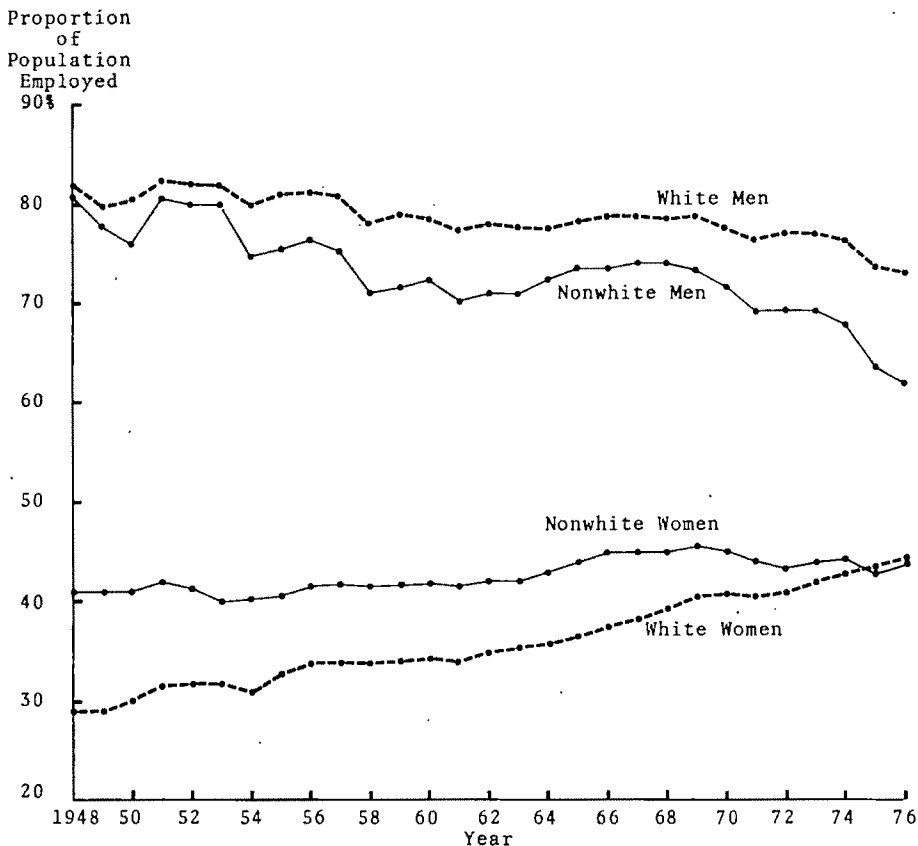
Figure 2. National Unemployment Rate and Ratio of Unemployment Rate of Nonwhites to That of Whites, 1948 to 1976, Population Age 16 and Over

Trends in Employment

Unemployment rates may inaccurately reflect labor force conditions since numerous unemployed individuals may become discouraged, give up the battle to find work and drop out of the labor force. Nonparticipation in the labor force also may indicate the relative position of blacks. To analyze these trends, we studied changes in the proportion of population who held a job, that is, employed persons as a proportion of the total adult

population. If one is interested in the generation of income and the elimination of poverty, it may be more meaningful to consider employment trends rather than unemployment trends (Shiskin, 1976). Figure 3 presents the age-standardized proportions employed for the white and nonwhite civilian labor force. No distinction has been made between workers who held part- or full-time jobs.

The data for men show modest decreases in the proportion of white men at



^aThese data standardized for Age. See text.

Source: U.S. Department of Labor, Manpower Report of the President, 1975, Tables A-4 and A-20; U.S. Bureau of Labor Statistics, Employment and Earnings, Vol. 22, Nos. 7-12, Table 1.

Figure 3. Proportion Employed among the Population Age 16 and Over, 1948 to 1975 *

work. This occurred, in part, because of a sharp reduction in the proportion employed among men over 55—a change undoubtedly brought about by expansion of Social Security benefits and private pension plans. At younger ages there have been small declines in labor force participation rates and substantial year-to-year fluctuations in unemployment.

At all dates, the proportion employed has been lower among nonwhite than among white men and the proportion at work has declined since the early 1950s. Among nonwhites, labor force participation rates have decreased not only among men over 55 but among those at younger ages. For instance, in 1953, about 96 percent of the nonwhite men 25 to 55 were labor force participants, but this figure dropped to 89 percent in 1975 (U.S. De-

partment of Labor, 1975; Table A-4; U.S. Bureau of Labor Statistics, 1976: Table 1). At ages 16 to 24, labor force participation rates once were approximately equal for white and nonwhite men but this too changed, and the proportion of young men in the labor force is now substantially lower among nonwhites than among whites.

In the mid-1970s, the trend toward lower labor force participation combined with high unemployment rates to produce sharp decreases in the proportion of nonwhite men with jobs. As Figure 3 indicates, in 1975 and early 1976 about three-quarters of adult white men, but less than two-thirds of nonwhite men were employed, and racial differences on this indicator are now larger than before.

Decreases in employment adversely af-

fect the income of nonwhite families. However, it is necessary to know more about the activities of men who are not in the labor force. If fewer men work because they attend school or are retired, we would draw different conclusions than if they do not work because jobs are unavailable.

Since 1967, the Bureau of Labor Statistics has tabulated the activities of persons not on the labor force. They are classified as keeping a home, going to school, unable to work or not working for "other reasons." If the lack of economic opportunities accounts for the decreasing proportion of nonwhite men at work, we would anticipate that a growing proportion of nonwhite men would not be in the labor force because of "other reasons," but the data are not consistent with this speculation. In the 1970s, there has not been a sharp increase in the proportion who are not in the labor force because of "other reasons." Rather, there has been a modest rise in the proportion in this category, as well as increases in the proportion unable to work or going to school (U.S. Bureau of Labor Statistics 1976: Table 1; 1970: Table A-1).

Employment trends among men cannot be readily summarized. The pattern of nonwhite unemployment rates being twice those of white rates emerged in the mid-1950s (Killingsworth, 1968:20-2). This ratio temporarily fell below two to one in the later 1960s, but climbed back to about two to one in the present decade. This, however, is only one facet. Since the 1950s, labor force participation has decreased among nonwhite men and, thus, the racial differences in the proportion of men who have jobs has increased. On this indicator, black men appear to fall behind white men in both prosperous and lean times.

Figure 3 also contains information about employment trends among women. The fraction of nonwhite women holding jobs has changed little in the last two decades, and at all dates just over 40 percent were employed. The recession of the mid-1970s reduced employment by a modest amount. A different trend characterizes white women, for the proportion at work has risen about one-half a percent-

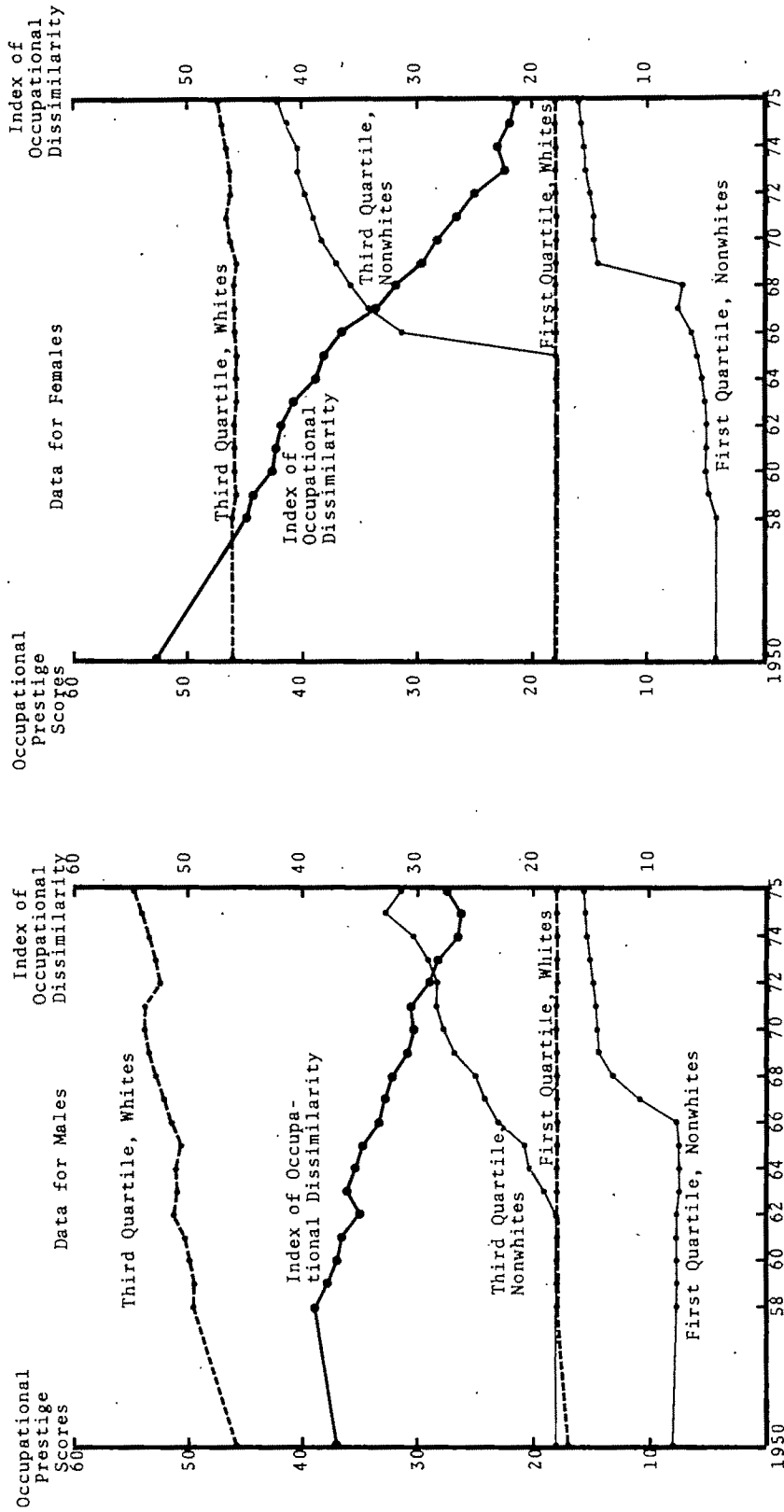
age point each year since the 1950s. Traditionally, a higher proportion of black than white women have worked (Bancroft, 1958:30-3; Bowen and Finegan, 1969:91). This difference has disappeared and, in 1975, the proportion at work was slightly higher for white than for nonwhite women.

OCCUPATIONAL CHANGE

Since the 1950s there has been a modest upgrading of the occupations pursued by white workers, but among nonwhites there has been a more dramatic shift into higher status jobs and thus racial differences in occupational prestige have declined.

Figure 4 shows trends in the occupations of employed workers. The decennial censuses and monthly labor force reports classify workers into eleven major categories. Persons in each category have been assigned a prestige score using the system developed by O. D. Duncan (1961, ch. 6; Hauser et al., 1975: Table 1). At the lower end, laborers were assigned a score of 7 while professionals, at the other extreme, were scored 75. Figure 4 shows the first and third quartiles of the occupation prestige distribution. To assess the similarity of the white and nonwhite distributions, indexes of occupational dissimilarity also are presented.

Between 1950 and 1970, the first and third quartiles of the white distribution changed very little, but among nonwhites the first and third quartiles rose. Particularly sharp jumps at the first quartile point occurred in the 1960s as nonwhites moved out of low prestige jobs as farm laborers or factory workers. As a result, the occupational distributions of white and nonwhite men became increasingly alike (Hauser and Featherman, 1974a:259-61). The dissimilarity index for men fell from 43 in 1940 (not shown) to 37 in 1960 (see Figure 4) to 31 at the end of that decade and to 26 in 1975. The relative improvements in the occupational status of nonwhite women were even greater than those for nonwhite men. In 1940, three-quarters of employed black women worked as domestic servants or as farm laborers (Bancroft, 1958:86). This



U.S., Bureau of the Census, Census of Population: 1950, P-C1, Table 128: Current Population Reports, Series P-57, No. 190, Table 16. U.S., Bureau of Labor Statistics, Employment and Earnings: Vol. 6 through 22, Occupational Distribution by Sex and Color, Annual Average.

Figure 4. First and Third Quartiles of Occupational Prestige and Indexes of Occupational Dissimilarity, 1950 to 1976

changed as black women moved into service jobs, clerical and sales positions. By 1975, only 10 percent worked as domestic servants or on farms (U.S. Bureau of Labor Statistics, 1976: Table 19).

The occupations of nonwhite workers were upgraded during the 1960s and 1970s. We examined annual changes in the numbers of whites and nonwhites employed in each of the major occupational categories and found that the number of nonwhites working at non-manual jobs or as craftsmen grew much more rapidly than the number of white workers in those occupations. Presumably, barriers to black employment in these prestigious jobs fell during the 1960s and 1970s. (For a discussion of crafts occupations, see Snyder and Hudis, 1976:230-1.) Two of the white-collar occupations—managers and sales—involve supervising other employees or meeting the public as a representative of the employer—jobs which were once reserved to whites. Gains in nonwhite employment in these classifications during the 1970s appear to exceed the gains of the 1960s (Garfinkle, 1975).

Throughout the period after the Depression, the occupational distribution of nonwhites improved as nonwhites moved away from jobs on farms or in domestic

service. Substantial gains were registered by blacks after 1960 and were not negated in the 1970s. Nevertheless, these changes have not eliminated the very large gap between the occupations pursued by whites and by nonwhites. Despite three and one-half decades of improvements, the average prestige score for nonwhite workers in 1975 was inferior to that of white workers in 1940 (U.S. Bureau of the Census, 1943b: Table 62; U.S. Bureau of Labor Statistics, 1976: Table 19).

FAMILY INCOME TRENDS

The most widely used statistics concerning the economic status of blacks are those showing median family incomes. The Census Bureau has tabulated such data since 1947; a summary is presented in Table 2.

Shortly after World War II, the median income of whites was approximately double that of nonwhites (see Table 2). Gains were made by blacks, especially in the late 1960s, and an income peak was reached around 1970 when black families had a median income 64 percent as great as whites (Wohlstetter and Coleman, 1972:19-29; Horowitz, 1974: Table 10-1; Thurow, 1969: Figure 2-2).

Ratios of medians offer one assessment,

Table 2. Trends in the Income of White and Black Families: 1947 to 1975

Year	Median Family Income in Constant Dollars ^a		Black Median Income as Percent of White	Index of Dissimilarity ^b
	Blacks	Whites		
1947	\$3,888	\$7,608	51%	38.3
1950	4,178	7,702	54	36.8
1955	5,113	9,271	55	34.7
1960	5,871	10,604	55	31.7
1965	6,812	12,370	55	32.8
1967	7,859	13,273	59	31.0
1968	8,292	13,826	60	29.1
1969	8,807	14,379	61	28.9
1970	8,703	14,188	61	27.3
1971	8,558	14,182	60	28.2
1972	8,831	14,858	59	28.0
1973	8,804	15,254	58	29.3
1974 ^c	8,737	14,633	60	27.8
1975	8,779	14,268	62	26.3

^a Amounts shown in constant 1975 dollars. Data for 1947 to 1965 refer to whites and nonwhites.

^b Indexes of dissimilarity computed from seven-category constant dollar income distributions.

^c Revised 1974 income figures. For details see Current Population Survey, Series P-60, No. 103.

Sources: U.S. Bureau of the Census, Current Population Reports, Series P-60 No. 103, Table 4.

but a more complete view may be obtained by measuring the overlap of the two income distributions. Indexes of income dissimilarity are shown on the right in Table 2. Between 1947 and 1960 this index declined from 38 to 32, indicating a growing overlap. During the 1960s, further gains were made by nonwhites and, at the end of that decade, the income distributions of the races overlapped more than at previous dates.

Family income trends in the most recent six-year span differ greatly from those of the preceding period. Rather than growing, family income, as measured in constant dollars, stagnated in the 1970s and, in 1975, the median incomes of families of both races were slightly lower than they were in 1969. The trend toward higher ratios of black to white medians ceased and the indexes of dissimilarity which measure the overlap of the income distributions of black and white families declined more slowly in the 1970s than in the 1960s. If family income is the criterion of status, there is no evidence that the pattern of improvement which was seen in the 1960s continued into the 1970s. On the other hand, blacks have not fallen further behind whites.

Changes in Family Composition

Trends in family income are influenced by changes in living arrangements of children and parents. Since 1960, four major shifts in American family structure are evident. First, a growing proportion of marriages end in divorce. The divorce rate—as indexed by the ratio of divorces to married women—began to increase in the 1950s (Glick and Norton, 1973: Table 1) and continues to rise (U.S. National Center for Health Statistics, 1976a: Table 2; Preston, 1975). Approximately one out of five first marriages of women who reached marriagable age in the 1940s ended in divorce. If present rates persist, about one in three of the first marriages of women who reached the same ages in the 1960s will be terminated by divorce (Glick and Norton, 1973: Table 2; U.S. Bureau of the Census, 1972: Table F).

Second, an increasing proportion of women are heads of families. In 1960,

about five percent of white and 15 percent of nonwhite women ages 25 to 44 were family heads. By 1975, these proportions grew to nine percent among white women and 32 percent among black women (U.S. Bureau of the Census, 1964: Table 2; 1975c: Table 2).

Third, the percentage of births out of wedlock had risen sharply. The proportion of births which were illegitimate was two percent among whites in 1960 and 22 percent for nonwhites (U.S. National Center for Health Statistics, 1969: Table 1-24). The fertility rates of married women have fallen more rapidly than those of single women (Cutright and Galle, 1973; Glick and Mills, 1974:6) and the proportion of births illegitimate in 1974 was seven percent among whites and 47 percent among blacks (U.S. National Center for Health Statistics, 1976b: Table 11).

Finally, the proportion of children who live with both parents—real or adoptive—has declined. In 1960, 90 percent of white children under 18 and 66 percent of nonwhite children lived with both parents. Fifteen years later, these proportions fell to 85 percent for whites and 49 percent for blacks (U.S. Bureau of the Census, 1964: Tables 1, 2, 19; 1975c: Table 4).

Families headed by women traditionally have had incomes about one-half as great as families headed by men, and children in families headed by a woman are much more likely to be impoverished than children in families headed by a man (U.S. Bureau of the Census, 1963a: Table 224; 1975b: Table 2; 1976c: Table 17). If we analyzed trends in family income or welfare without controlling for changes in living arrangements, we would underestimate the improvements which may have occurred.

Table 3 indicates the income of families by type in 1959, in 1969—when black family income peaked—and in 1975. Figures are shown for total families, for families which include both spouses and for families headed by women. At each date about three percent of the nation's families were headed by a man who did not have a wife present, and data are not shown for these families. Table 3 presents

Table 3. Median Incomes (in Constant 1975 Dollars) of White and Black Families by Type and Average Rates of Change: 1959, 1969 and 1975

	Median Income			Average Annual Rate of Change	
	1959 ^a	1969	1975	1959 to 1969	1969 to 1974
Total Families					
Black	\$ 5,837	\$ 8,807	\$ 8,779	+4.2%	-0.1%
White	10,885	14,379	14,268	+2.8	-0.1
Dollar Difference	-5,048	-5,572	-5,489		
Ratio of Medians	.54	.61	.62		
Husband-Wife Families					
Black	7,314	10,744	11,526	+4.7	+1.2
White	11,247	15,014	15,125	+2.9	+0.1
Dollar Difference	-4,537	-4,270	-3,599		
Ratio of Medians	.60	.72	.76		
Female-Headed Families					
Black	3,201	4,897	4,898	+4.3	0.0
White	6,535	8,063	7,651	+2.1	-0.9
Dollar Difference	-3,333	-3,165	-2,753		
Ratio of Medians	.49	.61	.64		

^a Data for 1959 refer to whites and nonwhites.

Sources: U.S. Bureau of the Census, Census of Population: 1960, PC(1)-1D, Table 224; Current Population Reports, Series P-60, No. 75, Table 17; No. 103, Tables 1, 3 and 4.

median income by race, ratios of median incomes, annual rates of change of the medians and the racial gap in purchasing power measured by the difference in medians.

During the 1960s, black families of both types—husband-wife and female-headed—experienced greater gains in income than comparable white families. The 1969 to 1975 interval was much less prosperous than the preceding decade, and the growth rate of the median income of families was actually negative. Nevertheless, the median income of husband-wife black families rose and the rate of increase was greater than for similar white families, that is, 1.2 percent annually for husband-wife blacks versus 0.1 percent for whites. The median income of families headed by black women did not change but that of similar white families fell by 0.9 percent annually.

There appears to be a puzzle. The median income of total black families declined in this span and the racial difference in purchasing power remained essentially constant at about \$5,500. However, the median income of the most common types of black families—husband-wife and female-headed—rose or remained un-

changed and, for families of these types, the racial difference in purchasing power decreased substantially. This is explained by the shifting distributions of families by type. Female-headed families—the lowest income families—became a larger component of total black families, increasing from 28 percent in 1969 to 36 percent in 1975 while husband-wife families fell from 68 to 60 percent (U.S. Bureau of the Census, 1970b: Table 17; 1976c: Table 1). This suggests that the analysis of family income trends must take into account the changing living arrangements of children and adults.

Trends in Personal Earnings

Since changes in family composition confound trends in family income, it is important to examine the earnings of individuals. If blacks improved in economic status in the 1960s and maintained these gains into the 1970s, we would expect that the earnings of blacks would rise faster than those of whites and that blacks and whites would begin to receive similar pay for such characteristics as educational attainment and occupational achievement. In this section we analyze, first, whether

the earnings of blacks rose faster than those of whites and, second, whether blacks and whites receive similar pay for ostensibly similar characteristics.

Data describing the earnings of individuals are obtained in the decennial censuses, and tape files of the one-in-one-thousand samples of the censuses of 1960 and 1970 were used. Every March the Census Bureau's Current Population Survey gathers information about earnings from the occupants of 50,000 households. Tape files of the March survey may be used to analyze the earnings process on an annual basis. Data from the March, 1975, survey were studied in this investigation.

The earnings an individual obtains are influenced by his or her education and occupation. Additionally, wage levels traditionally have been lower in the South than elsewhere and, other things being equal, we anticipate that earnings are positively related to the number of hours worked. Finally, experience in the labor market should affect earnings independent of other factors. The model used in this paper specifies that an individual's earnings are a linear function of region of residence, education, occupation, time worked and experience.

To ascertain parameters for this model, each individual in the 1960, 1970 and 1975 samples was classified by sex and race. Each received a score for region—one for the South and zero for other regions. The education score equaled the number of school years completed. The occupational prestige scores were based upon the detailed—not the broad—occupational categories (Featherman et al., 1975; O. D. Duncan, 1961). An estimate of hours worked was derived from weeks worked in the year for which earnings were reported and the hours worked during the week prior to the census or survey. This variable is identified as Time in the tables. Experience was estimated by taking the respondent's current age, subtracting years of schooling and the constant, six. For most men, this represents years of labor force experience. For many currently employed women, however, this is an imperfect indicator of work experience since they may have been out of the labor force for long periods. Human capital may

deteriorate with age and, to represent this effect, the experience variable was squared. This is identified as Decay in the tables (see Mincer, 1974:ch. 5). Earnings, the dependent variable, refers to the year prior to the census or survey, that is, to earnings obtained in 1959, 1969 or 1974.

This analysis is restricted to black and white persons 25 to 54 who reported earnings and who indicated the number of hours they worked. Thus, it excludes the long-term unemployed, those who took their first jobs during the year of the census or survey, and those who worked in 1959, 1969 or 1974 but were not working at the time of the census or survey.

A variety of models have described racial differences in earnings and the returns to human capital (Masters, 1975:ch. 5; Hauser and Featherman, 1974b; Cherlin and Hodge, 1973; Thurow, 1969:76–83; O. D. Duncan, 1969; Siegel, 1965). Because census data are analyzed, the model presented in this paper omits several variables which may influence earnings such as intelligence (Griliches and Mason, 1973; Duncan et al., 1972:88–102), religion (Greeley, 1974:73–87; Gockel, 1969; Goldstein, 1969; Laumann, 1969: Table 4), timing of labor force experience (Mincer and Polachek, 1973) or characteristics of the family of origin (Featherman and Hauser, 1976: Table 8). If these or other relevant variables were included, they might provide additional information about racial discrimination.

Means and standard deviations of the variables are shown in Table 4. Figures in the first row reveal that the earnings of blacks have risen faster than those of whites. Between 1959 and 1969, the mean earnings of white men rose 27 percent; those of black men rose 51 percent. Among women, the increases were 24 percent for whites and 69 percent for blacks. From 1969 to 1974, the earnings of whites—in constant dollars—actually declined but blacks reported gains. Data in Table 4 refer to a select subgroup of the population. However, when all persons who reported earnings or all persons with income are considered, a similar conclusion is reached. The earnings of black men and women rose faster than those of whites during the early 1970s. On this

Table 4. Means and Standard Deviations of Variables Used in the Analysis of Earnings: Employed Persons 25 to 54

Variables	Males						Females					
	1959		1969		1974		1959		1969		1974	
	White	Black	White	Black	White	Black	White	Black	White	Black	White	Black
Earnings*												
\bar{X}	\$10,638	5,473	13,522	8,260	13,432	9,137	4,762	2,934	5,901	4,995	5,760	5,652
σ	\$10,375	2,303	9,410	5,541	9,630	4,995	3,531	2,440	4,459	3,625	4,153	4,181
Region												
\bar{X}	.26	.57	.28	.50	.29	.49	.27	.57	.29	.52	.31	.53
σ	.44	.49	.45	.50	.46	.50	.44	.50	.45	.50	.46	.50
Education												
\bar{X}	11.2	8.5	12.1	10.1	12.9	11.2	11.4	9.5	12.0	10.8	12.6	11.9
σ	3.3	3.8	3.3	3.5	3.1	3.3	2.8	3.6	2.7	3.2	2.7	2.8
Occupation												
\bar{X}	37.2	18.9	42.3	24.9	44.3	27.7	38.1	20.1	42.6	28.6	44.8	33.3
σ	22.6	15.4	24.0	18.8	24.2	20.4	19.6	18.1	20.9	22.1	21.1	22.8
Time												
\bar{X}	2,145	1,852	2,157	1,950	2,184	1,910	1,614	1,453	1,585	1,620	1,617	1,651
σ	579	654	526	533	695	621	709	752	684	621	734	641
Experience												
\bar{X}	22	24	21	23	20	21	23	24	22	22	20	20
σ	10	10	10	10	10	10	9	10	10	10	10	10
Decay												
\bar{X}	577	692	538	608	485	538	606	648	577	577	507	495
σ	437	507	425	485	414	478	415	478	417	455	412	418

* Shown in 1974 dollars.

Sources: U.S. Bureau of the Census: Censuses of Population and Housing, 1960 and 1970 tape files of one in 1,000 samples; Current Population Survey, March, 1975, tape file.

measure, the gains of the 1960s were not negated and the racial difference in earnings declined in the 1970s (U.S. Bureau of the Census, 1970b: Tables 45 and 54; 1976c: Tables 11 and 12).

Table 4 indicates that a decreasing fraction of blacks live in the South and that the educational attainment of black workers has risen more than that of whites. Occupational upgrading has been somewhat greater for blacks than for whites (for trends among men, see Hauser and Featherman, 1974a). Among men, there is a persistent racial difference in the number of hours worked per year. The racial difference in years of experience has been small at all dates.

Next, we considered whether the earnings blacks and whites receive, as determined by their characteristics, have changed over time. The upper panel of Table 5 shows the regression models for males and the lower panel refers to females. The first line of the upper panel indicates that, in 1959, residing in the South had the net effect of reducing a white man's earnings by \$1,002. Each year of schooling was associated with a net increase in earnings of \$676, each occupational prestige point was worth \$95 and every additional hour worked netted a white man \$1.93. The returns for an additional year of experience were \$603. These returns to characteristics are expressed in constant 1974 dollars.

The regression coefficients show that black men are paid less for their attributes than are whites. At all dates, the earnings penalty associated with living in the South was about twice as great for blacks as for whites. For both races, returns to schooling increased in the 1960s and fell back in the 1970s, but a large racial difference was maintained. In 1974, an additional year of schooling—independent of other variables—was worth about \$600 to a white but only \$200 to a black. The returns to occupational prestige have not changed greatly and, at all dates, an increment of one point on the occupational prestige scale was worth about half as much for blacks as for whites. Apparently, the racial difference in returns to hours worked has been mitigated. The earnings returns for labor force experience are

much greater for whites than for blacks and there is no indication of a diminution in the fifteen-year span covered by these data. These models suggest that black men are paid less than whites for ostensibly identical characteristics and that there have been few changes in this pattern.

Among women, there have been alterations in the rates of return and the earnings of black women are approaching those of white women. In 1959, black women benefited considerably less than white women for each year of education, but the difference attenuated and there is now almost no racial difference in returns to education among women. By 1974, the increment in earnings associated with occupational prestige was also similar for whites and blacks. The racial difference in returns for hours worked also has diminished.

For white women, the earnings returns associated with experience, as measured in this paper, were essentially nil. That is, each of the regression coefficients was smaller than its standard error. Among black women, earnings were related to experience and, over time, the returns for experience increased sharply. Black women have a different pattern of labor force participation by age than white women, and it may be that black women typically have greater on-the-job seniority than do white women of comparable age (Mincer and Polachek, 1973: Table 2; Sweet, 1973: Table 1-9; Bancroft, 1958: Table 18). This may account for the advantages black women obtain on this determinant of earnings.

The figures in Table 4 reveal that blacks earn less than whites. One reason is that blacks have different characteristics than whites. For example, more blacks than whites live in the South where wages are low. Blacks also complete fewer years of schooling than whites and work at less prestigious jobs. Another reason is that blacks generally are paid less for a given characteristic than are whites. From both analytic and policy viewpoints, we would like to know whether the racial differences in earnings result primarily from racial differences in characteristics or from racial differences in the earnings returns

calculated

Table 5. Determinants of Earnings for Males and Females 25 to 54 by Race, Constant 1974 Dollars: 1959, 1969 and 1974

		Independent Variables								Error of Estimate	N
Data for Males	Region	Educ.	Occ.	Time	Exper.	Decay	a	R ²			
White Males		Metric Coefficients*									
1959	\$-1,002.06 (135.79)	675.70 (25.15)	94.61 (3.25)	1.93 (.10)	602.54 (28.70)	-10.22 (.62)	-11,713	.154	9,541	26,066	
1969	-1,438.70 (112.64)	699.74 (21.63)	103.32 (2.64)	2.62 (.09)	622.38 (23.25)	-10.79 (.53)	-11,855	.235	8,232	26,684	
1974	-862.44 (146.68)	586.52 (30.85)	94.96 (3.52)	1.76 (.10)	702.92 (29.97)	-12.79 (.71)	-9,535	.188	8,680	17,008	
Black Males											
1959	-2,095.53 (142.39)	195.98 (25.20)	51.54 (5.01)	1.16 (.11)	155.07 (34.45)	-2.58 (.67)	-100	.250	3,415	2,446	
1969	-2,370.78 (202.33)	285.00 (38.78)	59.37 (5.99)	1.86 (.19)	183.65 (45.71)	-3.00 (.96)	-838	.218	4,709	2,506	
1974	-1,999.55 (235.52)	203.38 (52.44)	43.01 (6.86)	2.12 (.19)	188.16 (48.98)	-3.56 (1.09)	631	.247	4,332	1,459	
White Males		Standardized Coefficients									
1959	-.043	.217	.206	.108	.552	-.429					
1969	-.069	.243	.263	.146	.643	-.487					
1974	-.041	.188	.239	.126	.721	-.550					
Black Males											
1959	-.266	.190	.204	.195	.398	-.336					
1969	-.214	.181	.201	.179	.334	-.263					
1974	-.200	.134	.175	.264	.394	-.341					
Data for Females		Metric Coefficients*									
White Females											
1959	\$ -603.71 (66.24)	229.86 (13.87)	28.02 (1.83)	1.97 (.04)	11.80 (15.29)	.19 (.34)	-2,330	.253	3,053	10,863	
1969	-575.08 (70.77)	317.04 (15.32)	39.80 (1.84)	2.86 (.05)	-14.90 (15.31)	.84 (.35)	-4,133	.302	3,726	13,615	
1974	-599.37 (74.15)	279.20 (17.24)	40.84 (1.95)	2.57 (.05)	.92 (15.61)	.48 (.37)	-3,832	.358	3,329	9,607	
Black Females											
1959	-1,109.01 (91.64)	97.73 (18.45)	54.74 (3.14)	1.00 (.06)	35.65 (22.13)	-.52 (.44)	-388	.462	1,790	1,613	
1969	-1,046.32 (128.97)	248.26 (27.88)	52.03 (3.54)	1.76 (.10)	49.82 (30.75)	-.74 (.67)	-2,156	.378	2,859	2,033	
1974	-929.78 (197.19)	277.73 (48.96)	44.98 (5.76)	1.94 (.16)	115.91 (45.13)	-2.12 (1.04)	-3,134	.312	3,458	1,293	
White Females		Standardized Coefficients									
1959	-.076	.185	.156	.395	.030	.022					
1969	-.059	.192	.186	.439	-.032	.078					
1974	-.066	.173	.207	.455	.002	.048					
Black Females											
1959	-.225	.143	.405	.307	.143	-.102					
1969	-.144	.219	.317	.301	.133	-.093					
1974	-.111	.185	.245	.298	.268	-.212					

* Standard errors in parentheses.

Sources: See Table 4.

which are associated with the characteristics we have considered.

The racial difference in earnings may be decomposed in many different fashions

(Althausen and Wigler, 1972:106-18). One method is illustrated in Table 6. In the equation which regressed the earnings of blacks upon the characteristics of blacks,

it was assumed that blacks had the average characteristics of whites rather than their own characteristics. What effect this change would have upon the average earnings of blacks was ascertained. In 1959, if black men had the regional distribution of whites but retained their own returns for region, their average earnings would have been \$650 greater. If they had the educational attainment of whites but retained their own rates of return, their average earnings would have risen \$542. This decomposition method differs from that used by several other investigators (Hauser and Featherman, 1974b:329; O. D. Duncan, 1969:97-103; Siegel, 1965).

Racial differences in characteristics do not account for the entire racial difference in earnings. There is a residual, reflecting the difference in the returns blacks and whites receive for their characteristics. In Table 6, this is labeled "the difference not associated with the independent variables." This component has been labeled the "cost of being a Negro" (Siegel, 1965) or the "cost of discrimination" (Hauser and Featherman, 1974b:329; O. D. Duncan, 1969:100). We note that this decomposition is not unique, and that different models of the earnings process might give different estimates of discrimination.

The top rows of Table 6 indicate that the racial difference in the earnings of men changed very little during the 1960s but decreased by about \$1,000 in the early 1970s. At each date, racial differences in characteristics accounted for a substantial fraction of the difference in earnings. If black men had the characteristics of whites in 1959 and 1969, they would have earned an average of \$2,500 more and, in 1974, an additional \$2,000. Nevertheless, this did not account for the entire racial difference. A black man with the average characteristics of a white man and paid at the return rates blacks received for their characteristics would have earned much less than the typical white man because of racial differences in pay rates. In 1959 and 1969, this difference, which reflects discrimination, was \$2,800 and, in 1974, \$2,300. At each date, about 53 percent of the racial difference in earnings was attributable to discrimination as estimated by this model.

Table 6 provides similar data for women. The racial difference in earnings declined from \$1,800 in 1959 to \$100 in 1974. The decomposition reveals that, in 1959, racial differences in characteristics did not account for all the difference in earnings. However, by 1959, a black woman

Table 6. Decomposition of Racial Difference in Earnings in Constant 1974 Dollars: 1959, 1969 and 1974

	Males			Females		
	1959	1969	1974	1959	1969	1974
Earnings of Whites	\$10,638	13,522	13,432	\$4,762	5,901	5,760
Earnings of Blacks	5,473	8,260	9,137	2,954	4,995	5,652
Racial Difference	+5,165	+5,262	+4,295	+1,808	+906	+108
Components of Earnings Difference Associated with Racial Difference in Independent Variables: ^a						
Region	+650	+512	+401	+331	+243	+212
Education	+542	+574	+329	+191	+294	+202
Occupation	+941	+1,034	+715	+989	+725	+520
Time	+340	+383	+584	+160	-61	-65
Experience ^b	-57	-55	-5	0	+4	-1
Total	+2,416	+2,448	+2,024	+1,671	+1,205	+868
Component of Earnings Difference Not Associated with Racial Differences in Independent Variables						
	+2,749	+2,814	+2,271	+137	-299	-760

^a Change in earnings which would occur if blacks retained their own rates of return but had the characteristics of whites.

^b Includes the effects of the Experience and Decay variables.

Source: Table 3.

who had the average characteristics of a white woman and was paid at the return rates of blacks would have earned more than the typical white woman. By 1974, this advantage for black women increased to \$760. This does not necessarily indicate that employers prefer black women and are paying them more than comparable white women. Rather, as the coefficients in Table 5 show, the returns to experience are considerably greater for black than for white women.

CONCLUSION

During the prosperous 1960s, racial differences in education, occupation and income generally declined. We investigated whether this trend continued into the 1970s and concluded that the gains of the 1960s apparently were not solely attributable to the prosperity of that decade, since racial differences in status narrowed in the 1970s as they did in the previous decade. Blacks and whites, especially the young, are more alike in years of school completed than ever before. Racial differences in the occupations of employed workers continue to decline. The income gap separating black and white families has remained constant, but this is largely a consequence of the sharp rise of female-headed families among blacks. Indexes describing the income of specific types of families or the earnings of individuals generally reveal that racial differences moderated during the early years of the 1970s.

In some areas, the gains are impressive. Black women, for instance, obtain earnings comparable to those of white women with similar characteristics. However, not all indicators show improvement. Employment opportunities are apparently severely limited for many black men. The very high rates of unemployment and nonparticipation in the labor force suggest that numerous young blacks experience great difficulty in launching careers.

The four processes delineated at the outset—the urbanization of blacks, the growing demand by blacks for civil rights, more liberal court rulings and laws, and reductions in the prejudicial attitudes of whites—have provided blacks with

greater opportunities to compete for economic rewards. The occupational upgrading of blacks and their growing representation in politics (U.S. Bureau of the Census, 1975a: Table 99) imply that they are more represented in American decision making than they were at the start of World War II. These changes mean that, even during a pervasive recession, blacks did not lose the gains they previously experienced.

On the other hand, reductions in inequality are small when compared to the remaining racial differences on many indicators. A continuation of the trends of the 1960s and 1970s offers no hope that racial differences will be eliminated soon. For instance, a higher proportion of white men in 1940 than black men in 1976 held white-collar jobs (U.S. Bureau of the Census, 1943a: Table 64; U.S. Bureau of Labor Statistics, 1976: Table A-21). The purchasing power of the typical black family in 1974 was equivalent to that of a white family twenty years earlier (Table 3), and the earnings of black men lag far behind those of white men (Table 4).

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LEGITIMATING MYTHS AND EDUCATIONAL ORGANIZATION: THE RELATIONSHIP BETWEEN ORGANIZATIONAL IDEOLOGY AND FORMAL STRUCTURE*

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The purpose of this paper is to investigate the relationship between ideology and formal organizational structure in American higher education. To do so, we develop an argument predicated on the idea that colleges and universities affect individuals and the society by: (1) creating membership categories, e.g., "college graduate"; (2) legitimating the social rights and meanings attached to these groups; (3) ritually certifying individuals as members. These processes occur independently of any impact schools have on students. As a result, colleges must create and validate myths concerning the intrinsic qualities graduates possess that derive from the college experience. Organizational structures dramatize the symbolic redefinition of students and legitimate the idea that important changes actually have occurred during college attendance. The effects of given organizational arrangements are established by accepted educational theories and conventional wisdom. It is argued that organizational structure must conform to schools' self-presentation and their advertised effects on students. Schools whose structure and ideology do not match may be under pressure to change or suffer the fate of nonconformity. On the basis of these ideas, we develop a typology of symbolic definitions colleges construct about graduates and discuss how particular organizational features are used to legitimate different conceptions of "student." Data from comparative organizational studies are used to illustrate these arguments and to indicate substantive conditions that limit the applicability of this argument.

Conventional thinking and research on schools has focused almost entirely on the

effects of organizational arrangements on student attitudes and behavior. This emphasis is misdirected for several reasons: (1) because most research has shown that school organization has rather small socialization effects on students' attitudes, abilities and status choices, etc.

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(see for example, Feldman and Newcomb, 1969) and (2) because schools affect graduates through several other processes which have been largely overlooked due to the exclusive attention given to locating socialization effects of different organizational conditions. Schools affect individuals and society in the following ways, independently of any impacts they have on individuals. First, they are used to construct categories of membership in society to which important job and other rights and social meanings are attached (see Meyer, 1970b; Ramirez, 1973). Second, schools legitimate these rights by institutionalizing ideas about the qualities and abilities of graduates versus nongraduates. Third, schools are the agencies that ritually certify graduates as members of social status groups to whose privileges they are entitled (see Collins, 1971). All of these processes operate independently of any direct socialization effects schools have on students.

Using these ideas first proposed by Meyer (1970b), we explore their implications for a theory of educational organization and develop a set of propositions to explain the organizational diversity in American higher education. Our argument rests on two ideas about schools and school organization: (1) a major effect of schools and colleges is to symbolically redefine graduates as possessing special qualities and skills gained through attendance. This redefinition occurs independently of whether or not any actual changes in competency, etc. have occurred. (2) The legitimacy of this status transformation must be negotiated with important audiences in society, e.g., employers, and this necessitates the development of legitimating myths about the quality of students' educational experience. These ideas are validated by the social organization of schools. Organizational structure is, therefore, important in legitimating the idea that initiatives have been changed in specified directions by the school experience, independent of any actual changes. Since graduates have to be accepted as changed persons *by definition*, the symbolic construction of credible rituals of status change is of paramount importance (see Garfinkle, 1956).

Schools, especially in higher education, differ with respect to the symbolic conceptions of graduates that they attempt to institutionalize among clients. This is a major source of organizational diversity and change. In this view, organizational structure and conceptions of "student" that schools try to maintain among their publics are causally related features of their social organization. It appears that the causal relationship between these features of organization is reciprocal: changes in conceptions of "student" will produce changes in organizational structure and, similarly, externally induced changes in organizational structure will necessitate a shift in organizational self images.

Colleges, for example, that claim to be developing specialized occupational skills among their students must have a highly differentiated curriculum to point to as evidence of their ability to transmit vocationally relevant skills. Ordinarily, such programs will be accepted by employers and others as adequate verification that graduates actually have learned vocational skills.

Similarly, schools that develop such ideas about their function and their students will be under social pressure to develop curriculum structures and other features that validate these conceptions of "student." On the other hand, structural changes, such as a decline in recruitment selectivity or an increase in size, will create pressures for the adoption of new organizational ideologies concerning the *social meaning* of university attendance.

In this paper, we will emphasize the effects of symbolic structure on social organization, realizing that this relationship is nonrecursive.

Types of Myths

It is important now to consider the kinds of legitimating myths colleges attempt to construct. To do this, we draw together two different theoretical traditions: (1) the work of Turner (1961), Collins (1971) and Neo-Marxist writers on the role of education in elite formation (see Bowles, 1972; Gintis, 1971) and (2) the largely anthropological tradition of work

on rituals and initiation ceremonies that emphasizes the dramaturgical function of rites in "creating" membership in *corporate* (descent) groups (cf. Cohen, 1964; Young, 1964; Ramirez, 1973). These lines of argument lead to the development of a typology of legitimating myths that colleges attempt to institutionalize among their constituencies.

Writers who focus on the function of schooling in maintaining and legitimating the distribution of power and privilege tend to see the major distinction between schools and colleges as that between elite and mass-producing institutions. In higher education, the difference is between those colleges which are educating societal elites and those that are turning out the large, highly differentiated middle-class segment of the population. It is important to note that these arguments are not all of a piece or in agreement with one another. What is common to them is the focus on the stratification function of schooling as primary. The first dimension of our typology concerns the membership descriptions of graduates that colleges attempt to construct *vis à vis* the wider societal occupational order. The distinction here is between schools who define their students as societal elites and those who do *not* describe them as elites.

The anthropological tradition has emphasized a function of primitive rituals and initiation ceremonies assumed by modern educational systems. Rites of passage serve to induct people into membership status in society and, in the process of creating members by symbolically redefining them, they also reaffirm the social reality of society and its central values. This Durkheimian theme is central to the argument we develop, for it emphasizes that the major significance of rituals is to dramatize and validate the assumption of membership in a corporate group. What the initiates learn or feel during, and as a result of their ritual experience is of secondary importance.

Recently, anthropologists have expanded on this theme and argued that extensive rituals of puberty only occur in societies where highly developed corporate groups exist. Cohen (1964) shows that initiation ceremonies in primitive societies

occur where corporate descent groups are present. Young (1964) shows that the intensity of initiation rituals among societies depends on the degree of male solidarity. Where it is high, the acquisition of sex roles involves greater dramatization, i.e., rituals involve more people, are more elaborate and last longer.

In modern societies, schools perform this initiation function. What then are the corporate groups into which schools initiate members? According to Swanson (1971), the corporate existence of a collectivity is found in (a) its legitimated sphere of jurisdiction and (b) its legitimated procedures for collective action. Collectivities that have rights and interests of their own, independent of the purposes of their members, enjoy a greater scope of legitimate action. Furthermore, collectivities that have created an agency with the right and power to act on behalf of the whole, articulating its purposes and defending its interests, are more likely to undertake collective action.¹ Educational institutions are a major vehicle of representation and defense for corporate groups.

As this discussion indicates, collectivities can be ordered in terms of their "corporateness" within and across societies. In modern societies, the major competing units are: nation-states, attempting to mobilize their societies for economic development; traditional religious bodies; and regionally based ethnic-racial groups. In American society, the national state has been relatively weak as a corporate unit (see Huntington, 1968: 93-139; Burnham, 1970: 175-93). It has acted mainly as a device to aggregate and represent the interest of a broad spectrum of groups, rather than as a mobilizing agent in its own right. One consequence of this is that direct control of education has been left in the hands of local bodies, e.g., counties, states and private groups. Compared to other societies, national efforts to politically construct the educational system to turn out the "New American Man" and to recruit national political elites have

¹ The discussion of corporateness relies heavily on the work of Meyer and his associates (see especially, Meyer et al., 1973; Ramirez and Weiss, 1975).

Allocation to Membership in Corporate Group	Social Status in Wider Society	
	Elite	Non-Elite
Yes	1. Myth of leadership selection by achievement and value socialization	3. Myth of value socialization and skill transmission
No	2. Myth of performance-based leadership elite	4. Myth of skill transmission

Figure 1. Typology of Status-Allocation Myths Constructed by Colleges

been weak (Ramirez, 1973). For example, the U.S. has no national university to select and train high-level civil servants, though there have been many proposals to establish one (see Tyack, 1966).

While the state as a corporate unit in American society has been weak, religious bodies and regional ethnic groups have been relatively strong sources of corporate solidarity (see Greeley, 1972). Private schools have been major vehicles for articulating the interest of such groups. The diversity of American education rests on the strength of these primordial status groups (see Jencks and Riesman, 1969; Astin and Lee, 1972). While the professoriate may be rationalizing the internal culture of these institutions in similar directions, the point remains that these institutions represent the interest of special groups and are involved in conferring group membership status on new generations.

The second dimension of our typology then concerns the function of colleges as agents of corporate groups and the claims they must make about the socialization graduates have received which makes them bona fide members of such groups.

The typology has two dimensions, based on the functions colleges claim to be performing: (1) as elite selection and training institutions and (2) as agents of corporate groups. To illustrate: private institutions often claim to be performing both functions. Public institutions are divided between those claiming elite training functions and those involved in middle-class occupational certification. The typology is presented in Figure 1.

These two dimensions of status allocation involve two separate kinds of claims colleges must make about their graduates:

(1) the level and quality of intellectual selection and training they have received and (2) the intensity and kind of value socialization students have undergone. Our purpose is to argue that organizational structures serve as *indices* of each of these socialization processes and thus operate to construct and validate social descriptions of graduates. Organizational development reflects the particular historical balance between these claims at which given institutions attempt to arrive. The dominant movement in American higher education has been for schools to de-emphasize their role as agencies of elite formation and as agents of primordial status groups. This is due partly to the economic costs of appropriate organizational arrangements and to the rationalization of academic culture.

For convenience, let us discuss these ideas one at a time, realizing that, in fact, institutions may have to legitimate several sets of descriptions of graduates simultaneously.²

² We need to emphasize the independence of legitimating myths from the actual "contextual" effects schools have on students. The research on higher education is consistent in the fact that differential selection accounts for most of the effects of colleges in affecting students' status allocation, though there are small and interesting aggregate college effects (see Feldman and Newcomb, 1969; Astin and Panos, 1969; Meyer, 1970a; Farkas, 1974). Furthermore, college effects on students' attitudes and values also appear to be very limited. Empirically, very little diffuse socialization appears to occur directly as the result of college structure (see Meyer, 1970b; for other views, see Parsons and Platt, 1970; Dreeben, 1968). Unlike Bidwell and Vreeland (1963), who introduced the distinction between "moral and technical socialization," our focus is on definitions of graduates which colleges develop and *not* on the "contextual effects" their organization produces.

Value socialization. Colleges that define themselves as agents of corporate groups must maintain a commitment to diffuse socialization so that graduates are seen as more committed to group values and traditions than when they entered college. Examples of such schools are: ethnic or racial institutions, private sectarian colleges, military academies and, perhaps, colleges attempting to create an intellectual elite (see Clark, 1971). In addition, if they define their mission as that of providing leadership elites for such groups, these colleges must create special rituals that separate potential leaders from non-leaders and give the former special training and status within the school. To do this, colleges must develop screening mechanisms and other organizational arrangements to inform audiences that group values are being effectively transmitted.

Competence selection and training. All schools must make some claims about the competencies of graduates. Colleges training societal elites must, however, symbolize different kinds of preparation. They must sustain the idea that graduates have been rigorously selected and have received intensive intellectual training and opportunities to develop analytical skills and leadership qualities. High status private and public institutions are examples of this type. Elite technological institutions also probably fit this category. Control over recruitment, high internal standards and other arrangements that reflect these ideas are essential symbolic devices for these schools.

Lower status colleges must also construct competency myths about graduates and maintain them. These ideas center around occupational skill training that graduates receive. Public colleges, less prestigious state universities and poor private institutions are examples of these schools. This is the largest group of colleges. Such institutions must sustain the idea that graduates possess a variety of technical skills which are available *only* to those who attend college. These conceptions of graduates are effectively symbolized by a highly differentiated curriculum and degree structure and other

certification rituals, e.g., internships, practicums and specialized facilities.

As the typology indicates, some colleges must construct complex sets of descriptions of graduates via their organizational arrangements. This produces problems of balancing and integrating sometimes competing definitions and of maintaining organizational audiences.

Legitimizing Myths and Ritual Structures

Our task now is to describe briefly the organizational structures which colleges develop to support their definitions of graduates and to provide an interpretation of the dramaturgic functions of specific organizational arrangements. The list of organizational features below is incomplete, but it provides a useful starting point for this analysis.

A. Selectivity. This is a major legitimating strategy for colleges who are trying to define their graduates as elites. Selection rituals can either be established at the point of entry to the school, thus creating the myth that admission to the school itself confers elite status (Turner, 1961) or they can be developed as internal features of the organization (Dornbusch, 1955). Public universities, for example, often have negotiated stringent selection rules about who will be admitted with their major client, the state. Prestigious private colleges can control selection directly through admissions policy.

Where such direct control is weaker, colleges often resort to developing internal selection processes that establish their reputation as highly competitive, meritocratic institutions. Flunking out large numbers of students and creating the myth of the freshman year are examples of the devices used to establish this idea among clients.

Schools that define their graduates as special leadership elites may develop selection rituals both at the point of entry and during the early part of the school career. The national military academies, for example, use such strategies since they have to establish that their graduates have both intellectual skills and special group-relevant leadership qualities. Inter-

nal selection symbolically guarantees audiences that the school has devices for separating those with leadership qualities from non-leaders, even though most students successfully pass this internal screening.

B. Residentiality and "collegiate" residential structure. This variable has figured prominently in the theoretical literature (e.g., Newcomb, 1943; 1962) and in debates among educational policy makers (see Brothers and Hatch, 1972, for a summary of the British policy debates). Though there is *no* consistent evidence to support the claim, a great deal of importance has been attached to residential structure in American educational theory (see Feldman and Newcomb, 1969; Brothers and Hatch, 1972, for a summary of the evidence). This emphasis on the direct socialization consequences of residential structure overlooks its important dramaturgic effects on the audiences of schools. Colleges that are the moral agents of corporate groups must indicate that their members have undergone intense socialization experiences and have been personally transformed into effective value carriers and agents of the group. Residential structure serves this definitional function and legitimates such conceptions of graduates. First, it establishes symbolically the transfer of socialization authority from parents to the school and thus locates the school by *definition* as possessing authority over students. This transfer of authority has historically been given legal status in the now discredited doctrine of "in loco parentis." Second, it establishes the credibility of the claim that intense socialization is actually occurring and is under the control of the school. The vesting of schools with broad moral and legal authority and the existence of a control structure, i.e., residential arrangements, confirm by definition the school's ability to affect students' values and character.

Residential structures symbolically validate the authority of colleges in another way. They *remove* students physically and socially from membership and participation in other groups and thus reaffirm the identity-conferring ability of the

college. This point has been given much attention in the socialization literature, and it is worth emphasizing *if the symbolic features* of this process are attended (see Scott, 1965). Removing candidates from "everyday life" is often an important way of calling attention to their distinctive status, apart from whether such isolation per se among peers actually has any socializing effect on recruits. In practice, it is, of course, usually empirically impossible to separate the effects created by the isolation, etc. from those created by the special status definitions such a ritual confers on them.

Residential structures vary in a number of ways that should be considered separately, though these items may approximate a scale. The dimensions are: (1) the presence of *rules* of residence requiring collegiate residence of all or most students; (2) rules specifying the responsibility of faculty for non-academic residential life; (3) the actual structure of control, e.g., faculty-student ratios *within* residential units; (4) the actual proportion of the student body living in collegiate residential units. These aspects of residential structure, we suggest, symbolize different *degrees of intensity of socialization rituals*.

This discussion of residential structure is limited to a specific historical context: twentieth-century American higher education. Historical analysis of the evolution of residential structures and educational "theories" vividly illustrates the dynamic nature of the relationship between ideology and formal structure of organizations. Residential colleges began as hostels for poor scholars in the thirteenth and fourteenth centuries (see Arries, 1962:155 ff.). Attracted by the discipline of these hostels, wealthy families began to seek admission for their children. As the social composition of the student group changed, residential "colleges" underwent a change in structure and ideology: the vocabularies of discipline were invoked to justify the entry of wealthy scholars and, in turn, the change in student social composition produced new myths about the colleges and their products.

C. College size and complexity. They refer to a central feature of bureaucracy—the degree of role specialization and structural differentiation. Complexity refers directly to the latter property, i.e., the number of functions that are incorporated in the college structure. Size/complexity functions to define for audiences the symbolic primacy and effectiveness of undergraduate socialization. First, they are likely to be seen by audiences as indicators of the *extent* of direct organizational control exercised over students by faculty and other official agents of the school. Small schools with no graduate programs, for example, are likely to be defined as involving students in direct and intense relationships with faculty, administrators, etc., regardless of whether they actually do and, hence, of having important socialization effects. Second, size is also apt to be viewed as indicating whether the school represents a community of value and, hence, is capable of moral socialization.

Size and complexity signify to audiences the intensity of socialization students undergo and ritually justify the assumption that important changes have occurred during college, e.g., increases in maturity. Schools involved in defining their graduates as corporate elites and in maintaining group membership boundaries must develop organizational legitimating strategies involving size. Several alternatives are possible. Colleges can remain small and simple in structure but pay the price of offering fewer programs and being unable to attract highly qualified faculty and students. Or they can develop graduate schools, etc., but maintain the distinct identity of the college by some form of structural separation of it from the graduate divisions. The third option is to develop smaller, decentralized college units within the context of a large, complex university. All three represent adaptations that colleges can choose to solve the problem of symbolizing their effectiveness as agents of value socialization and as agencies involved in competence-based selection and training.

Colleges without the responsibilities of maintaining status boundaries may, on the other hand, find that they must promote

internal structural differentiation in order to affirm that they are capable of equipping graduates with the depth and diversity of competencies needed for either elite or non-elite occupational status.

D. Formal curriculum. This characteristic plays a central dramaturgic function, since it directly symbolizes the purposes of the college and the meanings attached to the undergraduate experience. It also indicates to audiences the intrinsic qualities which graduates have assimilated. Both the content of the curriculum (its diversity) and the technology of instruction (tutorials, work study) are used to legitimate the symbolic redefinition of graduates (see Clark, 1971). Colleges that allocate students to membership in corporate groups will attempt to expose *all* students to similar formal socialization that emphasizes group values and traditions and that is *defined* as developing moral character or leadership abilities as well as technical skills. This may involve sectarian religious education or exposure to subjects that are defined as embodying group values, e.g., black studies, military history.

Schools preparing students for elite status in the wider society are apt to maintain a commitment to providing a broad, common education for all, e.g., general education programs, together with specialized training in diverse substantive areas. Both are important in defining students as societal elites. Liberal arts training is currently accepted as the best way to select and develop general intellectual excellence, and substantive specialization is defined as developing high level analytical skills and knowledge. Colleges that emphasize the transmission of skills and define their graduates as specialists must opt for wide diversity of programs and training technologies. A highly differentiated vocational curriculum is for them an important symbolic device in establishing that they actually transmit substantive competencies and occupationally relevant skills to students. This definition of graduates and congruent organizational self-presentation produces a continuing built-in pressure for organizational differentiation. The curriculum, in this view, is a major vehicle for the *imputation* of

motives and social identities to candidates. It is important because of its agreed-upon identity conferring properties and, once established as a central feature of college structure, such "technologies" become central elements of organizational myth and thus resistant to change. Change comes when *organizational* conceptions of "student" shift, or when externally induced changes in structure occur.

E. Demographic structure and geographical location. Moral socialization is often viewed as requiring the isolation of initiates from potential sources of disruption and corruption. In American educational thought, two influences have been seen as such, though the trend is rapidly changing: cities and the opposite sex. The former have been seen as cultural competitors to college and, hence, capable of undermining its socializing influence. Thus, until recently there has been a strong anti-urban strain in American social and educational thought.

Similarly, single-sex education also was supported as aiding the college's socializing capability. It did this in two ways: (1) this arrangement made available to the college a great deal of sublimated erotic energy and, hence, increased its control and (2) it prevented the withdrawal of students from the public life of the college into private heterosexual relationships, thus removing them socially and psychologically from the control of their peers and the college (see Newcomb, 1943). Hence, the demographic structure was viewed as another way of intensifying the college *experience* and the moral authority of the organization through the increased peer and organizational solidarity it produced.

Organizational Myths and Structure: A Set of Propositions

The main propositions of our argument are:

- A. Schools constructing elite-forming claims will dramatize their *selectivity*, whether or not they are actually very selective in recruitment. They may also organize *internal* selection rituals and dramatize them, e.g.,

academic "mortification" processes.

- (1) In addition they must also dramatize their ability to give intensive, high level intellectual training. This leads them to organize the following features: high teacher-student ratios and a highly qualified teaching staff.
- (2) Schools making elite-developing claims must remain small because size is an important way of dramatizing selectivity.
- (3) Similarly, schools making elitist claims must restrain the differentiation of faculty and programs around "specialties."

B. Schools producing occupational specialists or "college graduates" will organize and dramatize certification rituals that are bureaucratically organized and administered. These include:

- (1) a complex degree structure
- (2) a diverse and vocationally oriented curriculum
- (3) bureaucratically organized competency requirements, e.g., courses, tests, internships, time in residence, etc.

C. Schools producing entry into corporate groups will organize residential structures and other boundaries between the school and society and will dramatize such features.

These propositions should be seen as dynamic and nonrecursive, though we have framed them to emphasize the effects of ideology on formal structure. The order of causality could just as easily have been reversed. Pressures for change may come from either structural or ideological shifts.

Dynamics of Organizational Growth and Change

This argument about the symbolic functions of organizational structure indicates that schools adopt specific features not for reasons of internal functional efficiency, but because the *costs in terms of*

external legitimacy of not adopting them are prohibitive. Organizations develop and change in response to the prevalent functional theory of organizational structure in their environments (see Meyer, 1975; Blau, 1973:258ff.). While we have emphasized the cost in legitimacy to an organization of adopting formal structures that are defined as inappropriate to its proclaimed functions, i.e., socialization outcomes, schools also can incur such costs in another way. They can adopt or maintain ideologies about graduates that are incompatible with changes in structure that have occurred. Schools can become victims of their own mythology in periods of rapid social change.

For educational organizations, the symbolic function of their structures creates special dilemmas. Competency claims and value socialization claims may dictate the choice of very different kinds of structures as legitimating devices. For example, colleges that seek to define their graduates as societal elites must attract highly able students and faculty. Both, however, are attracted to large institutions that are both affluent and highly differentiated. The best faculty want and need colleagues working in their specialty, and able students are lured by the presence of an eminent research-oriented faculty and a rich diversity of departments and programs (see Blau, 1974). Similarly, schools attempting to maintain corporate status boundaries also must be able to certify students as possessing special skills. Such certification, however, increasingly requires a highly differentiated curriculum. Yet these pressures for differentiation and growth threaten to sacrifice irreversibly some of those very structural features discussed earlier by which colleges legitimize their ability to affect intensive intellectual training or diffuse value socialization. Historically, the issue for many schools has been the choice between growth and expansion of organizational functions or maintenance of ritual structures appropriate to maintaining group status boundaries or for elite development. Advocates of expansion and academic quality frequently have won these battles and thus many schools have

given up conceptions of graduates as societal elites or as members of corporate groups.

This argument about the relationship between the conceptions of students that schools construct and their social structure suggests two general propositions about rates and levels of growth and differentiation: (1) *rates* of growth will be lower for universities and colleges which define themselves as elite training institutions or as the value socialization agencies of corporate status groups and (2) the absolute *levels* of size and structural differentiation will be lower for such institutions *when* the level of institutional resources and other indicators of academic quality, e.g., faculty prestige, are controlled. Given the nonrecursive structure of the relationship between ideology and formal structure, we also expect that schools that opt for high growth rates or that experience high levels of differentiation will drop elitist claims and ideas about diffuse socialization in favor of new ideologies about their function and effects on students.

Two recent research findings help to illustrate this argument. While the first is readily susceptible to alternative interpretations, the latter is more puzzling and less easily explained. In a survey of college administrators, Sieber et al. (1968:18-9) found that among officials at *private* universities the greatest amount of ambivalence or mixed feelings occurred concerning goals pertaining to *organizational expansion*. As Sieber et al. (1968:18-9) observe, "the issues seem to be whether private universities should share the functions of other types of institutions of higher education. . . ." Public university administrators were *not* as likely to have mixed feelings about expansion but were more ambivalent about the goals of developing a national leadership cadre, developing moral capacities and ethical standards, and several other service goals (Sieber et al., 1968:18). Private universities were also found to exceed all other institutions in emphasizing the organizational goal of "developing a leadership cadre to serve the nation and the world" (Sieber et al., 1968:21). This evi-

dence suggests the negative relationship between the *goals* of elite formation and organizational expansion.

A second set of findings shows that established institutions are less differentiated than others. In a study of American colleges and universities, Blau (1973:256) found that *older* universities were likely to attract better students, to pay better salaries and thus recruit more qualified faculty *but* were also *less* likely to create new departments, i.e., were less structurally differentiated. He interprets this resistance to innovation as the result of academic conservatism engendered by a long tradition of scholarly leadership. An alternative interpretation suggests that organization *age* is associated with elite training goals. According to proposition two, older universities should also be smaller and less complex when the level of institutional resources is held constant (see Huntington, 1968:13ff. for a discussion of the meaning and correlates of organizational age). Substantively, organizational age may indicate the ability of schools to claim a role in the development of national leadership elites. Older universities are more likely to be intensely committed normatively to corporate value socialization and goals of elite formation, since they are associated with charter ethnic groups that have had a longstanding role in national leadership. Such colleges have developed a tradition of producing leaders of government, business and the professions, notably high finance and corporate law (see Collins, 1971); and this "saga" becomes a central definition of the rights and meanings attached to graduates. According to our argument, these symbolic conceptions of graduates should lead them to dramatize selectivity by resisting increases in size and to dramatize broad intellectual preparation of students by resisting academic specialization until such work can be integrated into the undergraduate curriculum.

The major objection to this general argument is substantive and will serve as an important qualification. In many societies, the organizational structure of schools is not an important source of legitimation of symbolic definitions of

graduates because the latter are institutionalized *in society*. In the Philippines, for example, the meaning and functions of the national high school in Manila are established by the fact that its graduates are *defined* as important political actors and as prospective political elites by virtue of their *admission*. The prestige and meaning of "student" derive from the school's chartered role as an agent of the state. Organizational structure plays little role in this process (Meyer, 1970b). One empirical consequence of the institutionalized definitions of the school and its graduates is that students who pass the series of national exams for admission experience dramatic changes in self esteem *before* they even reach the school (Benitez, 1973).

Such schools have to dramatize their selectivity, but this is done by *societal* arrangements, e.g., nationally-administered entrance tests, and not by organizational features. Also, such schools do not need to dramatize the separation of school from society by the construction of residential features and other symbolic structures. Value socialization is validated by the institutionalized societal definitions of the school and its graduates. Hence, the legitimating function of school organization is constrained by societal definitions of education and of schools.

Conclusion

This paper has stressed the importance of the social meaning attributed to university attendance for understanding organizational structure and development.

Rather than review all of the argument, we will summarize the major idea and indicate its implications for the study of schools.

Schools symbolically redefine people and make them eligible for membership in societal categories to which specific sets of rights are assigned, e.g., income. The social organization of schools is a major symbolic index of the kind of socialization that has occurred and thus legitimates the conferral of specific status rights. Organizational characteristics are thus causally

linked to the social meanings attached to university attendance.

This argument has several research implications for the study of schools. The following kinds of research are required to develop and test this line of argument: (1) comparative studies that investigate the relationship between organizational features and the specific images schools present of themselves and their graduates; (2) research that investigates whether schools with elitist myths and appropriate organizational structures actually are more likely to produce elites than schools with only one of the two components (either myth or structure); (3) studies over time of how changes in either myth or structure trigger changes in the other; (4) studies of the fate of organizations that *resist* synchronizing their ideology and structure, either by holding on to myths long after their supporting structures have faded or by maintaining organizational structures associated with outmoded images of students. This is an area where case studies of the *process* of change and resistance would be useful.

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POLITICAL WITCH HUNTS: THE SACRED AND THE SUBVERSIVE IN CROSS-NATIONAL PERSPECTIVE*

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This paper proposes a general theory of political witch hunts, viewing them as ritual mechanisms for the periodic rejuvenation of collective sentiments in national societies. Ultimate national purposes require not only their worshipers, but also their enemies. When these sacred forces penetrate daily reality, then their opposites—subversives—will also appear in daily institutional life. The corporateness of societies, as expressed in their political system, is theoretically linked to the penetration of transcendent reality into daily life, and a witch-hunting dispersion index is proposed to measure the extent to which subversion is ritually discovered throughout a society's social institutions. The overall rate of witch hunting is also measured. Data on rates of political witch hunting between 1950 and 1970 for 39 countries is presented to evaluate the general theoretical argument. The data suggest that as societies politically express more of their corporate national interest they ritually cleanse more institutional areas, as measured by the dispersion index. Along with the representation of the corporate national interest, the overall rate of witch hunting is significantly affected by country size, level of economic development and the relative power of the state. The dispersion of witch hunting, on the other hand, is unaffected by these control variables and seems to be a more purely Durkheimian phenomenon.

Modern national societies, like primitive societies, must periodically renew the meaning of corporate existence. Where rites were once performed to symbolizations of the tribe, they are now performed to symbolizations of the nation-state. Durkheim (1965) observed that sacredness required profanity, and sacred national purposes seem to require those who would undermine and subvert them. Similarly, as Durkheim argued that crime is a normal aspect of social life, so are political witch hunts. The nation-state's creation of political subversives, no less than the community's manufacture of deviance, is a mechanism for renewing common moral sentiments and redefining the contours of social reality.

This theoretical perspective on political witch hunting derives from and links Durk-

heim's observations on religious ritual and myth and the social functions of crime. His work in religion and crime has a common concern with the function of ritual activity in creating and maintaining collective reality, whether symbolic representations in the analysis of primitive religion or moral boundaries in the analysis of crime. However, there has been little intellectual contact between later students of the sociology of religion (for example, Swanson, 1964; 1967; Douglas, 1966; 1970; Goffman, 1956; Berger, 1969; Luckmann, 1967) and the sociology of deviance (Erikson, 1966). Moreover, this theoretical formulation is congruous with two empirical findings which recur in studies of political trials (Cohen, 1971; Kirchheimer, 1961), purges (Brzezinski, 1956; Conquest, 1968; Dallin and Breslauer, 1970), rectification campaigns (MacFarquhar, 1960; Goldman, 1965; Solomon, 1971; Baum and Teiwes, 1968) and loyalty controversies (Bell, 1964; Lipset, 1955; Parsons, 1955). Efforts at exposing, unmasking and discovering subversives seem oriented as much toward dramatizing their presence as toward ac-

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tually apprehending them; and, accordingly, the arrests, trials, confessions and other acts which comprise a modern political witch hunt seem largely ritualistic in character.

The ritual mobilization of a community searching for imaginary enemies traditionally has been viewed as largely epiphenomenal—the real causes of witch hunting are thought to lie elsewhere. The trials, purges and political terror of Communist regimes, for instance, have been seen as an outcome of the fear and paranoia generated by totalitarian states and as an “instrument of the state” used by elites to thwart potential opposition and maintain political power (Brzezinski, 1956). Similarly, the hysteria of the McCarthy period has been dismissed as the paranoid projections of social groups experiencing status stress (Bell, 1964). In contrast, I want to propose a theory of political witch hunts explicitly based on their distinctly mythical and ritualistic character.

THEORY

The Sacred and the Subversive

For Durkheim, the moral reaffirmation of collective life involved two causally interrelated notions—one, that society presents itself through a variety of symbolizations or collective representations ranging from material objects to systems of ideas; the other, that society mobilizes itself through rites performed to these symbolizations as a means of periodically renewing their presence and simultaneously renewing the larger social order they symbolically represent. Like primitive societies, modern national societies present themselves through numerous symbolizations. Some are material objects like flags, emblems and political leaders. Others are symbolic representations of the national collectivity itself—images of *the Nation* or *the People* as organic entities, or political ideologies such as Communism, Socialism, Fascism or Democracy. As in primitive societies, rites are performed to these modern collective representations of the corporate national community. Some of these rites are what

Durkheim (1965) called *positive rites* where *the Nation* and *the People* are worshipped: coronations, inaugurations, national holidays and memorial days. Others are *negative rites*, like witch hunts. Through such activities as the purge, trial, investigation, accusation, arrest and imprisonment, society creates its own internal enemies to ritually reaffirm the very sacred national purposes which subversives are supposedly undermining. The corporate nation needs not only its worshippers, but also its enemies. This process of moral revitalization is complex, centering on the interrelationship of corporate society, its collective representations and the ritual which rejuvenates and redefines them.

The Penetration of Ordinary Life by Sacred Forces

Social reality is a matter of definition. It is neither naturally mundane, with ordinary people and profane motives, nor sacred, with transcendent political purposes animating individuals and social events. Depending upon how closely a nation's collective myths are merged with the profane and ordinary, more or less ultimate political significance can be infused into daily existence. The penetration of daily life by sacred forces is, in effect, a variable, and not the singular property of primitive religious systems as we have traditionally believed.¹

In pluralistic Western societies, by and

¹ Bellah (1970:27), writing on religious evolution, argues that one of the distinguishing characteristics of primitive religion is “the very high degree to which the mythical world is related to the detailed features of the actual world. Not only is every clan and local group defined in terms of the ancestral progenitors and the mythical events of settlement, but virtually every mountain, rock, and tree is explained in terms of the actions of mythical beings.” The same could be said for the so-called modern religious situation. In certain highly ideological societies, for example, virtually every individual action is linked to the transcendent world of History or Nature and every event is seen as an instance of the mythical forces of “imperialism,” “socialism” or “capitalism.” Because our religious representations are at the same time the theory of our own universe, we have had a very difficult time assigning some beliefs to the category “myths” and others to what we take as constituting our “real” reality.

large, life is understood as the interaction of groups and personages governed by secular and profane motivations of the here and now. Life is not filled with the rumbling and movement of Marx's historically preordained struggle between social forces in "constant opposition to one another, carrying on an uninterrupted, now hidden, now open fight." Conversely, the two most pronounced examples of sacred political forces mingling with people in daily life are Fascist and Communist societies where there is a very close connection between religious representations and daily existence.

Underlying the Nazis' belief in race laws as the expression of the law of nature in man, is Darwin's idea of man as the product of a natural development which does not necessarily stop with the present species of human beings, just as under the Bolsheviks' belief in class-struggle as the expression of the law of history lies Marx's notion of society as the product of a gigantic historical movement which races according to its own law of motion to the end of historical time when it will abolish itself. (Arendt, 1973:463)

These ideologies can be considered Durkheimian representations of the corporate social order. Individuals come and go but groups persist: the idea of an unending universe of evolving Nature or History is the perfect symbolization for the corporate continuity of society over the temporary lives of mundane individuals. From the point of view of highly corporate societies, those countries in which groups and classes do not realize their participation in the transcendent reality of historical development are said to have "false consciousness." From the other side, these corporate societies are described as highly "ideological" and their people as "brainwashed." Each side claims its experience of reality to be correct and the other's false. What is experienced, though, is mediated by each society's definition of reality, and there is no absolute reality independent of society and its imposed system of classifications and definitions.

Political ideologies, like the sacred forces of History and Nature, or the ideas of *the People* or *the Nation* can penetrate and merge with ordinary reality so that

daily activity becomes the realization of these transcendent realities. The ritualistic creation of enemies, as a means of renewing the presence of these sacred forces, centers on discovering "enemies of the people," the nation, and even enemies of Nature and History itself.

Terror . . . its chief aim is to make it possible for the force of nature or of history to race freely through man . . . [it] singles out the foes of mankind against whom terror is let loose, and no free action of either opposition or sympathy can be permitted to interfere with the elimination of the "objective enemy" of History or Nature, of the class or the race. Guilt and innocence become senseless notions; "guilty" is he who stands in the way of the natural or historical process which has passed judgement over "inferior races," over individuals "unfit to live," over "dying classes and decadent peoples." Terror executes these judgements, and before its court, all concerned are subjectively innocent: the murdered because they did nothing against the system, and the murderers because they do not really murder but execute a death sentence pronounced by some higher tribunal. The rulers themselves do not claim to be just or wise, but only to execute historical or natural laws; they do not apply laws, but execute a movement in accordance with its inherent law. Terror is lawfulness, if law is the law of the movement of some suprahuman force, Nature or History. (Arendt, 1973:465)

Ritual Transformations: Trial, Purge, Accusation, Investigation

There is no clear line separating ordinary mundane reality and the larger purposes and personages of a transcendent reality such as History or Nature. One can see himself, his work and others as the embodiment of the laws of history working themselves out here on earth, as when factory work is experienced as building socialism and realizing the historical role of the proletariat. One can also find oneself engaged in mortal combat with these cosmic forces and being charged with being an enemy of socialism and conspiring against the laws of social development. In the first situation, one is marching with the sacred forces that have penetrated one's existence and, in the other, one is marching against them. In both

situations, one will probably be doing identical activities—only the definitions will have changed.

Political witch hunts are the ritual mechanisms that transform individuals, groups, organizations or cultural artifacts from things of this world into actors within a mythical universe. These rituals are the social "hooks" that keep sacred transcendent forces present in the lives of ordinary people and relevant for everyday institutional transactions. In effect, Berger's (1969) "Sacred Canopy" extends to different lengths, more or less penetrating everyday reality, with ritual being the "lock" which keeps sacred reality tied to daily reality. The discovery of "wreckers" in factories, "hostile elements" in the Party bureaucracy, "bourgeois thoughts" in literature, and "anti-state" activity in government is the ritual activity which functions to reaffirm the presence of the sacred struggle between Capitalism and Socialism within the fabric of everyday life.

The crimes one is charged with and the motives imputed are not of this world. Through some ritual transformational logic, they become part of the mythical reality of political ideology where, for example, "proletarian virtue" is battling "bourgeois selfishness" and the "socialist and capitalist lines" are struggling for supremacy. Trivial activity is ritually transformed into actions of large historical forces. Using one kind of fertilizer or another on a communal farm is transformed into the mythical world of "taking the capitalist road" or "following the socialist road." Reading a book or seeing a play is transformed into being "poisoned by bourgeois thoughts" or being a "communist sympathizer." Correspondence with friends abroad can be transformed into acts "restoring capitalism," making one an "imperialist agent" and "counterrevolutionary."

When these ritual convulsions occur, ordinary reality loses its usual meaning and human beings mingle with mythical beings, playing roles in a cosmic drama. As Hannah Arendt observed, questions of guilt and innocence are irrelevant; they are causal relations rooted in the structure of meaning of mundane reality and make

no sense when the definition of reality has been ritually shifted to the mythical world of political ideology. What one "really" did or did not do is irrelevant, for the "doing" has meaning in a world that has been supplanted by a mythical universe with its own characters, motives and crimes. Being an "ultra-leftist," "ultra-rightist," or "left in appearance but right in essence" makes no sense within the meaning structures of this world; they are acts, motives and types of people from the world of political ideology.

One does not enter this mythical world through any act one performs. The sacred and profane are two different worlds, there is nothing one can do in one world to enter into the other. Hence, guilt and innocence have never mattered during political witch hunts. The most trivial infraction, or no action at all, has the same weight as the most serious crime. It is the activity of the trial, the purge, the accusation or the self-confession which transforms individuals from one reality to the other.

It is no wonder then that witch hunts appear irrational, terrifying and *unreal*. In some sense, they are truly unreal and irrational, for their logic derives from the symbolic significance of the ritual encounters between mythical beings and forces and not from the actualities of human conduct. There is no intrinsic quality to that which is treated as sacred and there is no intrinsic quality to individuals considered subversive and dangerous. Durkheim wondered how such insignificant things as lizards, frogs, turkeys, ants and caterpillars could, in and of themselves, engender the sense of the sacred; and we have also wondered how the wording of a Girl Scout Handbook, a play exploring the thoughts of a conscientious objector, or interest in the United Nations, race relations and civil liberties could be considered dangerous, subversive and un-American. Anything can serve as a vehicle for the designation "sacred" or "subversive," and anyone can become "an enemy of the people" and "think dangerous thoughts." As Arendt (1973:462) observed:

Totalitarian lawfulness, defying legality and pretending to establish the direct reign of justice on earth, executes the law of History

or of Nature without translating it into standards of right and wrong for individual behavior. It applies the law directly to mankind without bothering with the behavior of men.

Societal Differences in Witch-Hunting Activity

Collective representations mirror the social order, and Durkheim's observation that the attitude of respect toward our gods is similar to the attitude of respect toward social authority provides the now well-known (Swanson, 1964; 1967) theoretical linkage between corporate social groups and religious experiences with sacred forces and spirits.

As mentioned earlier, the extent to which transcendent reality merges with daily reality appears to be a variable and, as these sacred forces are symbolic representations of the corporate social order, variation in the extent to which that corporateness is expressed should be reflected in variations in the extent to which the sacred cosmos penetrates mundane reality.² The more the corporate interest of the nation as a whole is politically expressed, the more sacred forces should intervene in daily life, and the more witch hunting there should be to reaffirm these collective representations and secure their presence in daily affairs. In effect, as the corporate interest of the society is weakened, so is the strength of the gods. They become less powerful, more elusive and more tenuously tied to the specifics of daily life.

For example, representations of the corporateness of the United States are not as well developed as those of Communist countries. The idea of the American People, or Public Opinion, as a force or spirit in our lives does not penetrate our existence to the extent to which representations like "the thoughts of Chairman Mao" do in China (Schwartz, 1968). Nor

are they embodied in specific political institutions to the extent to which the Communist Party is considered the literal embodiment of the "proletarian will." Finally, the American People or Public Opinion do not have a mythical link to the specifics of historical evolution as does the Communist's Proletariat. The Manifesto stated, "The history of all hitherto society is the history of class struggles. Freeman and slave, patrician and plebian, lord and serf, guildmaster and journeyman . . ." and now, in our times, the bourgeoisie and proletariat. In comparison, the "When in the course of human events . . ." beginning to the Declaration of Independence is a much more casual and almost offhand reference to the primordial origins of the American nation compared with the lockstep progression of History in the Manifesto that has brought forth present-day Socialist countries. There is also no single body of literature for the liberal democratic West comparable to that of Marxism defining, say, America's "Historical Role." There are no "sacred texts" and no common set of intellectual heroes like Marx, Lenin and Mao. Marxism provides one version of ultimate reality, which is modified to each particular social system, but is still linked to the transcendent process of social development and the laws of History.

Expressing corporateness: political party systems. Swanson (1967; 1971) argues that social collectivities have a corporate existence; they can make collective decisions and take collective action. For national societies, the structural mechanism through which collective decisions are formulated and collective action taken is the institution of government. Collectivities vary in the degree to which they allow the expression of the corporate interest of the collectivity as a whole, as opposed to the interests of the constituent groups within the society. Political party systems are the most common structural mechanism for expressing group interests, whether corporate or constituent.³

² This theoretical discussion leans heavily on the work of Swanson (1964; 1967) on corporate groups and experiences with the sacred in daily life. His ideas on immanence and constitutional systems are the backbone of my thinking about these matters, and those familiar with his work will see the strong effect he has had on my thinking.

³ The expression of corporate versus constituent interests need not always be mutually exclusive, although it most often appears that way. One exception would be a country like France which has a

Multi-party systems, such as the European parliamentary states, allow the most penetration of constituent group interests into the structure of government. Specific groups—agricultural, religious, working class—each have their own party and, with the proportional representation electoral arrangements multi-party systems usually have, these parties are virtually guaranteed some seats in the legislature. *Two-party* arrangements, like the Anglo-American countries, collapse many specific group interests into two broadly based parties. The emphasis is upon what the different groups have in common, rather than what separates them, and more of the common corporate interest of the society is manifested. Finally, there are *one-party* states, whether Communist, Fascist or one-party nationalist. Here, only the interest of the collectivity as a whole is represented by the single party. Partial interests are not provided a formal role through parties in the political organization of these nations as corporate entities.

Some Hypotheses

The penetration of sacred forces into ordinary reality is not the sole property of primitive belief systems, and the ritualistic search for enemies as a means of reaffirming these transcendent forces is not a property of any one kind of society; both vary according to the extent to which corporate, as opposed to constituent, group interests are politically expressed. We have been referring to daily affairs and everyday reality in quite general terms. We can refer to them more precisely in terms of the different institutional spheres of which daily reality is composed. We can speak of the extent to which religious, political, economic or educational institutions are found infested and polluted with subversion.

From the preceding theoretical discussion, the following hypotheses can be

made: (1) Other things being equal, there should be a positive relationship between the expression of corporate interests and the distribution of witch hunting across a society's social institutions, reflecting the more extensive penetration of daily institutional reality by sacred forces. (2) We should also expect the expression of society's corporate interest to be positively related to increased frequency of witch hunting in general. One-party states should experience subversion in more institutional areas and have a higher overall rate of witch hunting than two-party states, and they, in turn, should experience subversion in more institutional areas and have a higher overall rate than multi-party states.

DATA AND METHOD

Independent Variables

Countries were chosen for analysis if they had a stable party system from 1950 through 1970. A stable system is defined as one in which the party system did not change and the country was not involved in major political conflicts. The attempt was to isolate, as much as possible, the effect of party system upon a country's propensity for witch hunting. The countries are listed in Appendix 1. A country's party system was coded from Blondel's (1973) classification of national legislatures. Countries were coded one, two and three, representing one-, two- and multi-party systems. The 39 countries differ in population size, level of economic development and the degree to which political power is concentrated within the state, as well as with respect to political party system. The former factors may be seen as rivals to party system in accounting for cross-national differences in witch hunting and, as I am arguing that *other things being equal*, corporateness should be related to the dispersion and rate of witch hunting, I have compiled measures of each country's 1960 population (IBRD, 1973), 1960 per capita GNP (IBRD, 1973) and per capita internal security forces (Taylor and Hudson, 1971) to control for these factors.

multi-party system allowing for the expression of constituent group interests and a strong and centralized national bureaucracy which expresses the corporate interests.

Dependent Variables

Frequency of witch hunting. Counts of witch hunting activity are obtained by coding government activities (for example, trials, purges, arrests) from news sources whenever these activities charge someone with threatening or standing in opposition to the national interest. Since government represents the social structure through which the nation itself takes collective action, only acts by a country's national government will be coded (this includes the military). Charges of subversion by private citizens, for instance, will not be coded. The *New York Times Index*, 1950-1970, was chosen as the basic source because of its broad international coverage.⁴ The scheme used for coding provides for 19 different kinds of government acts, ranging from mere charges of subversive activity to large-scale purge trials. Each category is mutually exclusive. The specific code categories are: (1) warnings of danger to the nation, (2) specific charges, (3) discovery of plots, (4) calls of public action to thwart subversion, (5) proposed actions, (6) new laws, ordinances or executive decrees, (7) restrictions of personal activity, (8) expulsion of agents of foreign governments, (9) deportations, (10) arrests, (11) imprisonment, (12) resignations, (13) purges, (14) court actions, (15) mass mobilizations, (16) censorship campaigns, (17) political trials, (18) sentences following trials and (19) government investigations. This scheme is intended to cover the great variety of government activity a society can employ in creating the idea of subversion in its midst.

The coding was done by four graduate students. Activities were assigned codes independently by two coders and the value of Robinson's (1957) coefficient of

agreement was found to be .85. This compares favorably with the reliability achieved by other researchers who have coded news sources (see Gurr, 1968; Feierabend and Feierabend, 1966; Banks, 1971).

The coding scheme also provides for 36 different institutional areas in which subversion might be discovered. The information available about the activities made it impossible to determine the institutional areas for 57 percent of the government activities coded. For the present analysis, these 36 areas are grouped into seven general institutional categories: (1) government—national and local, (2) military personnel and facilities, (3) educational institutions and students, (4) economy, (5) intellectuals, (6) religious groups and institutions and (7) a category for foreigners.

The coding procedure works as follows. The *Times Index* is read for each country. When one of the code activities (a trial, purge, arrest) is encountered, it is coded along with the institutional area where the subversion was located. This procedure generates the basic data on both the overall volume of witch hunting (the number of government activities) and its institutional location. Some countries could not be coded for the full 21 years (such as the African countries, which did not become independent until around 1960), so each country's total number of activities was divided by the number of years coded, creating a witch-hunting per year variable.

The Witch-Hunting Dispersion Index. To measure the dispersion of witch-hunting activity throughout a society's institutional space, a population diversity index is employed. This 'dispersion' index, adapted from Simpson's (1949) index of population diversity, is defined as

$$D = 1 - \sum_j \frac{N_j(N_j - 1)}{N(N - 1)}$$

where N_j equals the number of witch-hunting activities in the j th institutional category, N equals the total number of witch-hunting activities and D equals the degree of dispersion of witch hunting across institutional categories. The higher the score on the index, the more dispersed

⁴ There are numerous sources of bias involved in using press reports. There are actions taken by political authorities in identifying and prosecuting subversives that never reach the pages of the *Times*. This would seem particularly true of Communist states and countries with little contact with the United States. There is also undoubtedly a bias in terms of the number of events reported for large and prestigious nations over those smaller and less known. For a discussion of some of the problems in coding news sources, see Danzger (1976).

the witch hunting across institutions. This index was computed across the categories: government, military, education, economy, intellectuals, religion, foreigners and agents of foreign governments. At least ten government activities were arbitrarily chosen as a minimum to compute an index score. No index was computed for the following countries, as they had less than ten acts within their institutional areas: Chad, Guinea, Ivory Coast, Senegal, Tanzania, Australia, New Zealand, Colombia, Belgium, Iceland and Ireland.

FINDINGS

The Dispersion of Subversion through Institutional Space and the Overall Rate of Witch Hunting

We can measure the extent to which different institutional areas are polluted with subversion using the dispersion index mentioned earlier. The higher the score the more dispersion; the lower the score the more witch hunting is concentrated within a few institutional areas. Appendix 1 presents the total number of witch-hunting activities by institutional area for each country. Control variables

for country size (population), level of economic development (GNP) and the relative power of the state (internal security forces) are entered into the following regression analysis along with the variable political party system. It was logged to help correct for the skewed distribution of the witch-hunting per year variable. The correlation matrix for the regression analysis is presented in Appendices 2 and 3.

The regression analysis in Table 1 strongly supports our first hypothesis that, other things being equal, there is a positive relationship between party system and the dispersion of witch hunting throughout a society's institutional space. The political party variable has a strong effect, with a beta of .575. It is also the only unstandardized coefficient twice its standard error. The effect of population and GNP are negligible with betas of .047 and .042, respectively, and the indicator of state power, internal security forces, has a small negative effect with a beta of -.242.

The second hypothesis stated that, other things being equal, there is a positive relationship between party system and the overall rate of witch hunting. Political party system was significantly re-

Table 1. Regression Coefficients of Witch-Hunting Dispersion Index and Log Witch Hunting per Year on Political Party System, Internal Security Forces, Population and per Capita Gross National Product

Dependent Variables	Independent Variables				Constant	R ²
	Political Party System	Internal Security Forces	Population	Per Capita GNP		
Unstandardized Coefficients ^a						
Witch-Hunting Dispersion Index	46.162 (22.917)	-.762 (.688)	.0027 (.0118)	.00473 (.0298)	774.20 (69.89)	.271
Log Witch-Hunting Activities per Year	.698* (.307)	.193* (.0088)	.000538* (.000174)	.0009* (.00041)	2.675 (.950)	.446
Standardized Coefficients						
Witch-Hunting Dispersion Index	.575	-.242	.047	.042		
Log Witch-Hunting Activities per Year	.455	.313	.438	.429		

Sources: Political Party System=One-, Two- and Multi-Party. Internal Security Forces=Internal Security Forces per thousand working age population (Taylor and Hudson, 1971). Population=1960 population in millions (I.B.R.D., 1973). GNP=1960 Gross National Product, per capita (I.B.R.D., 1973).

^a Standard errors in parentheses.

* $p < .05$.

lated ($p < .05$) to the overall rate of witch hunting, but so were the control variables. All of the independent variables were significant at the .05 level and all the unstandardized coefficients were twice their standard errors. The overall rate does not seem to be a simple function of the representation of corporate or constituent interests.

One-party countries have the highest rate of witch-hunting activity. They have a median 6.1 log witch-hunting activities per year compared with only 1.6 for two-party and 1.2 for multi-party. The large score (Appendix 1) for the United States should be interpreted cautiously, as it is undoubtedly inflated by the very extensive coverage the *Times* gives the U.S. Except for Ghana, the African one-party states have very little activity. Most one-party witch hunting, therefore, was conducted by Communist countries. We have categorized countries by party system, but we have no way of further differentiating among Communist states as to the representation of partial or corporate interests within these one-party regimes. Some support for the applicability of the general theoretical argument that witch hunting is related to representing solely the collective interest at the expense of competing or partial interests is suggested by Hannah Arendt. She observes that, even for one-party Communist and Fascist states, the further elimination of competing interests is associated with an increase in witch hunting and political terror.

terror increased both in Soviet Russia and Nazi Germany in inverse ratio to the existence of internal political opposition, so that it looked as though political opposition had not been the pretext of terror . . . but the last impediment to its full fury. [In Soviet Russia] . . . full terror did not break loose in the twenties but in the thirties, when the opposition of the peasant classes was no longer an active factor in the situation.—Khrushchev, too . . . notes that "extreme repressive measures were not used" against the opposition during the fight against the Trotskyites and the Bukharinites, but that "the repression against them began" much later after they had long been defeated.

Terror by the Nazi regime reached its peak during the war, when the German nation was actually "united." Its preparation

goes back to 1936 when all organized interior resistance had vanished and Himmler proposed an expansion of the concentration camps. (Arendt, 1973:393)

To assess more systematically the importance of Communist countries per se, as opposed to the more general Durkheimian idea of corporateness, one would want to consider examining other historical forms of one-party states, developing an indicator of corporateness other than party system, or searching for a means of differentiating, among one-party states, their relative expression of corporate and constituent interests.

The Distribution of Subversion across Specific Institutional Areas

We can more closely examine the distribution of witch-hunting activity by looking at the percentage distribution of ritually discovered subversion in different institutional areas (see Table 2). The Government, Military, and Foreigners categories generally have the highest percentages of witch-hunting activity. If we exclude the Religion category, because of confounding factors for many of the newer one-party states which will be discussed later, then these three categories account for 64.4 percent of the total in one-party states, 74.4 percent in two-party states and 73.9 percent in multi-party countries. This suggests that the creation of subversion surrounds structures and persons who are related in some fashion to the distinctly corporate aspect of a country's existence. Government and military provide the organizational structure through which nations attain their corporate existence and seem imbued with larger political significance and, accordingly, those who would subvert those sacred national purposes. Foreigners, on the other hand, relate to the corporate nation in a different manner. The very definition of foreigners, as outsiders and non-members of the national collectivity, is derived from their relationship to the corporate nation as a whole.

Multi-party states discover a very large proportion (42 percent) of their subversion among foreigners. This could reflect

Table 2. Percent Distribution of Witch-Hunting Activities by Institutional Area and Political Party System

Institutional Area	One-Party		Two-Party		Multi-Party	
	Percent	N	Percent	N	Percent	N
Government	26.4	549	33.1	212	15.0	54
Military	4.5	93	19.3	124	15.0	54
Education	11.2	233	12.2	78	9.2	33
Economy	5.7	119	6.6	42	3.6	13
Intellectuals	10.8	225	6.4	41	12.8	46
Religion	21.9	455	1.9	12	1.9	7
Foreigners	19.4	402	20.6	132	42.5	153
Total	100.0	2076	100.0	641	100.0	360

the low degree to which their institutional structures are imbued with larger political significance. These countries, as nation-states, have a corporate existence, although they do not formally express as much of their distinctly corporate interests as two- and one-party states. Accordingly, sacred national purposes are not as extensively infused into their institutional structures and, consequently, they conduct much of their witch hunting outside their institutional infrastructure, e.g., among foreigners.

The percentages among the general institutional categories, Education, Economy and Intellectuals, are all quite similar. There is, though, a large discrepancy between the proportion of witch hunting centering on persons from the general category of religious groups and institutions for one-party countries (22 percent) and two- and multi-party countries (2 percent). This high percentage for one-party states could reflect the church-state struggles involved in the nation-building process of those new states which emerged following the Second World War. Partial evidence for this is found in comparing the amount of witch hunting which occurred during the early 1950-1955 period for new and older one-party states, with the latter having already passed through many of the pangs of nation-building. The new states (China, East Germany, Albania, Bulgaria, Czechoslovakia, Hungary, Poland, Rumania and Yugoslavia) averaged 73 percent of their within-religious-institutions witch hunting during the 1950-1955 period. One-party states estab-

lished before 1945 (the USSR, Spain and Portugal) averaged only seven percent of their within-religious-institutions activity during this early period. The new states also accounted for some 86.6 percent of all one-party witch hunting around religious institutions which further suggests that most of this activity was tied to the problems of newly emerging states.

Subversion within Government

We also can examine the proportion of witch hunting within the four sub-categories composing the general area of government. Table 3 shows that the vast proportion of witch hunting in government is concentrated within national bureaucracies (one-party, 63.4 percent; two-party, 78.8 percent; multi-party, 71.1 percent). There are, though, some interesting differences. As the political representation of the corporate national interest is compromised with the increasing representation of constituent group interests—that is, moving from one- to multi-party—the proportion of subversion discovered around the national executive officer, who represents the interests of the collectivity as a whole, gradually decreases (one-party, 20 percent; two-party, 10.4 percent; multi-party, 1.9 percent). The inverse relationship holds for the proportion of subversion discovered within national legislatures. As the representation of constituent interests increases, the proportion in legislatures increases (one-party, 4.3 percent; two-party, 6.1 percent; multi-party, 11.3 percent). It seems that where the expression

Table 3. Percent Distribution of Witch-Hunting Activities within Government by Political Party System

Government	One-Party		Two-Party		Multi-Party	
	Percent	N	Percent	N	Percent	N
National Executive Officer	20.0	70	10.4	22	1.9	1
National Bureaucracy	63.4	222	78.8	167	71.7	38
National Legislature	4.3	15	6.1	13	11.3	6
Local Government	12.3	43	4.7	10	15.1	8
Total	100.0	350	100.0	212	100.0	53

of the nation's corporate interest is paramount (one-party) the office which stands for the collectivity as a whole, the national executive officer, is the source of more subversive activity (20 percent) than the legislature (4.3 percent), which is the structure representing constituent group interests. Conversely, where the collectivity is structured so that constituent interests are expressed at the expense of the corporate national interest (multi-party), then legislatures have a higher proportion (11.3 percent) than the national executive officer (1.9 percent), who represents the now-compromised corporate interest. Finally, two-party countries express less corporate interests than one-party but more than multi-party. Consequently, they have more witch hunting around their executive officer than multi-party, but less than one-party countries, and more in their legislatures than one-party, but less than multi-party countries.

SUMMARY AND CONCLUSION

Although the most extreme instances of political terror and witch hunting are associated with totalitarian regimes, the ritualistic search for imaginary enemies should be conceptualized as a *variable*, not the singular property of totalitarian states. The substance of the charges and

accusations and the kinds of ritual may vary from country to country, but the sociological process is identical. Nations, as corporate entities, are all searching for the same thing: the mythical enemy which stands in symbolic opposition to the collectivity as a corporate whole.

The perpetuation of social reality through the complex interaction of ritual and a mythical universe populated with all sorts of extraordinary spirits and forces is similarly not the sole characteristic of primitive religious systems. The penetration of the sacred into daily reality is also a *variable*. Modern men also mingle and walk among their gods and find themselves in mortal combat with the mythical forces of Nature and History or The People and The Nation. These are Durkheimian representations of the corporate reality of modern societies. The more corporate reality that is present, the stronger, more clearly defined and more closely merged with everyday reality are those symbolic representations which mirror that corporate reality. Daily life becomes filled with transcendent political significance and, simultaneously, the enemies of the sacred purposes. The ritual creation of oppositions to representations of corporate social reality is one of the fundamental forms of the modern religious life.

Appendix 1. Total Number of Witch-Hunting Activities by Institutional Area

Party System and Country	Total Activities	Activities by Institutional Area							
		Total ^a	Government	Military	Education	Economy	Intellectuals	Religion	Foreigners ^b
One-Party									
Total	3347	2076	549	93	233	119	225	455	402
China	532	318	88	20	23	18	19	107	43
Czechoslovakia ^c	475	300	95	7	16	22	27	57	76
Poland	406	307	57	9	43	6	49	95	48
U.S.S.R.	405	288	65	15	15	27	21	39	106
East Germany	324	169	55	3	42	13	3	45	8
Hungary ^e	316	195	43	13	13	8	23	49	46
Yugoslavia	242	159	52	5	16	3	36	27	20
Spain	197	108	3	1	34	14	31	19	6
Bulgaria	109	66	27	4	9	2	3	6	15
Rumania	94	51	27	1	1	0	2	8	12
Ghana ^b	90	45	15	8	4	4	6	1	7
Portugal	62	27	0	5	14	2	5	1	0
Albania	52	20	11	1	1	0	0	1	6
Senegal ^c	15	5	3	0	0	0	0	0	2
Guinea ^b	10	6	1	0	0	0	0	0	5
Tanzania ^d	9	8	3	1	2	0	0	1	1
Chad ^c	6	4	3	0	0	0	0	0	1
Ivory Coast ^c	3	1	1	0	0	0	0	0	0
Two-Party									
Total	1267	641	212	124	78	42	41	12	132
United States	1040	507	166	99	70	38	35	11	88
Great Britain	123	87	37	21	3	2	2	0	22
Philippines	34	20	6	2	3	2	0	1	6
Australia	29	6	0	0	1	0	1	0	4
Austria ^a	25	15	1	0	1	0	3	0	10
Colombia ^b	14	5	2	2	0	0	0	0	1
New Zealand	2	1	0	0	0	0	0	0	1
Multi-Party									
Total	826	360	54	54	33	13	46	7	153
France	275	112	23	20	8	3	27	2	29
West Germany ^a	157	53	13	1	9	5	6	1	18
Italy	103	43	4	6	4	1	6	0	22
Sweden	64	29	1	7	1	4	0	0	16
Canada	51	12	2	0	3	0	2	0	5
Switzerland	49	30	1	1	2	0	0	0	26
Netherlands	25	24	2	3	3	0	1	0	15
Finland	24	18	2	2	1	0	0	4	9
Norway	22	12	0	6	0	0	0	0	6
Denmark	20	15	1	7	2	0	2	0	3
Belgium	20	7	1	1	0	0	1	0	4
Ireland	15	5	4	0	0	0	1	0	0
Iceland	1	0	0	0	0	0	0	0	0
Luxembourg	0	0	0	0	0	0	0	0	0

^a 1956 and later.^b 1958 and later.^c 1950 and later.^d 1964 and later.^e Not all activities were described completely enough to permit assignment to an institutional area.^f Because of foreign occupation, not coded during 1968.^g Because of foreign occupation, not coded during 1956.^h Combined with category agents of foreign governments.

Appendix 2. Correlation Coefficients among the Variables Party System, Internal Security Forces, Population, per Capital GNP and Log Witch-Hunting Activities per Year (N=34)

Variables		Variables				Mean	S.D.
		PARTY	SECFOR	POP	GNP		
Political Party System	PARTY	1.00				2.21	.91
Internal Security Forces	SECFOR	.202	1.00			21.62	22.74
Population	POP	.149	.446	1.00		44.69	114.16
Per Capita GNP	GNP	-.701	.055	-.056	1.00	829.27	672.16
Log Witch-Hunting Activities per Year	LWHA	.283	.314	.446	.055	5.81	1.40

Appendix 3. Correlation Coefficients among the Variables Party System, Internal Security Forces, Population, per Capital GNP and Witch-Hunting Dispersion Index (N=25)

Variables		Variables				Mean	S.D.
		PARTY	SECFOR	POP	GNP		
Political Party System	PARTY	1.00				2.16	.94
Internal Security Forces	SECFOR	.393	1.00			36.44	24.10
Population	POP	.195	.212	1.00		58.88	130.85
Per Capita GNP	GNP	-.694	-.283	-.119	1.00	907.16	673.58
Log Witch-Hunting Dispersion Index	WHDI	.460	-.038	.205	.294	.852	.075

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SOCIAL REFORM ORGANIZATIONS AND SUBSEQUENT CAREERS OF PARTICIPANTS: A FOLLOW-UP STUDY OF EARLY PARTICIPANTS IN THE OEO LEGAL SERVICES PROGRAM*

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This paper considers the extent to which participation as a salaried professional in a reform-oriented organization affects the participant's subsequent career. This issue is studied in the context of one such organization, the OEO sponsored Legal Services Program, which was probably the largest and best known organization oriented to the redistribution of professional services in the late 1960s. Because of the paucity of literature on the consequences of participation in reform organizations, a related literature, that of the consequences of participation in the student movement of the sixties, is drawn upon for insight, yet also critically examined.

Comparison of the subsequent careers of 228 lawyers in Legal Services in 1967 to those of 981 other lawyers who were practicing law in 1967 indicates that participation in the program has an important effect on both the distribution of professional services and the rendering of reform-oriented pro bono (free or reduced fee) work. In contrast to previous studies, the explanation offered here differentiates between various components of socialization. In addition, the importance of job market factors is stressed. A further difference from previous work is the consideration, albeit brief, of the effects of variation in experience in the organization.

In the study of social reform movements and organizations, a good deal of attention has been paid to the

characteristics of participants at the time of entry, but relatively little to the effects of participation on the subsequent careers of participants. There are many reasons for this; most obviously, current participants are relatively easy to locate, while former participants are not. In addition, much of the literature on participation relates to the social reform activities of the 1960s, for which it is only now practical to collect follow-up data.

The activism of the 1960s was most evident among college youth; hence, there is

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a large literature on the characteristics of participants in the student movement,¹ and there is now a small literature on the subsequent activities of these activists (Fendrich, 1974; Fendrich and Krauss, 1975; Krauss, 1974; Demerath et al., 1971; Greene, 1970; Maidenbergh and Meyer, 1970).² This literature on the subsequent attitudes and activities of college activists is rather eclectic and is based on small samples, but it is generally consistent in indicating that former student activists have retained relatively radical attitudes, have generally continued to participate in protests (although not as actively as in the sixties) and have tended to avoid business-oriented careers in favor of jobs in the knowledge and human services sectors. Taken together, the studies tend to disconfirm the maturation hypothesis, which holds that activists outgrow their radicalism; they also are inconsistent with the hypothesis that radicals become disillusioned with society after being thwarted in their attempts at radical social change.³ (For discussion of these hypotheses, see Fendrich, 1974 or Krauss, 1974.) However, as discussed later, this literature has been less successful in dealing with the question of whether the subsequent careers are a result of experiences in the movement or of preexisting ideology, and it has not dealt adequately with the process through which movement experiences may have affected subsequent careers.

In addition to the very visible reform *movements* like the student movement, the sixties also were marked by the formation of many reform-oriented *organizations* that offered full-time employment in jobs having a direct impact on the situa-

tion of relatively powerless groups. Such organizations include Vista and the Peace Corps, as well as several offering reform-oriented variations of traditional professional careers (see, e. g., Gross and Osterman, 1972; Borosage et al., 1970). This paper analyzes the subsequent careers of early participants in one such organization, the Legal Services Program sponsored by the Office of Economic Opportunity. The primary question is whether such reform-oriented programs are essentially transitory experiences, with participants simply taking time out to "do good" for the indigent citizen or, alternatively, whether they can act as channelling mechanisms, substantially redirecting the careers of professionals who pass through them and thus effecting a redistribution of professional services in society.

THE ORGANIZATION

Although the 1960s marked a spurt in reform-oriented efforts in both the public and private sectors of the legal profession, the Legal Services Program has been the mainstay of the day-to-day efforts to deal with the legal needs of underrepresented citizens (Moonan and Goldstein, 1972). Legal Services was formed in 1965 under the auspices of the Office of Economic Opportunity, but the first full year in which it had a large number of programs operating was 1967, when it included about 1,200 lawyers.⁴ From the beginning, the program has tried to shift the balance of power in the legal system in two ways (Finman, 1971; Griffin, 1967; Stumpf, 1968). First, it has tried to increase the power of indigent citizens by pursuing a strategy of "test case" litigation. Suits have been brought—often against government agencies—in the name of a client but with implications for all people in a similar situation (see, especially, Cahn and Cahn, 1964). Some major cases of this type have been won by Legal Services lawyers (see, e.g., Miller, 1973), and these cases have been the source of much polit-

¹ There are several reviews and interpretations of this literature, including Lipset (1968) and Lipset and Altbach (1966). A few of the more important individual works are Flacks (1967), Keniston (1968) and the critique by Finney (1971).

² All of the studies cited are of the U.S. student movement, except that of Krauss, which is on Japanese activists. Krauss' study contradicts a variety of oft-quoted journalistic accounts, some of which have found their way into the sociological literature.

³ Of course, these dropouts would be very unlikely to be reached in a sample survey. See, for example, the discussion in Carey (1968).

⁴ The history of the Legal Services Program is discussed in more detail in a separate publication (Handler et al., forthcoming).

ical opposition to the program.⁵ The second strategy, and the one which has been dominant in terms of time spent per lawyer, has been that of representing indigent citizens in individual matters without special attention to broader consequences of the case. This work has had an impact simply because of the increased representation of people who previously were relatively helpless against those individuals and agencies with access to the legal system.

METHOD

Analysis in this paper focuses on the differences between 1967 Legal Services lawyers and the other lawyers in the bar in terms of job characteristics six years later.⁶ These characteristics include type of practice setting, type of client served and, for lawyers in private practice, the rendering of reform-oriented *pro bono* (free or reduced fee) work. The data for Legal Services lawyers are from a stratified random sample of all known participants in the program in 1967, interviewed in the fall of 1973.⁷ The responses from the strata are weighted to correspond to the estimated true distribution in the population. The sample is biased (to an unknown degree) in that it underrepresents persons with short tenure in the program, persons who have dropped out of the legal profes-

sion since leaving Legal Services, and persons who are too mobile to be located in spite of our extensive inquiries through a variety of sources.

Characteristics of Legal Services lawyers are compared to those of a stratified random sample of practicing lawyers, interviewed in fall or spring, 1973-74.⁸ This sample was drawn primarily from the Martindale-Hubbell Directory of Lawyers (1972) but, since there is evidence (Ladinsky, 1964) that this directory underrepresents solo practitioners, lawyers not in private practice and, especially young lawyers, a supplementary sample was drawn from other sources. This supplementary sample was constructed by first selecting fifteen states at random (with the probability of being chosen conditional on the number of lawyers in the state) and then sampling from the most complete list of lawyers available for that state. Responses are pooled and weighted to correct for the various sampling ratios employed. Lawyers who retired before 1967, who received law degrees after 1967, or whose 1967 job was in Legal Services are dropped from this control group.

This paper reports on 228 white male Legal Services lawyers and 981 white males in the bar. Blacks and women are excluded from the analysis because of their small sample size, despite the fact that each comprised about an eighth of the participants in Legal Services in 1967 (Erlanger, 1976). (White males, black males and white women who were in Legal Services all appear to have quite different distributions of 1973 job. See footnote 16.) Because of the restriction to white males, the pronoun "he" is used in this paper.

CRITICAL ISSUES IN THE STUDY OF THE EFFECTS OF PARTICIPATION IN SOCIAL REFORM ORGANIZATIONS

Given the small empirical literature on the consequences of participation in reform organizations (Zald and McCarthy, 1975), the literature on the student movement is the most relevant guide to an inquiry on this subject.

⁵ Such opposition contributed to the removal of the program from the OEO and the creation of an independent Legal Services Corporation to oversee it (Handler et al., forthcoming; Hiestand, 1970).

⁶ The target job is the predominant job held at the time of the interview; this is not necessarily a change from the job held in 1967. Many lawyers hold more than one job simultaneously (one of our respondents reported holding four), and the predominant job is defined as the one in which the respondent spent 60 percent or more of his time or earned 60 percent or more of his income in the preceding year. Ninety-five percent of all respondents having more than one job could be classified in this way. For the other five percent, a predominant job was designated on the basis of time and income shared among jobs. Analysis indicates that concentration on the predominant job does not affect the conclusions drawn in this paper.

⁷ Sampling and weighting procedures are discussed in greater detail in Handler et al. (forthcoming). All interviews were conducted by telephone by the staff of the Wisconsin Survey Research Laboratory. The average interview lasted over an hour.

⁸ Interviewing procedure was the same as for the Legal Services lawyers.

The Problem of Self-Selection

Other than small sample size, the most obvious problem in the literature on the effects of participation in the student movement is the absence of controls for factors predating participation. This problem is due primarily to the journalistic nature of many of the studies, although it also appears in the only book length scholarly study (Krauss, 1974).⁹ In the study of the effects of student activism, Fendrich's (e.g., 1974; Fendrich and Krauss, 1975) papers on subsequent political attitudes and behavior are the only ones that systematically use control variables.

Since people are not randomly assigned to participation in reform movements, the problem of self-selection can never be fully dealt with,¹⁰ and there is always the possibility that some unobserved variable renders the observed relationships spurious even when control variables are used.¹¹ Some control variables such as college major or prior political participation, along with the participation and its apparent effects, may be consequences of an unobserved variable such as "orientation to social reform." It is necessary, therefore, to control for variables that occur prior to the unobserved variable—in this case, variables such as religion of family of origin, parents' SES or parents' politics. If these controls do not substantially reduce the relation between participation and subsequent activity, then the critic is obliged to suggest an unobserved variable that is substantially independent

of such parental characteristics. In the prior literature, parental characteristics have not been controlled; appropriate variables are, however, included in the present analysis. In addition, self-selection is less of a factor in reform organizations offering salaried positions, because the factors leading to participation are more varied. Thus, compared to a more explicitly political movement, a reform organization does not begin with as committed a group of participants.

Conceptualization of the Problem at Issue

The major multivariate analyses of the effects of student activism have been framed in terms of the question, "How much variance in current political attitudes or political behavior can be explained by prior student activism, net of controls?" (Fendrich, 1974; Fendrich and Krauss, 1975). The difficulty with this conceptualization is that a variable like student activism or participation in Legal Services cannot explain any meaningful part of the variance in current attitudes or behaviors (measured for a whole population), because the rate of activism or participation is so low.¹² For example, in 1967 less than one-half of one percent of the bar was in Legal Services. For just this reason, researchers studying the effects of participation sample the participants and the control group separately.

Now if one has heavily oversampled one group, one must decide how to weight the responses. With no correction for the different sampling ratios, variance is artificially created on the oversampled variable, and the variance is changed on all of the variables correlated with it. As an example of the problem, consider the effects of different weights on the variance explained in the "status of practice" of lawyers engaged in private practice. (This variable, which indicates success by traditional criteria, will be defined and discussed in detail below.) In Example 1 of Table 1, responses of Legal Services lawyers are weighted so that they consti-

⁹ This problem is remedied to a certain extent in Krauss' more recent work (Fendrich and Krauss, 1975).

¹⁰ In the study of the effects of particular programs, there is occasionally a way to do a more controlled study. If, due to a lack of positions, the program turned away candidates that otherwise it would have taken, and if those turned away are initially similar to those accepted, then a good control group is generated.

¹¹ See the discussions of regression artifacts in Campbell and Erlebacher (1970), Riecken and Boruch (1974: 174ff) or Weiss (1972). Goldberger (1972) has, however, formally set out at least one plausible model in which regression artifacts would not occur. For a more detailed discussion of regression artifacts and other issues in the evaluation of social programs, see the papers in Bernstein (1975).

¹² For discussion of other limitations of approaches which seek to explain the variance, see Duncan (1975:63-6).

Table 1. Variance Explained by Independent Variables under Different Weighting Schemes^a

Independent Variables	Legal Services Lawyers, as Percent of Total Sample		
	Example 1 0.3%	Example 2 13%	Example 3 50%
(1) Participation in Legal Services	R ² = 0%	R ² = 7%	R ² =17%
(2) Participation in Legal Services Plus Eight Controls, Entered as Groups of Dummy Variables	R ² =11%	R ² =15%	R ² =23%

^a White males in private practice only; N=713. Dependent variable is "status of practice" index. R² corrected for degrees of freedom.

tute 0.3% of the lawyers in private practice; in example 2, no special weights are applied,¹³ and the difference in sampling ratios results in Legal Services lawyers comprising about 13% of the lawyers; in example 3, Legal Services lawyers are reweighted so that they comprise 50% of the lawyers. As the table clearly shows, the weights make an important difference. Moreover, only example 1 accurately estimates the variance explained by participation in Legal Services.

The variation in the R² depending on the weighting scheme does not necessarily mean that the R² must be totally ignored when certain variables are artificially skewed, as an examination of R²s can serve a heuristic function. However, it does seem to be incorrect to define the task at hand as the determination of the contribution of the participation variable to the total variance explained in the dependent variable. An alternative approach to conceptualizing the research issue when participants have been oversampled, and the one which will be used in this paper, is to examine the unstandardized regression coefficients to see (1) whether participation has a substantively meaningful effect on the dependent variables and (2) whether control variables have an effect on those raw coefficients. Unlike standardized coefficients, unstandardized ones do not vary with the different weighting schemes, except for rounding error.

Varieties of Experience

The literature on student activism has not explored in any detail the possibility that the effects of participation may vary with differences in the activist experience. For example, the leaders of the various organizations, marches, etc. presumably had a much deeper involvement than most followers, and both leaders and followers varied in the length of time they were active. Further, participants differed in the types of activities they engaged in (especially legal versus illegal), the types of issues they addressed, the degree of contact with the police and the degree of official sanction (if any) they experienced. Similarly, in Legal Services, participants differed on such variables as the extent of direct contact with the poor, the types of cases dealt with, and the courts in which they appeared.¹⁴ Thus, an important issue is the extent to which the Legal Services experience in general, rather than the specific varieties of it, affects the individual participants.

Process through which Participation Affects Subsequent Attitudes and Behavior

Previous literature has assumed that if it can be shown, first, that former activists are different from others in their cohort and, second, that these differences are not spurious, then the cause of the correlation between participation and subsequent at-

¹³ As discussed above, Legal Services participants and nonparticipants were sampled separately using a stratified design and unequal sampling ratios. Weights to correct for this design were retained in the analysis.

¹⁴ Except for Table 2, all analysis is restricted to lawyers who left the program. However it should be noted that the program is flourishing today in much the same form as in 1967 (Handler et al., forthcoming).

titudes or behavior is socialization in the movement. However, comments by informants in the present study indicate that elaboration of the process of socialization is necessary and that when occupational careers are considered, additional mechanisms are clearly relevant. These additional mechanisms include networks yielding information about potential jobs and clients and employer preferences in hiring. It seems plausible that similar processes have affected former student activists.¹⁵

THE SUBSEQUENT CAREERS OF LEGAL SERVICES LAWYERS

Since the Legal Services Program has been funded continuously for over ten years and since it offers permanent employment, it is possible that most of the early participants are still there. However, there has been a rather high rate of turnover (Erlanger, 1976). A third of the (white male) lawyers in Legal Services in 1967 left before the end of their third year, another quarter left by their fifth year, and only 29% were still there when our data were collected in fall, 1973. Moreover, the data indicate that it is the lawyers with prior involvement in social reform activity, those working in the reputedly "excellent" offices, and those from more liberal backgrounds who tend to leave earlier (Erlanger, 1976). The first issue for analysis is, then, where did they go?

Type of Practice

Compared to nonparticipating lawyers, lawyers who participated in Legal Services in the early years had quite different jobs in 1973. As Table 2A shows, former Legal Services lawyers are less likely to be in private practice.¹⁶ Historically, the

overwhelming majority of lawyers have been in private practice (Blaustein and Porter, 1954); virtually all well-known lawyers spend most of their careers in private practice (although they will, on occasion, put in a period of high-level government service); and private practice certainly is potentially the most lucrative form of practice.

However, the difference in propensity to be in private practice is only a small part of the story. More fundamentally, whether in private practice or not, former Legal Services lawyers serve a different type of client and do a different type of work. Tables 2B and 2C show this clearly.¹⁷ Eighty-six percent of former Legal Services lawyers in private practice (compared to 64% of the bar) are either in solo practice or in very small firms and, consequently, deal almost exclusively with the affairs of individuals with low or moderate incomes and of relatively small businesses (Handler et al., forthcoming). Only a small percentage of former Legal Services lawyers are in firms of ten or more members, and none in our sample has moved into the major firms that often

9%; firm of 2-4 members, 9%; firm of 5-9 members, 4%; counsel for business corporation, 6%; university faculty, 14%; activist government agency, 5%; legal rights job, 1%; other salaried job, 1%; non-law job, 14%; retired or unemployed, 1%. Note that percentages based on such a small N are unstable, especially when the data are weighted. The control group sample yielded only eight nonwhite lawyers, too small an N for analysis.

The job distribution for white women also is based on small Ns, 22 for the Legal Services lawyers and 28 for the bar. For the list that follows, the Legal Services percent is shown first, followed by a slash and then the control group percent. Unlike the comparisons for white males shown in Table 2, the data here are not standardized by year of graduation. Same Legal Services job, 8%/-; different Legal Services job, 2%/0%; solo practice, 42%/15%; firm of 2-4 members, 2%/16%; firm of ten or more members, 2%/6%; counsel for business corporation, 1%/4%; university faculty, 4%/11%; other salaried job, 6%/20%; non-law job, 0%/11%; retired or unemployed, 33%/16%.

¹⁷ Preliminary analysis of the data using controls for year of graduation revealed no important interaction effects. However, since the former Legal Services lawyers are much younger than the control group (Erlanger, 1976), for all percentages shown (in text and tables) the year of graduation of lawyers in the control group has been standardized so that the distribution of that group matches that of the Legal Services lawyers.

¹⁵ The characteristic most likely to affect employer judgments of a former student activist is probably the existence of an arrest record, which in some quarters was virtually a membership badge in the movement. Business employers are much more likely than those in the human services sector to reject an applicant solely on those grounds.

¹⁶ The current positions of the twenty-nine male nonwhite lawyers who were in Legal Services in 1967 is as follows: in same Legal Services job, 33%; in different Legal Services job, 2%; solo practice,

Table 2. Jobs Held, 1973-1974 ^a

	Legal Services Lawyers	Lawyers in Bar
A. Type of Practice		
Same Legal Services Job as 1967	22% (41)	... ^b
Other Legal Services Job	7% (14)	0% (1)
Private Practice of Law	40% (92)	67% (621)
Other Jobs	31% (77)	31% (305)
Retired; Unemployed	1% (4)	2% (54)
	101% (228)	100% (981)
B. Distribution within Private Practice		
Solo Practice	43	27
Small Firm (2-4 lawyers)	43	37
Medium Firm (5-9 lawyers)	10	17
Larger Firm (10+ lawyers)	4	18
	100%	99%
(N)	(92)	(621)
C. Distribution of Other Jobs		
Non-Law Job	13	30
Salaried Counsel for Business	3	29
Legal Rights Job	15	1
Activist Government Agency	16	2
Other Salaried Counsel (Mostly Government Agencies)	43	31
University Faculty	10	7
	100%	100%
(N)	(77)	(305)

^a White males only. Distribution of year of graduation of lawyers in bar standardized to that of Legal Services lawyers. Responses weighted to correct for stratified sampling; unweighted Ns are shown.

^b Not applicable to lawyers in bar.

have over 100 lawyers and handle the affairs of the major corporations and of wealthy individuals (Smigel, 1969). By contrast, 18% of the bar are in firms with ten or more lawyers (Table 2B).

Similarly, former Legal Services lawyers who are not in private practice are heavily concentrated in nonbusiness pursuits. Fifteen percent of these lawyers have remained in "legal rights" work (Borsage et al., 1970; Marks, 1972), with a public defender's office, a public interest law firm, a social reform-oriented foundation or similar type of organization; another 16% work for an activist government agency, such as the Civil Rights Division of the Department of Justice. Ten percent are employed primarily as law professors, usually teaching at least one nontraditional course such as welfare law, consumer protection, etc. Forty-three percent are in miscellaneous salaried posi-

tions, mostly in federal, state and local government, while only 16% are staff counsel for a business corporation or working in a non-law (usually business) job. As Table 2C shows, this distribution is very different in the bar, for which 60% of lawyers not in private practice work either in a non-law job or as a salaried counsel to a business corporation. (The percent of law faculty is roughly the same in the bar, but further analysis indicates that the courses taught are different.)

Further Differences within Private Practice

Firm size often is used as an indicator of the type of work engaged in by a lawyer in private practice (Carlin, 1966; Ladinsky, 1963), but it is really a proxy for several aspects of practice such as type of clients (business rather than individuals relatively wealthy businesses and individu-

als), income, type of courts appeared in (federal and state appeals, rather than lower state and county courts), physical work setting (large firms generally have posh offices and are located in the "better" parts of town), types of cases (contracts, trusts, tax, etc. as opposed to matrimonial, criminal or personal injury); etc.

In order to incorporate more of the variables which traditionally have been seen by both lay persons and lawyers as defining a lawyer's status, we have developed a "status of practice" index based on firm size, income level of business and individual clients and professional income.¹⁸ Table 3 shows a sharp differentiation between former Legal Services lawyers and other lawyers in private practice when these traditional criteria of professional status are used. Although actual scores on the "status of practice" index range from 0 to 13, half of the former Legal Services lawyers, as compared to only 19% of other lawyers in private practice, score 0, 1 or 2. Similarly, only 5% of Legal Ser-

¹⁸ The status of practice index is the sum of scores on three separate indices, each of which was first converted to a z-score based on the distribution for the control group. The three subindices were: (1) log of firm size (solo practice = firm size of one), (2) income from the practice of law during the preceding year (corrected for multiple jobs) and (3) a "status of clients" scale, based on the income of business and individual clients, weighted by the proportion of time spent on each type of client. A score of three on one of these subindices indicates that the lawyer's raw score was within one standard deviation of the control group mean on that index. One point was then added or subtracted for each standard deviation above or below the mean in which the respondent's score fell, up to a maximum of three points. A score of zero on a subindex thus indicates that the score was three standard deviations below the mean, a score of six indicates that it was three standard deviations above the mean. Since the distributions on all the subindices were heavily skewed to the bottom of the scale, the z-score technique is technically not appropriate. However it was used because it generated a score which could meaningfully be added to those on the other subindices. Experimentation with different scoring methods and analysis of the subindices independently did not affect the findings. The index, which is similar to those used in previous work (Carlin, 1966; Handler, 1967), is discussed in more detail in Handler, et al. (forthcoming). One advantage of such an index is that it allows for the situation in which one or a few lawyers practice on a small scale numerically, but on a large scale in terms of impact (see, e.g., Goulden, 1972; Green, 1975).

Table 3. Score on "Status of Practice" Index *

Score on Index (range 0-18)	Cumulative Percentage	
	Former Legal Services Lawyers	Lawyers in Bar
Less than 2	12	7
Less than 3	50	19
Less than 4	78	37
Less than 5	89	56
Less than 6	95	72
Less than 7	96	83
Less than 9	100	94
Less than 14	100	100
(N)	(92)	(621)

* White males in private practice only. Distribution of year of graduation of lawyers in bar standardized to that of former Legal Services lawyers.

vices lawyers, as compared to 28% of others, score six or more. Since the Legal Services lawyers have social and educational backgrounds comparable to lawyers in the bar (Erlanger, 1976), they would be expected to have similar practices. Differences of this magnitude in the kind of practice taken up thus, in essence, represent a redistribution of legal services away from the upper-middle and upper classes toward the middle, lower-middle and lower classes.

A second effect of Legal Services on the type of subsequent private practice is the extent to which clients of minority backgrounds are served. Former Legal Services lawyers are much more likely to have a large percentage of minority (primarily black) clients. Data standardized by year of graduation show that close to half (45%) of the lawyers in private practice have only a few clients from minority groups; for former Legal Services lawyers, the figure is 26%. Similarly, only 12% of the private practice bar, as compared to 29% of former Legal Services lawyers, have a practice in which a third or more of their clients are members of minority groups.

A third effect is on type of *pro bono* work done by lawyers. The obligation to do *pro bono* work has long been part of the ethic of the legal profession, but historically the extent and range of this type of work has been quite limited (Carlin et

al., 1966; Marks, 1972). Even today the average lawyer spends a rather modest part of his or her time on *pro bono* work, generally performs this work for traditional clients such as relatives, friends of clients, church groups, charities and indigent individuals, and generally uses a case-by-case approach, drafting legal documents, rendering advice and appearing in criminal courts (Handler et al., 1975; Lochner, 1975). Our data indicate that former Legal Services lawyers are more likely than other lawyers to do *pro bono* work; but, most important, they are more likely to take on clients and cases oriented toward law reform rather than individual adjudication—for example, cases involving welfare rights or consumer issues, or clients who are migrant farm workers or inmates of mental hospitals. Data standardized by year of graduation show that in the year prior to being interviewed, 42% of former Legal Services lawyers, as compared to 27% of other lawyers in private practice, did *pro bono* work oriented to social reform (Handler et al., forthcoming).

Tests for Spuriousness

In a quasi-experimental design, spuriousness can never be totally ruled out; however, as discussed earlier, the present analysis is strengthened by the presence of indicators of the political and social reformist orientations of the respondent's family of origin. These variables include father's political stance, parents' participation in social reform organizations, mother's religion and father's occupation. (Several other indicators also were available, but they did not affect the findings reported below.) For the respondent himself, data were collected on prior political activity, year of graduation from law school and educational characteristics (type of law school attended, class standing), all of which may be expected to affect the type of practice a lawyer enters. Although the relationships among these control variables as well as the direct and indirect effects of these variables on current job would be of some substantive interest, these considerations are not rel-

evant to the task at hand.¹⁹ Rather, this analysis seeks only to gauge the effect of participation in Legal Services on the lawyer's subsequent career, net of the effect of these controls. Hence, in the analysis of spuriousness, control variables were, for the most part,²⁰ entered into the model in one group. The presentation here will deal only with lawyers in private practice. Analysis of the entire sample, using other dependent variables, yielded similar conclusions.

Comparison of rows (a) and (b) in Table 4 shows the simultaneous effect of all controls except prior job on the three characteristics of private practice previously discussed.²¹ The table indicates that these control variables have little effect on the tendency for Legal Services lawyers to avoid those types of practice which traditionally have been accorded high status and to do reform-oriented *pro bono* work.

¹⁹ One important finding which emerges is that in this national data set, social background variables do not have nearly as strong an effect on status of practice as would be expected from earlier studies in individual cities (e.g., Carlin, 1962; 1966; Ladinsky, 1963; 1967).

²⁰ When Legal Services lawyers are compared to the bar as a whole, prior job is omitted. For reasons explained below, prior job is added to the other controls only after lawyers with stable job histories are dropped from the control group.

²¹ The regressions were run with weighted data to correct for disproportionate sampling ratios for strata within the Legal Services sample and within the bar sample. However, the most efficient use of the available data dictated no further use of weights to correct for the oversampling of Legal Services participants.

Tables 4 and 5 are based on dummy variable regression (multiple classification analysis), a variant of the general linear model (Andrews et al., 1967; Cohen, 1968; Melichar, 1965). A global test for interactions in which each category of each control variable was interacted with the variable "participation in Legal Services" indicated that, for each dependent variable, addition of the interaction terms did not significantly increase the corrected R^2 .

For the dichotomous variable "does social reform *pro bono* work," analysis also was done using a logit model (Theil, 1971), a log linear model which yields more accurate estimates than regression when the dependent variable is dichotomous. A maximum likelihood estimation procedure was used. This analysis yielded stronger findings than those reported in the text for the regression analysis; the Legal Services effect was actually somewhat greater with the control variables than without. The regression findings are presented in the tables because they are more easily interpreted.

Table 4. Effect of Participation in the Legal Services Program on Characteristics of Subsequent Private Practice I^a

	Dependent Variables		
	Status of Practice (range 0-18)	Percent Minority Clients (range 0-100)	Does Social Reform <i>Pro Bono</i> Work (range 0-1)
Mean Score, Lawyers in Bar	5.0	14	.24
Mean Score, Former Legal Services Lawyers			
a) Without controls	3.2	25	.42
b) Controlling for year of graduation and background and educational variables ^b	3.3	23	.40
c) Controlling for variables above, ^b plus status of practice index	...	21	.39

^a White males only; N=713.^b Variables entered as sets of dummy variables in a multiple classification analysis.

Percent minority clients is reduced somewhat more than the other variables, and an additional control for status of practice reduces it further. At this point, however, the difference between participants and nonparticipants in the percent minority clients is still statistically significant ($p < .005$).²² The Legal Services effect for the status of practice and *pro bono* variables seems, then, to operate primarily in an additive fashion to these control variables.

Let us now consider the effect of control for prior job on the findings for the three characteristics of private practice. Prior job is a critical control variable, since about three-fourths of the Legal Services lawyers came from another job, and one would expect the contacts and experiences of the prior job to influence the type of job held subsequently. To analyze the influence of prior job, the most appropriate control group to which Legal Services lawyers can be compared is one composed of other lawyers who changed jobs during 1965-68, a period roughly equivalent with the span of time

in which Legal Services lawyers joined the program. Since lawyers are rarely fired and tend, once established, to stay in a job for a long period of time, most lawyers are not "in the market" and their careers cannot appropriately be compared to those of the mobile lawyers.²³

The job held by a lawyer prior to making a job shift in the period 1965-1968 is a relatively good predictor of the job held in 1973-74. Lawyers who were in solo practice in the earlier period are more likely to be in solo practice or with a small firm in the later period; lawyers who were in larger firms are more likely to be in relatively large firms; etc. But the important point for the analysis here is that for two of the dependent variables (status of practice and reform-oriented *pro bono* work), the effect of participation in Legal Services is independent of the control variables (Table 5). However, when background, status of practice and prior job are all controlled, the difference in the percent of clients who are members of minority groups is no longer statistically significant (Table 5). The observed difference in percent minority clients is thus partly attributable to differences in background, education and prior job, and partly attributable to an indirect effect associated with the type of clientele served.

²² Fractional weights were used, so the significance level is not affected by artificial increase in sample size. However, the significance level is not as accurate as with a true random sample because the weighting scheme used sacrifices some efficiency in sample design. Because of the relatively large N, rather small coefficients are statistically significant. Hence, significance levels are not shown in the tables but, for small coefficients, are reported in the text for reference by the interested reader.

²³ Perhaps the findings would be more precise if various control groups were used, depending on the number of job changes since the early 1960s. Sample size does not permit this procedure.

Table 5. Effect of Participation in the Legal Services Program on Characteristics of Subsequent Private Practice II^a

	Dependent Variables		
	Status of Practice (range 0-18)	Percent Minority Clients (range 0-100)	Does Social Reform <i>Pro Bono</i> Work (range 0-1)
Mean Score, Lawyers in Bar	4.6	17	.24
Mean Score, Former Legal Services Lawyers			
a) Without controls	3.0	27	.44
b) Controlling for year of graduation and background and educational variables ^b	2.9	23	.43
c) Controlling for variables above, plus job left in 1965-68 ^b	3.0	23	.44
d) Controlling for all of the above, ^b plus status of practice index	...	19	.41

^a White males only; N=210. Legal Services lawyers are those who had a prior job; lawyers in bar are those who changed jobs in the period 1965-68.

^b Variables entered as sets of dummy variables in a multiple classification analysis.

DISCUSSION AND CONCLUSIONS

The Effects of Participation

Given the limits of quasi-experimental design, the conclusions here must be tentative; but, within these limitations, the data for white male Legal Services lawyers clearly indicate that participation in the program leads to a redistribution of service among lawyers in private practice.²⁴ Former Legal Services lawyers in private practice tend to avoid the more prestigious types of practice (as measured by the traditional criteria of type of clients, type of work setting and professional income) and they are more likely to do *pro bono* work oriented to social reform than are other lawyers of comparable background and experience. Former Legal Services lawyers who do not go into private practice also have quite different careers than their counterparts in the bar; most especially, they seem to shun corporate counsel and nonlaw jobs.

The subsequent career of the Legal Services lawyer, much like that of any worker, is affected by a mix of socialization and job market factors. The following

seem particularly important for the Legal Services lawyer. First, socialization of attitudes doubtless plays a role. In Legal Services, the lawyer learns to view the problems of the "little guy" as important, or, already holding this view, has it reinforced. But a focus on attitudes alone overlooks other important processes. A second component of socialization experienced by the Legal Services lawyer is training in the skills relevant to the legal problems of poor people, with the relative absence of training in legal skills relevant to corporations and wealthy individuals. The Legal Services lawyer, in much the same way as other lawyers who have worked in a particular area, becomes a specialist of sorts and, thus, has limited attractiveness to some potential clients, employers or colleagues and enhanced attractiveness to others.²⁵

Finally, we know from a variety of studies that job milieu has a strong effect on the type of subsequent job held.

²⁴ This is basically the pattern of subsequent jobs predicted in McCarthy and Zald's analyses (McCarthy and Zald, 1973; Zald and McCarthy, 1975) of recent trends in social reform movements.

²⁵ Major firm lawyers whom we have interviewed on this issue indicate that "bad training" is viewed as a serious problem. In addition, there is the problem of "lateral entry" to firms. Major firms find it preferable to bring in new lawyers at the bottom (i.e., fresh out of law school) or at a senior level; they do not like to bring in lawyers with a few years of experience not directly relevant to the firm's specialty.

Studies of many different occupations indicate that most jobs are obtained not through the "open market," but through information gained from personal acquaintances who have direct or indirect knowledge of the availability of a particular job.²⁶ (This does not mean that these acquaintances have influence with the potential employer but, rather, that they make a person aware that a good job exists.) Moreover, a substantial percentage of persons who change jobs are never directly in the job market, but hear of a job through informal channels at a time that is opportune for them. The reliance on informal processes is especially characteristic of lawyers, for whom formal recruitment mechanisms primarily exist only upon graduation from law school. And even lawyers practicing on their own are dependent on the same type of network for clients. The network of associations generated for a Legal Services lawyer, especially one working primarily on service cases, was very different than that of, say, a lawyer in a large firm or a lawyer working directly for a business corporation; and this very likely affected the subsequent jobs taken.

Emphasis on the socialization and job market effects of participation in Legal Services is not to deny that there could also be a self-selection effect. Such an effect would most likely operate through a prior orientation to social reform or through prior career choices. Granted, controls for these and related factors such as year of graduation, law school attended, etc. are imperfectly measured. But it is striking that together they have so little impact on the relationship between participation in Legal Services and key characteristics of the lawyer's subsequent job. Moreover, these conclusions are strengthened because of the inclusion of several indicators of characteristics of family of origin which one would expect to cause a reformist orientation. This is much preferable to a design which con-

trols only for factors which themselves may be consequences rather than causes of social reformism. As noted earlier, a critic of these conclusions must proffer an unmeasured self-selection variable which is so slightly related to those variables examined that this unmeasured variable could account for the effects found in this study.

In addition, it is important to note that the explanation presented here does not require an absence of self-selection. Rather, the argument is that, in the case of self-selection, socialization and especially the job market factors would reinforce the decision. In short, lawyers join Legal Services for a variety of reasons but, overall, participation tends to decrease the probability that they will pursue the traditional routes to success. The importance of program effects as opposed to self-selection effects also can be seen in a comparison of lawyers who stated different motives for joining the program. Legal Services lawyers gave three general types of reasons for joining—reasons which would appear to have an explicit social reform content ("I felt I had a duty to help the poor," "I wanted to work with these types of issues"); reasons which might include a social reform component (the types of cases would be "challenging," the job afforded "community contacts," etc.); and reasons which appear to include only personal factors (the desire to gain practical experience, the location of the office, the steady income). If the observed effects of participation in Legal Services were spurious, then one would expect that the effects would be strongest for those lawyers who report a reformist motivation for joining. However, there are no statistically significant differences between the three groups on the dependent variables. Moreover, insofar as there are differences, lawyers explicitly citing a social reform motivation for joining are actually somewhat *lower* than either of the other groups in both the percent minority clients served and the rendering of reform-oriented *pro bono* work.

Obviously, the argument here must be tentative, since no direct data on the socialization or job changing processes for Legal Services lawyers are available.

²⁶ See, for example, the studies reviewed by Granovetter (1974:5-6). There is no study of the job-finding process for lawyers, but studies of other professionals (e.g., Brown, 1965; Shapiro et al., 1965; Granovetter, 1974) are consistent with the description in the text.

What has been documented is a "black box" effect, and a series of processes, operating in a different mix for different people, are suggested to explain it. This is presented as an improvement over previous studies, which by and large have assumed that if participation had real effects, they must be the result of socialization of attitudes. In the future, these processes need to be sorted out and other processes hypothesized and investigated.

The Effect of Variation in Experience

One important consideration in the further analysis of the effects of participation in social reform organizations is the effect of variation in the experience of different participants. Probably the greatest difference in experience in Legal Services is between lawyers engaged in law reform and those doing service work. As noted earlier, from the outset Legal Services has had dual aims: the serving of the immediate needs of individual clients through direct adjudication of their cases and the changing of laws affecting the poor, primarily through class action or test case litigation. Those lawyers who spent a significant amount of time on law reform work experienced a very different milieu; in some local programs, they even worked out of a different office (Carlin, 1970).

A lawyer doing law reform work had substantially less direct contact with clients than did other Legal Services lawyers. Most of the time on this type of litigation is spent researching the law, preparing lengthy briefs, appearing before the court and, on occasion, preparing appeals to higher courts. In doing this work, the Legal Services lawyer may have undergone a different type of socialization than the lawyer doing service work exclusively. First, the political component of the work was different, being focused on change in the centers of power, rather than affecting a change for discrete individuals. Second, lawyers in this type of work might be expected to put a premium on meticulousness, skill in legal research and ability to draft complex briefs. For lawyers doing service work, the pressures were different. Our data indicate that

these lawyers carried very heavy caseloads, generally averaging over 100 open files at a time. With this type of workload a lawyer, no matter how conscientious, was in a situation in which every angle of a case could not be pursued and a great deal of attention could not be devoted to detail.

Different combinations of socialization, job information networks and employer preferences would seem to lead to different subsequent careers for lawyers engaged in the two types of work. One might expect differences in the type of salaried job taken, the type of client served in private practice, the extent to which minority clients are served, the extent of social reform-oriented *pro bono* work, etc. However, even with the rather large sample used in this study, examination of the effects of variation of experience is hampered by small sample size. For example, only 91 Legal Services lawyers in our sample went into private practice, which is an inadequate N for extensive analysis. Thus, this analysis must be tentative; but hopefully it will serve to suggest the importance of pursuing such differences in future research.

Only about 15% of Legal Services lawyers spent a majority of their time on law reform work, but half averaged at least one day a week. Analysis indicates that insofar as doing law reform work has an effect on subsequent career, this effect is for lawyers who did one day a week or more versus those who did less than one day a week. White male lawyers who were engaged in law reform work are less likely to be in private practice than those who did almost all service work (31% versus 49%); but, contrary to expectations, the particular non-private practice job taken does not seem to have been affected by the law reform experience. However, having done law reform work does affect the type of private practice engaged in. Lawyers who did law reform work are much more likely to be "successful" by traditional criteria. Controlling for all the variables discussed earlier (including prior job), the mean status of practice for these lawyers is 3.8, as compared to 2.8 for lawyers who did service work almost exclusively. But insofar as there are effects

of variation in experience within the organization, they are additive to the effect of the participation in the organization itself. The coefficients for both subgroups of Legal Services lawyers are still substantially different from those of lawyers who were not in Legal Services.²⁷

Consequences for the Redistribution of Professional Services

In recent years, there has been increasing concern about the distribution of legal services across the population. The services of lawyers are purchased by business corporations and affluent individuals disproportionately; not just the impoverished but also the vast middle class are underrepresented (see, e.g., Christensen, 1970; Mayhew and Reiss, 1969). This pattern also can be seen in the data from our survey, which shows that 53% of the individual clients of lawyers in private practice have incomes over \$15,000, while the census shows that only 16% of adult Americans have incomes of that amount.

The Legal Services Program directly generates a redistribution of the services of lawyers toward underrepresented groups through its rendering of service without fee on a massive scale to indigent citizens. But the analysis in this paper indicates that the program also has very important spillover effects. Legal Services lawyers apparently do not just take time off from other pursuits to work with the poor, nor are they just doing what they would have done if they had not participated. Rather, the program acts as a struc-

tural mechanism that helps to direct lawyers to a different kind of client and to different types of issues. Service to the indigent is expanded because of the greater amount of *pro bono* work performed by former Legal Services lawyers and because of their propensity to take clients and cases that challenge the status quo. Service to citizens with moderate incomes is expanded through the tendency of Legal Services lawyers to go into solo practice or into relatively small firms, which tend to serve this type of client rather than corporations or wealthy individuals. In addition, service to minority clients is increased as a concomitant of this different type of practice.

These findings indicate that the well-known "problem" of turnover in programs like Legal Services is not necessarily a problem at all. Once a cohort of professionals has stayed long enough to be affected by their participation (and analysis indicates that length of service in Legal Services is not an important predictor of subsequent practice), it may be desirable that they move out into the community and make room for a new cohort. The subtle redistribution in the type of recipients of professional services will not, in itself, be sufficient to correct the inequalities in the system, but it will be an important step in that direction.

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- ²⁷ There is no way of knowing the extent to which these findings can be generalized to the study of student activism. There have, however, been hints in the literature that differences in experience are relevant. For example, Greene (1970) reports his impression that the former leaders of the Berkeley FSM are more radical than the followers, although he has no control for self-selection. Krauss (1974) begins with a separation between leaders, activists and intermittent activists, but then has to collapse categories because of insufficient N. At least one study also has indicated that variation in the intensity of experience is important; in a study of students at Kent State, Adamek and Lewis (1973:347) concluded that, in spite of the problem of self-selection, there was evidence that the "extreme social control force applied by the National Guard [at Kent State] radicalized the students most directly involved."

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INSURGENCY OF THE POWERLESS: FARM WORKER MOVEMENTS (1946-1972)*

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Drawing on the perspective developed in recent work by Oberschall (1973), Tilly (1975) and Gamson (1975), we analyze the political process centered around farm worker insurgencies. Comparing the experience of two challenges, we argue that the factors favored in the classical social movement literature fail to account for either the rise or outcome of insurgency. Instead, the important variables pertain to social resources—in our case, sponsorship by established organizations. Farm workers themselves are powerless; as an excluded group, their demands tend to be systematically ignored. But powerlessness may be overridden if the national political elite is neutralized and members of the polity contribute resources and attack insurgent targets. To test the argument, entries in the New York Times Annual Index are content coded and statistically analyzed, demonstrating how the political environment surrounding insurgent efforts alternatively contains them or makes them successful.

From about 1964 until 1972, American society witnessed an unprecedented number of groups acting in insurgent fashion. By insurgency we mean organized attempts to bring about structural change by thrusting new interests into decision-making processes. Some of this insurgency, notably the civil rights and peace movements, had begun somewhat earlier, but after 1963 there were organized attempts to bring about structural changes from virtually all sides: ethnic minorities (Indians, Mexican-Americans, Puerto Ricans), welfare mothers, women, sexual liberation groups, teachers and even some blue-collar workers. The present study isolates and analyzes in detail one of these insurgent challenges—that of farm workers—in an effort to throw light on the dynamics that made the 1960s a period of dramatic and stormy politics.

Our thesis is that the rise and dramatic success of farm worker insurgents in the late 1960s best can be explained by changes in the political environment the movement confronted, rather than by the internal characteristics of the movement organization and the social base upon which it drew. The salient environment consisted of the government, especially the federal government, and a coalition of liberal support organizations. We shall contrast the unsuccessful attempt to organize farm workers by the National Farm Labor Union from 1946 to 1952 with the strikingly successful one of the United Farm Workers from 1965 to 1972.

The immediate goals of both movements were the same—to secure union contracts. They both used the same tactics, namely, mass agricultural strikes, boycotts aided by organized labor, and political demands supported by the liberal community of the day. Both groups encountered identical and virtually insurmountable obstacles, namely, a weak bargaining position, farm worker poverty and a culture of resignation, high rates of migrancy and weak social cohesion, and a perpetual oversupply of farm labor, insuring that growers could break any strike.

The difference between the two chal-

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lenges was the societal response that insurgent demands received. During the first challenge, government policies strongly favored agribusiness; support from liberal organizations and organized labor was weak and vacillating. By the time the second challenge was mounted, the political environment had changed dramatically. Government now was divided over policies pertaining to farm workers; liberals and organized labor had formed a reform coalition, attacking agribusiness privileges in public policy. The reform coalition then furnished the resources to launch the challenge. Once underway, the coalition continued to fend for the insurgents, providing additional resources and applying leverage to movement targets. The key changes, then, were in support organization and governmental actions. To demonstrate this, we will analyze macro-level changes in the activities of these groups as reported in the *New York Times Annual Index* between 1946 and 1972.

The Classical Model

In taking this position, we are arguing that the standard literature on social movements fails to deal adequately with either of two central issues—the formation of insurgent organizations and the outcome of insurgent challenges. Drawing on Gusfield's (1968) summary statement, the classical literature holds in common the following line of argument. See also Turner and Killian (1957; 1972), Smelser (1962), Lang and Lang (1961), Kornhauser (1959), Davies (1962; 1969) and Gurr (1970).

Social movements arise because of deep and widespread discontent. First, there is a social change which makes prevailing social relations inappropriate, producing a strain between the new and the old. Strain then generates discontent within some social grouping. When discontent increases rapidly and is widely shared, collective efforts to alleviate discontent will occur. Though there is disagreement about how to formulate the link between strain and discontent, e.g., subjective gaps between expectations and satisfactions versus emotional anxiety in-

duced by anomie, the central thrust is consistent. Fluctuations in the level of discontent account for the rise of movements and major changes in movement participation.

Recent research, though, has cast doubt on the classic "discontent" formulations. Disorders do not arise from disorganized anomic masses, but from groups organizationally able to defend and advance their interests (Oberschall, 1973; Tilly et al., 1975). As for relative deprivation, Snyder and Tilly (1972) and Hibbs (1973) have failed to find it useful in accounting for a wide variety of collective disruptions. Nor is it clear that we can use the concept without falling into post hoc interpretations (cf. Wilson, 1973:73-9).¹

In this study, we do not propose to test each of the various "discontent" formulations currently available. *A priori*, it is rather hard to believe that farm workers' discontent was, for example, suddenly greater in 1965, when the Delano grape strike began, than throughout much of the 1950s when there was no movement or strike activity. Indeed, it seems more plausible to assume that farm worker discontent is relatively constant, a product of established economic relations rather than some social dislocation or dysfunction. We do not deny the existence of discontent but we question the usefulness of discontent formulations in accounting for either the emergence of insurgent organization or the level of participation by the social base. What increases, giving rise to insurgency, is the amount of social resources available to unorganized but aggrieved groups, making it possible to launch an organized demand for change.

As for the outcome of challenges, the importance of resources is obvious. Though the classical literature has rarely dealt with the issue directly, there has been an implicit position. The resources mobilized by movement organizations are assumed to derive from the aggrieved social base. The outcome of the challenge, then, whether or not one adopts a

¹ Shifts in perceptions, treated as central by relative deprivation theorists, in our view would be secondary to the main process—changes in social resources.

"natural history" model of movement development, should depend primarily upon internal considerations, e.g., leadership changes and communication dynamics among the membership.

However, are deprived groups like farm workers able to sustain challenges, especially effective ones, on their own? We think not. Both of the movements studied were, from the outset, dependent upon external groups for critical organizational resources. Nor, as the history of agricultural strikes amply attests (McWilliams, 1939; London and Anderson, 1970; Taylor, 1975), have farm worker movements proven able to mobilize numbers sufficient to wring concessions from employers. For a successful outcome, movements by the "powerless" require strong and sustained outside support.

If this line of argument is correct, we need to contest a second thesis frequently found in the classical literature—the assertion that the American polity operates in a pluralistic fashion (cf. Kornhauser, 1959; Smelser, 1962). A pluralistic polity is structurally open to demands for change.² As Gamson (1968; 1975) has put it, the political system should be structurally "permeable," readily incorporating new groups and their interests into the decision-making process. Once organized, groups redressing widely-shared grievances should be able to secure at least some part of their program through bargaining and compromise.³ Yet our evidence shows that farm worker challenges have failed, in part, because of the opposition of public officials, and that a success-

ful challenge depended upon the intervention of established liberal organizations and the neutrality of political elites.

We can then summarize the classical model as follows. (1) Discontent, traced to structural dislocations, accounts for collective attempts to bring about change. (2) The resources required to mount collective action and carry it through are broadly distributed—shared by all sizeable social groupings. (3) The political system is pluralistic and, therefore, responsive to all organized groups with grievances. (4) If insurgents succeed, it is due to efforts on the part of the social base; if they do not, presumably they lacked competent leaders, were unwilling to compromise, or behaved irrationally (e.g., used violence or broke laws).

In contrast, we will argue that (1) discontent is ever-present for deprived groups, but (2) collective action is rarely a viable option because of lack of resources and the threat of repression. (3) When deprived groups do mobilize, it is due to the interjection of external resources. (4) Challenges frequently fail because of the lack of resources. Success comes when there is a combination of sustained outside support and disunity and/or tolerance on the part of political elites. The important variables separating movement success from failure, then, pertain to the way the polity responds to insurgent demands.

Structural Powerlessness of Farm Workers

The major impediment to farm worker unionization has been the oversupply of farm labor, undercutting all attempted harvest strikes. There are few barriers of habit or skill that restrict the entry of any applicant to work in the fields. The result is an "unstructured" labor market, offering little job stability and open to all comers (Fisher, 1953). The fields of California and Texas are close enough to the poverty-stricken provinces of Mexico to insure a steady influx of workers, many of whom arrive by illegal routes (Frisbee, 1975). Continuous immigration not only underwrites the oversupply of labor, but complicates mobilization by insuring the existence of cultural cleavages among workers.

² Note also the central role played in pluralistic interpretations by the "discontent" hypothesis. Assuming that all groups have ready access to the resources needed to mobilize, Rose (1967:249) argued: "As soon as a felt need for some social change arises, one or more voluntary associations immediately springs up to try to secure the change."

³ As the central tenet of pluralist theory, the "permeability" argument can be found in almost any presentation of the view. Dahl (1967:250) argues: "even minorities are provided with opportunities to veto solutions"; Truman's (1951) speculations about "potential groups" and Smelser's (1963:364-79) recommendations to elites for channeling "value-oriented" movements into "norm-oriented" ones both build on the assumption of a flexible political system based on a pluralistic social structure.

Furthermore, there are reasons to believe that a significant number of workers have only a limited economic interest in the gains promised by unionization. The majority of farm workers, both domestic and alien, are short-term seasonal workers. During the early 1960s, farm employment in California averaged less than three months of the year (Fuller, 1967). This means that a majority of workers are interested primarily in the "quick dollar." Imposition of union restrictions on easy access to jobs would conflict with that interest. And for the vast majority of farm workers, regardless of job commitment or citizenship status, income is so low as to leave little economic reserve for risk-taking. Since a major portion of the year's income comes during the brief harvest period, workers are reluctant to risk their livelihood on a strike at that time.

In addition to these structural restraints on collective action, there were the very direct restraints of the growers and their political allies. The California Department of Employment and the U. S. Department of Labor have long operated farm placement services that furnish workers for strike-bound employers. Insurgent actions that directly threaten growers, like picket lines and mass rallies, consistently have been the target of official harassment. Though never returning to the scale of the "local fascism" of the 1930s (McWilliams, 1942; Chambers, 1952), grower vigilante actions are not uncommon.

Bringing these considerations to bear on the comparison of farm worker challenges, there is reason to believe that circumstances were slightly more conducive to the mobilization efforts of the UFW. Between 1946 and 1965 farm wage rates rose slightly and a few public welfare benefits were extended, at least within California. Presumably, farm workers were slightly more secure economically by the mid-1960s. More significant, though, were changes in the social composition of the farm labor force. During the late 1940s farm workers in California were either "dustbowlers" or Mexican *braceros* (government-imported contract workers); by the mid-1960s the California farm labor force was predominantly

Mexican-descent, short-term workers, most of whom only recently had migrated across the border. Not only were linguistic-cultural cleavages somewhat less pronounced, but these new immigrants were more likely to settle and develop stable community ties than their "Okie" predecessors.

Also, the United Farm Workers pursued a mobilization strategy better designed than that of the NFLU to sustain the participation of farm workers. From its inception, the UFW was an Alinsky-styled community organization. The primary advantage was that it offered a program of services and social activities that did not depend upon first securing a union contract. Members developed an attachment to the organization independent of the immediate gains that might derive from any strike. Though the National Farm Labor Union had taken limited steps in a similar direction, its program remained primarily that of the conventional "business" union, promising wage gains and better working conditions rather than social solidarity and community benefits.

But the critical issue is whether differences in either the structural position of farm workers or the mobilization strategy adopted by the movements affected either dependent variable. As we shall see, the impetus for both of the challenges came from the interjection, into an otherwise placid situation, of a professionally-trained cadre backed by outside sponsors. Farm worker discontent remained unexpressed in any organized way until outside organizers arrived on the scene.

As for the question of challenge outcome, despite the UFW's advantages, it experienced no more success in strike efforts than did the NFLU. Where the NFLU had to contend with the semi-official use of *braceros* as strikebreakers, the UFW had to deal with vastly increased numbers of illegal aliens and short-term workers crossing the picket lines. The combination of structural constraints and direct controls insured that neither union was able to mobilize a sufficiently massive social base to be effective.

What separated the UFW success from the NFLU failure was the societal re-

sponse to the challenges. The NFLU received weak and vacillating sponsorship; the UFW's backing was strong and sustained. Under the pressure of court injunctions and police harassment, the NFLU boycott collapsed when organized labor refused to cooperate. By contrast, the UFW boycotts became national "causes," receiving widespread support from organized labor and liberal organizations; though official harassment remained, the UFW did not deal with the same systematic repression confronted by the NFLU. The success of a "powerless" challenge depended upon sustained and widespread outside support coupled with the neutrality and/or tolerance from the national political elite.

Method

To test this argument we need two bodies of information, one bearing on events leading to the initiation of insurgency and the other dealing with the political environment shaping challenge outcomes. For the first, we have drawn on published accounts of the movements, filled in and corroborated by extensive interviews conducted with movement participants and informed observers. For the second, we have turned to newspaper sources to provide a picture of the societal response to the two challengers. By content coding the abstracts of news stories that dealt with farm labor issues printed in the *New York Times* over a twenty-seven-year period (1946-1972), we can determine the types of groups concerned with the question of farm labor, whether their actions favored the structural changes advocated by insurgents, the types of activities in which they were engaged and, finally, the pattern of interaction prevailing between these various groups during the course of the respective challenges. This way we have a systematic data base against which to test hypotheses bearing on movement-environment interaction.⁴

As with any data source, there are limits to the *Times* data. We cannot, for

example, use it to test hypotheses on the internal dynamics of mobilization. For this, we have gone to interviews and published sources. Nor, as Danzger's (1975) work has recently indicated, can we view the *Times* reportage as a complete picture of all insurgent activity and environmental responses to insurgency. Since it is a national newspaper, the *New York Times* will not provide us with day-to-day coverage, for example, of police repression in Delano, California. Nor can we count on the *Times* to reveal the hidden bargains and machinations that might underlie public positions and alliances.

We do not ask it to do so. What we are using the *Times* for is to construct a systematic, reliable index of the publicly visible political activities that formed the environment of each challenge. By comparing statistics drawn from this data base and relating these measures to differences in challenge outcome, we can see if our environmental thesis holds up.⁵

To see if the *New York Times* is a reliable source, we have compared the coverage given by the *Times* with that of two other newspapers, the *Chicago Tribune* for a more conservative picture and the *Los Angeles Times* for a more proximate source. After comparing the stories on farm labor carried by these three papers for one month (selected at the peak of activity for the three periods of analysis), we have concluded that the *New York Times* is basically a more complete version of the same "news." In the month selected from the first period (March, 1951), the *New York Times* covered seventeen events, only one of which was picked up by each of the other papers; no events in the "test" papers were missed by the *New York Times*. In the second period (April, 1958), the *New York Times* carried nine events, two of which the *Los Angeles Times* covered and none of which the *Tribune* covered; again the *New York Times* missed no events covered in the other papers. Only in the third period (October, 1968) did the *New York Times* miss an event, one involving a local

⁴ For a copy of the coding schedule used, contact the first author.

⁵ Inter-coder reliability was set at 90%; all items failing to meet this standard were excluded from the analysis.

organization that pressured the Los Angeles City Council to boycott grapes. This was reported in the *Los Angeles Times*. Of eight events covered by the *New York Times*, half appeared in the *Los Angeles Times* and none in the *Tribune*. In sum, if you want newspaper reportage on farm labor events, the *New York Times* is a more thorough source and reveals no clearly different bias than the other papers during one period of time, say, the NFLU challenge, than another, e.g., the UFW effort.

Finally, there is the question of whether news reportage, regardless of cross-validation with other news sources, is valid. Danzger (1975) has argued that news coverage is affected by editorial policy, and that systematic error creeps in because the geographic location of national wire service offices produces uneven reportage of relevant events. It is important to note that we code events, not news stories. The prominence given to stories by the editors of the *New York Times* is irrelevant, as are the evaluations of the events by news personnel. Additionally, our data set should be relatively immune to the main source of error identified by Danzger. Both insurgencies centered in the same locale. Assuming that the corrective mechanisms within the news agencies identified by Danzger were operative, time-series data should be less vulnerable to error than cross-sectional data. Also, we should note the limitations to Danzger's conclusions given his own data base. As Snyder and Kelly (1976) have demonstrated, news-based conflict data dealing with violence appear quite valid; more error exists in nonviolent protest data (employed in Danzger's test). Extending that distinction to our own data set, we can place more confidence in our measures of "concrete" activities than those for "symbolic" ones.

Basic Variables

Our analysis centers on the comparison of three time periods. The first, 1946–1955, spans the challenge of the National Farm Labor Union. Chartered to organize farm workers at the 1946 American Federation of Labor convention, the NFLU

launched a strike wave in the Central Valley of California that ended with the abortive Los Baños strike of 1952. The selection of 1955 as the end point of the period was somewhat arbitrary.

By comparison, the third period, 1965–1972, covers the sustained and successful challenge of the United Farm Workers. The 1965 Coachella and Delano strikes announced the UFW challenge; in 1970, after two years of nation-wide boycott efforts, the UFW brought table-grape growers to the bargaining table and began institutionalizing changes in the position of farm workers. (The Teamster entry in 1973 is not dealt with in this paper.)

During the period intervening between the two challenges, 1956–1964, important changes took place in the political system that set the stage for a successful challenge. In the absence of a major "push" from insurgents,⁶ issues pertaining to farm labor received a different treatment in the hands of established liberal organizations and government officials. We will argue that these years constituted a period of germination and elite reform that made possible the success of the late 1960s.

From the *New York Times Annual Index*, we have coded the types of groups involved, the direction and form of their activity and the issues involved. The groups are: (1) the farm worker associations and unions that represented farm worker insurgents; (2) federal, state and local governments; (3) the liberal organizations (religious, philanthropic, political action and "public interest" groups); (4) organized labor; (5) agribusiness associations, corporations and individual farmers, referred to collectively as the growers. Of these, the growers have the fewest events reported in the *Times*, probably because fewer of their activities are likely to constitute "notable" events in a journalistic sense—e.g., securing the services

⁶ The Agricultural Workers Organizing Committee (AFL-CIO) was chartered in 1959, but never posed a serious threat to growers (London and Anderson 1970: 46–78); the National Farm Worker Association was an independent community organization launched by Cesar Chavez in 1962 and entered the labor question in an offensive way only in 1965 (London and Anderson, 1970: 148–53).

of local police, hiring strikebreakers, rounding up support among legislators. Their views are generally presented quite effectively by the Department of Agriculture so they need do little public relations on their own.

The first step is to break down group activity by direction—into actions favorable, unfavorable, ambiguous, or not relevant to the interests of farm workers. (Only government had significant numbers of both favorable and unfavorable actions. All other groups were either wholly favorable or unfavorable. Government was also the only type with a large number of “ambiguous” or “not relevant” actions. These are excluded from the analysis; they do not depart in terms of issue or type of action from “directed” actions.) We then can estimate the balance of favorable/unfavorable actions in the political system during the course of each challenge, and chart the fluctuations in favorable and unfavorable actions by different types of groups (see Figures 1 and 2).

In addition to group and direction, we are concerned with the form of action adopted. We will distinguish between “symbolic” and “concrete” actions. Purely rhetorical acts which attempt to shape public opinion, e.g., speeches or hearings, are “symbolic”; actions that attempt to directly allocate control over material resources, e.g., court rulings and mass protest, fall under the rubric of “concrete.”

Issue is our final variable: (1) labor supply, which is largely centered around the importation of Mexican labor under the *bracero* and “green card” programs and which was the dominant issue during the NFLU challenge; (2) working and living conditions of farm workers, which dominated the remaining two periods; (3) unionization, i.e., the legality of collective bargaining in agriculture, a question which first appeared in significant measure only during the UFW challenge.

Two types of statistics drawn from this data set will be used. N's, percentages and percent differences set off the rough differences between the three periods of activity. To capture more precisely the divergent patterns of interaction taking

place between insurgents and among various groups in the polity, Pearson product-moment correlations are reported. The scores entering the analysis are counts of actions taken by different groups, on different issues, for conventional calendar years. High r 's are taken to indicate that considerable concomitant activity took place over the time period between relevant pairs of groups, e.g., insurgents and liberals; low r 's, the absence of concomitant activity. Bringing this to bear on the environmental thesis, differences in descriptive statistics and r 's for relevant pairings of groups will reveal any differences that existed in the societal response to the challenges.⁷

Period I: The NFLU Conflict (1946–1955)

The first period illustrates in classical terms the obstacles to a sustained and successful farm worker challenge. In addition to the structural constraints restricting farm worker activity, the political environment confronting the insurgents was unfavorable. Government officials at all levels and branches came into the conflict predominantly on the side of the growers, despite the mandate of agencies such as the Department of Labor or the Education and Labor Committees in Congress to protect the interests of deprived groups like farm workers. Though external support was decisive in launching the challenge, it was weak and frequently ill-focused, dealing with the consequences rather than the causes of farm worker grievances. When support was withdrawn, the challenge soon collapsed.

Chartered at the 1946 convention of the American Federation of Labor, the Na-

⁷ Contrary to most time-series analyses, controls for auto-correlation are inappropriate. The correlation analysis does not causally relate a dependent variable (e.g., level of insurgent activity) to a set of independent variables (e.g., level of liberal activity). Instead, it is designed to reveal whether significant differences exist between time periods in movement-environment interaction. These differences are then held to account for the divergent outcomes. Instead of asking, “Does liberal activity cause insurgency?” we are asking, “Did insurgent and liberal activities co-occur to a different extent during one challenge than another? Did this difference relate to different challenge outcomes?”

tional Farm Labor Union set out to accomplish what predecessors had been unable to do—successfully organize the farm workers of California's "industrialized" agriculture. The leadership cadre was experienced and resourceful. H. L. Mitchell, President of the NFLU, was former head of the Southern Tenant Farmers Union; the Director of Organizations, Henry Hasiwar, had been an effective organizer in several industrial union drives during the 1930s; Ernesto Galarza, who assumed prime responsibility for publicity efforts, had served as political liaison for Latin American unions and had a Ph.D. in economics from Columbia University.

Initially, the strategy was quite conventional: enlist as many workers as possible from a single employer, call a strike, demand wage increases and union recognition, and picket to keep "scabs" out of the fields. American Federation of Labor affiliates would then provide strike relief and political support to keep the picket line going. An occasional church or student group would furnish money and boost morale.

But the government-sponsored alien labor or *bracero* program provided growers with an effective strike-breaking weapon. According to provisions of the law, *braceros* were not to be employed except in instances of domestic labor shortage and *never* to be employed in fields where domestic workers had walked out on strike. Yet in the two major tests of union power, the DiGiorgio strike of 1948 and the Imperial Valley strike of 1951, the flood of *braceros* undermined the strike effort of domestic workers (London and Anderson, 1970; Galarza, 1970; Jenkins, 1975: ch. 3). In the Imperial strike, the NFLU used citizen's arrests to enforce statutes prohibiting employment of *braceros* in labor disputed areas. However, local courts ruled against the tactic and the Immigration Service refused to remove alien "scabs" from the fields (Galarza, 1970:78; Jenkins, 1975: ch. 4). Nor were affairs changed when the *bracero* administration was transferred to the U.S. Department of Labor in 1951. Domestic workers were pushed out of crops by *braceros*, and *braceros* reap-

peared in the Los Baños strike of 1952 to break the challenge (Galarza, 1970:79).

In response, the NFLU launched a two-pronged political challenge—a demand for termination of the *bracero* program and, to get around the problem of ineffective strikes, requests for organized labor's support of boycotts. Neither demand found a favorable audience. Lacking strong labor or liberal support, the demand for an end to the *bracero* traffic ended in minor reforms in the *bracero* administration (Galarza, 1970: ch. 4). As for the boycott, despite initial success, it collapsed when a court injunction was issued (improperly) on the grounds that the NFLU was covered by the "hot cargo" provisions of the Taft-Hartley Act. The National Labor Relations Board initially concurred and reversed its position over a year later. By then the Union's resources were exhausted and organized-labor support had long since collapsed (Galarza, 1970:73–92).

Figure 1 charts the level of favorable actions by selected groups, allowing us to gauge the societal response to insurgency. The curves delineating government, liberal, and farm worker activities move roughly in concert. (Organized labor, though, played little public role in this or the next period.) Checking these impressions, Table 1 reports Pearson r 's on relevant pairs of groups. Largely a reflection of the pressure campaign waged by the NFLU, the strongest correlation is between insurgents and favorable government activity (.63), concrete activities seemingly being more efficacious (.70 versus .49 for symbolic acts). R for insurgent/government activity drops only slightly when controls are introduced for liberal activity (.57), indicating that liberal activity was not necessary for this measure of official response.

The main issue for the period was labor supply. Looking at activities concerned with this issue, the correlation between insurgent and pro-farm worker government activities is high (.59); for the issue of living and working conditions, the relation disappears (–.08). The union attempted, through court actions, lobby efforts and public protest, to pressure gov-

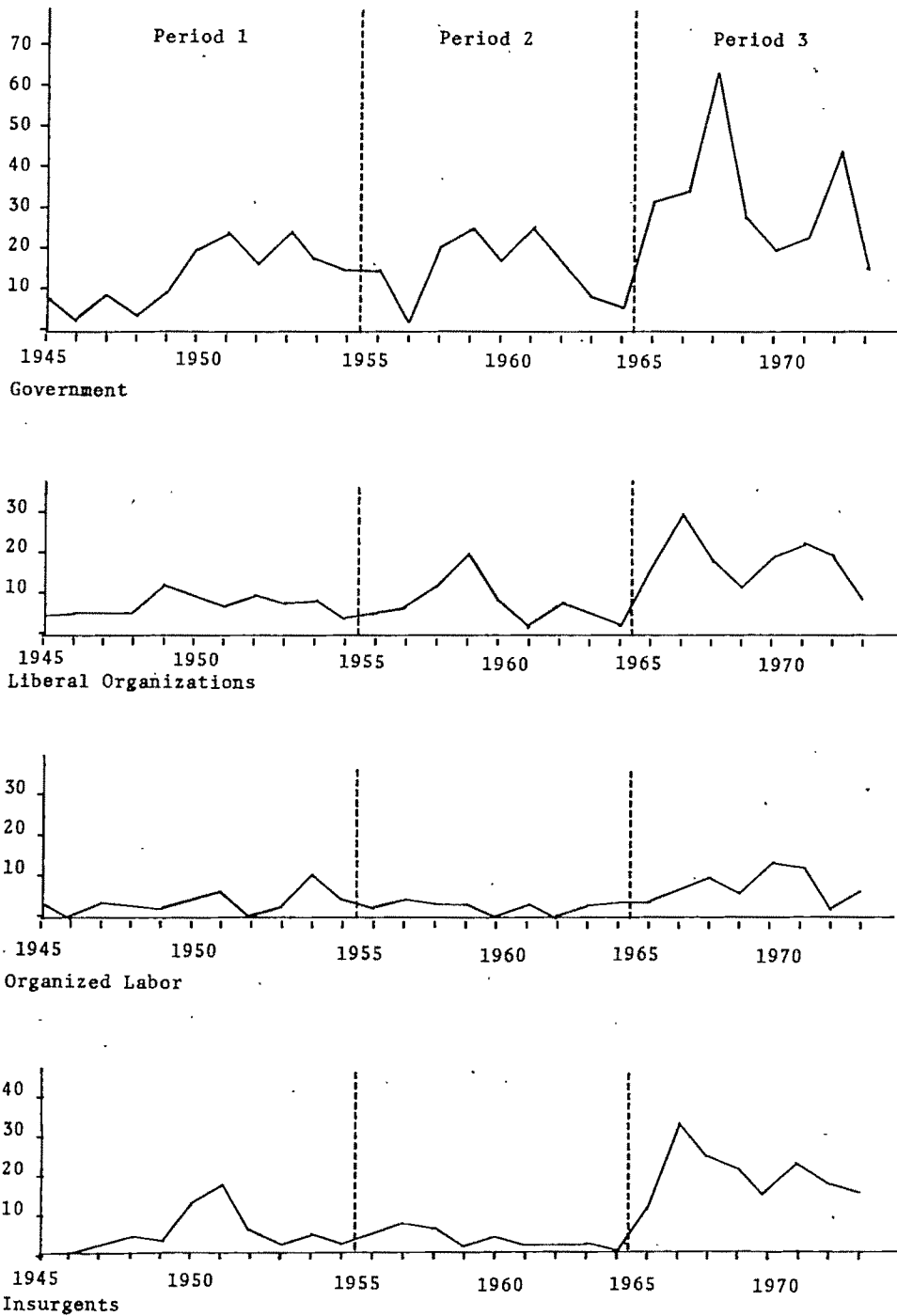


Figure 1. Actions Favorable to Insurgents

ernment to end the *bracero* program since it was so central to the control of the labor supply. The official response, however, was largely symbolic. Though govern-

ment tended to respond to concrete insurgency with favorable concrete actions, the majority of favorable governmental actions were actually symbolic (58%).

Table 1. Extent of Concomitant Activity—Pro Farm Worker (*r*'s)

	All Acts	Symbolic Acts ^a	Concrete Acts ^a
Period I (1946–1955)			
Insurgents and Government	.63	.49	.70
Insurgents and Liberal Pressure Groups	.45	.56	-.02
Insurgents and Organized Labor	.08	-.08	— ^b
Liberal Pressure Groups and Government	.33	.37	-.17
Organized Labor and Government	.36	.35	— ^b
Period II (1956–1964)			
Insurgents and Government	-.26	-.26	-.42
Insurgents and Liberal Pressure Groups	-.10	.05	-.13
Insurgents and Organized Labor	.59	.67	-.33
Liberal Pressure Groups and Government	.50	.53	.25
Organized Labor and Government	.06	-.60	.58
Period III (1965–1972)			
Insurgents and Government	.26	.26	.04
Insurgents and Liberal Pressure Groups	.62	.06	.83
Insurgents and Organized Labor	.16	.43	-.01
Liberal Pressure Groups and Government	.04	.16	-.08
Organized Labor and Government	-.002	.46	-.54

^a Symbolic or concrete for both types of groups.

^b N for one group during this period was zero.

Nor did many of these concrete moves decisively aid the farm worker cause. Key actions, such as pulling strikebreaking *braceros* out of the fields, did not occur.

What, then, are we to make of the fact that 50% of reported governmental actions were coded as favorable to the interest of farm workers? Was government responding to the conflict between insurgents and growers in some even-handed "pluralist" way? Here it is necessary to recall that we are using news media reportage on a social problem and efforts to redress that problem. The news media will be more sensitive to efforts attempting to define or solve that problem than to efforts to maintain the *status quo*. Consequently, unfavorable actions by government and growers are underrepresented in our data. If only 50% of news-reported government actions can be coded as favorable, then the full universe of governmental activities should, in the balance, be more favorable to growers.

The strength of this assertion is borne out by information on actions favorable to growers. Figure 2 charts these actions for government and growers. The correlation between pro-grower government activities and grower activities is quite high (.75), actually stronger than the respective *r* for insurgents. In quantitative terms,

government was more responsive to agribusiness interests. Clearly, in critical instances, e.g., leaving *braceros* in struck fields, government policies favored growers over workers.

In addition to the predominantly unfavorable response of government, the NFLU failed to receive sustained, solid support from the liberal community. The major problem was the type of activities in which liberals engaged. When they acted, liberals consistently supported farm workers over growers but they rarely moved beyond symbolic proclamations. Only 24% of liberal actions during the period were concrete. By contrast, 38% during the UFW challenge were so. Even more indicative, though, is the modest level of the correlation between liberal and insurgent activity (.45). What concomitant activity did exist between these two groups involved only symbolic acts (.56 versus -.02 for concrete acts). Looking ahead, the respective *r*'s for the UFW challenge indicate a quite different liberal response. Overall, *r* was .62; for concrete actions, *r* was .83 and, for symbolic acts, .06. Where the UFW experienced consistent and concrete support, the NFLU found itself relatively isolated.

Though liberals did not rush to the side of the NFLU, they did play a role in the

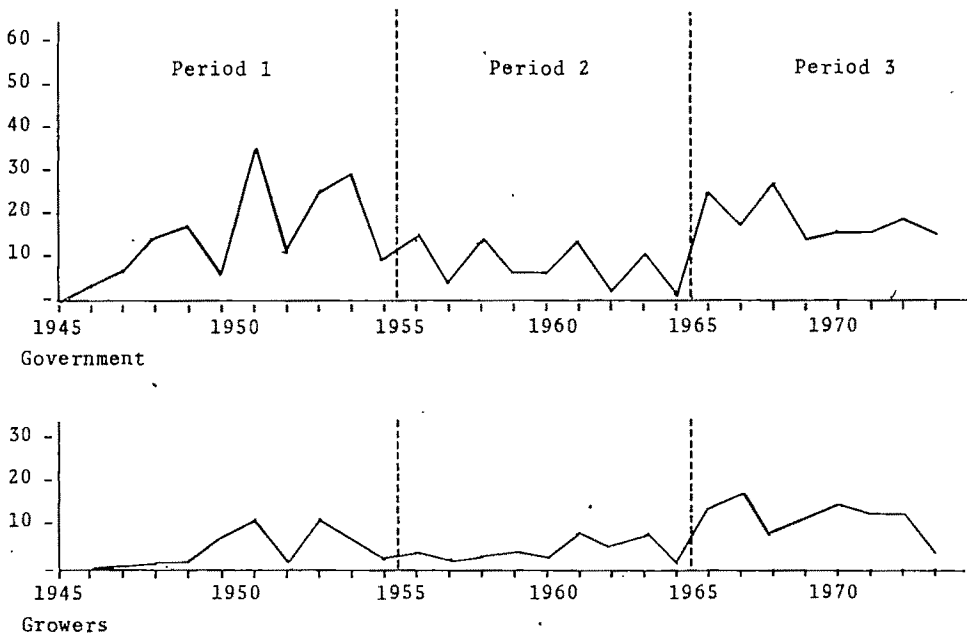


Figure 2. Actions Favorable to Growers

pressure campaign. When controls are introduced for government activity on the relation between insurgents and liberals, the modestly positive relation turns negative ($-.10$). Insofar as liberals did act alongside insurgents, apparently it was in the presence of public officials. But there were problems even with this limited-scale liberal support. Liberals focused almost exclusively on the working and living conditions of farm workers. Following the lead of Progressive Party candidate Henry Wallace in 1948, several religious and "public interest" associations sponsored conferences and issued study reports publicizing deplorable camp conditions and child labor. In what might be considered a typical pattern of liberalism of the time, they were concerned with the plight of the workers rather than the fact of their powerlessness or the role of the *bracero* program in underwriting that powerlessness. It was a humanitarian, nonpolitical posture, easily dissipated by "red baiting" in Congressional investigations and "red scare" charges by growers and their political allies throughout the late 1940s and early 1950s. The two issues, poverty and the question of labor supply, were not to be linked by the liberal organizations until well into Period II.

Period II: Elite Reform and Realignment (1956-1964)

The late 1950s and the early 1960s, the second Eisenhower administration and the brief Kennedy period emerge from this and other studies in the larger project as a period of germination. Contrary to some interpretations, the remarkable insurgencies of the late 1960s did not originate with the Kennedy administration, but with developments that initially began to appear during Eisenhower's second term. Nor did the Kennedy years witness a dramatic escalation of insurgent activity. Indeed, in the case of farm workers, insurgency showed a decline (Figure 1). For our purposes, the two presidential administrations can be treated as a single period, one that witnessed important realignments and shifts in political resources in the national polity, culminating in a supportive environment for insurgent activity.

Farm worker insurgency during the reform period was at a low ebb. Actions by farm worker insurgents dropped from 16% to 11% of all pro-worker activity. In 1956-1957 the NFLU, now renamed the National Agricultural Workers Union (NAWU), secured a small grant from the

Table 2. Extent of Concomitant Activity—Pro Grower (*r*'s)

	All Issues	Work-Life Conditions ^a	Labor Shortage ^a	Union- ization ^a
Period I (1946–1955)				
Insurgents and Government	.63	-.08	.59	— ^b
Insurgents and Liberal Pressure Groups	.45	.21	.76	—
Insurgents and Organized Labor	.08	.17	.08	—
Liberal Pressure Groups and Government	.33	.02	.40	—
Organized Labor and Government	.36	.18	.37	—
Period II (1956–1964)				
Insurgents and Government	-.26	-.28	-.04	—
Insurgents and Liberal Pressure Groups	-.10	-.13	-.04	—
Insurgents and Organized Labor	.59	.20	.58	—
Liberal Pressure Groups and Government	.50	.50	.23	—
Organized Labor and Government	.06	.21	-.05	—
Period III (1965–1972)				
Insurgents and Government	.26	.74	— ^b	-.21
Insurgents and Liberal Pressure Groups	.62	.84	—	.35
Insurgents and Organized Labor	.16	.57	—	.09
Liberal Pressure Groups and Government	.04	.49	—	-.08
Organized Labor and Government	-.002	.13	—	.36

^a Work-life for both types of groups; labor shortage for both, unionization for both.

^b N for one group during this period was zero.

United Auto Workers, enabling it to hang on as a paper organization. Galarza, by then the only full-time cadre member, launched a publicity campaign to reveal maladministration and corruption within the *bracero* administration. Aside from a brief and ineffective organizing drive launched in 1959 by the Agricultural Workers Organizing Committee (AWOC), generating only one reported strike (in 1961), this was the sum of insurgent activity for the nine-year Period II (Figure 1). Growers remained publicly inactive and seemingly secure in their position, aroused only at renewal time for the *bracero* program to lobby bills through Congress. Until the insurgency of Period III began, growers retained a low profile in the *Times* (Figure 2).

With the direct adversaries largely retired from the public arena, affairs shifted into the hands of government and the liberals. Despite the absence of significant insurgency, the balance of forces in the national polity had begun to shift. Actions favorable to the interests of farm workers increased from 50% to 73%, remaining on the same plane (75%) throughout the following UFW period. Beginning during the last years of the Eisenhower administration, three interrelated developments brought about this new supportive envi-

ronment: (1) policy conflicts within the political elite that resulted in a more "balanced," neutral stance towards farm workers; (2) the formation of a reform coalition composed of liberal pressure groups and organized labor that, in the midst of elite divisions, was able to exercise greater political influence; (3) the erosion of the Congressional power-base of conservative rural interests, stemming immediately from reapportionment.

The concern of liberal pressure groups initially was focused on the need to improve housing and educational conditions of migrant workers. In 1956, the Democratic National Convention included a plank for increased welfare aid to migrants. The next year, the National Council of Churches, already involved in the early civil rights movement in the South, began a study of migrant camp conditions and child labor. In early 1958, the Council brought public pressure to bear on Secretary of Labor James Mitchell to enforce existing laws regarding migrant camps throughout the nation. In late 1958, several liberal pressure groups were joined by the AFL-CIO in attacking the *bracero* program, scoring administrative laxity, and arguing that federal labor policies were the origin of social problems. The two as yet unrelated issues—poverty and

labor policies—were now firmly linked in the public debate.

The fusion of these two issues was significant. Of course, economic conditions already had been linked with social deprivations in public parlance, but the concern of liberal groups in the past had been with inspection of housing, assurances of educational opportunity, and public health measures. To argue now that a public program of importing foreign labor perpetuated the list of conditions deplored by liberals was a substantial change. As later happened more generally with the New Left (cf. Perrow, 1972), the advocates of reform had begun to look at the source of problems in terms of a system.

About the same time, organized labor took a new interest in farm workers. In 1959, the AFL-CIO Executive Council abolished the NAWU and created the Agricultural Workers Organizing Committee (AWOC), headed by Norman Smith, a former UAW organizer. Despite strong financial backing, the AWOC produced little results. Concentrating on 4 A.M. "shake-ups" of day laborers, the AWOC managed to sponsor a number of "job actions" but only one major strike and little solid organization. Like the NFLU, the AWOC had to confront the problem of *braceros*. In the one reported strike, the Imperial Valley strike of February, 1961, the AWOC used violence to intimidate strikebreaking *braceros* and create an international incident over their presence. Officials quickly arrested the cadre, and the AWOC ceased to exist except on paper. Though the AWOC drive consumed over one million dollars of AFL-CIO funds, it produced neither contracts nor stable membership (London and Anderson, 1970:47–50, 77). Yet, and this indicates the shift, this type of financial support had never before been offered by organized labor.

The final element in the formation of a supportive environment was a shift in governmental actions. Actions favorable to farm workers increased from the unfavorable 50% prevailing during Period I to a more "balanced" 68% of all governmental actions. Of these, the portion coded "concrete," and therefore more likely to have impact, increased from 40%

in Period I to 65%. Indicative of the change taking place in official views, the focus of governmental attentions shifted from the labor supply issue (56% of favorable actions during Period I) to the question of farm workers' living and working conditions (73% during Period II).

The change in official actions stemmed, in part, from internal conflicts within the national political elite. Secretary of Labor James Mitchell was a surprise Eisenhower appointee from the Eastern wing of the Republican Party, a former labor consultant for New York department stores and a future protege of Nelson Rockefeller. Mitchell took the Department of Labor in a more pro-union direction than was thought possible, at the time becoming a "strong man" in the cabinet because of his success in mollifying unions.⁸ In 1958, an open fight between the Taft and Eastern wings of the Republican Party developed, with the conservatives favoring a national "right-to-work" law. Mitchell, as an advocate of unionism and apparently jockeying for position for the Republican Vice-Presidential nomination, became a figure of elite reform within Republican circles.

A second factor contributing to the shift in official actions was the pressure campaign launched by the reform coalition. The effects of the campaign can be captured, in part, from the *Times* data. Though the correlation between liberal activity and government activity favorable to farm workers is modest (.50), it is considerably higher than during the other periods (.33 for the first and .04 for the third) and it is independent of insurgent activity.

Tangible effects of the pressure campaign appeared almost immediately. In 1957, under pressure from the liberal reform coalition, the Department of Labor under Mitchell's guidance carried out an internal review of farm labor policies. The upshot was a series of executive orders to tighten up enforcement of regulations covering migrant camps (Craig, 1971: 151–5). When the economic recession of 1958–1959 arrived, sensitivity within the

⁸ *New York Times*, October 5, 1958, VI; 9: 2; October 20, 1964: 37:1.

Administration to rising unemployment levels increased. In response, Mitchell vowed to enforce more fully the 1951 statutes requiring farm employment to be offered to domestic workers prior to importation of *braceros*. Growers, long accustomed to having their *bracero* requests met automatically, rebelled when asked to provide more justification (Jacobs, 1963: 183-4). In February, 1959, Mitchell took an even stronger step, joining the liberal reformers in support of legislation to extend minimum-wage laws to agriculture and to impose new restrictions on the use of *braceros*.

The following year, the division within the Eisenhower Administration opened up into a full-scale, cabinet-level battle over renewal of the *bracero* program. The Farm Bureau and the state grower associations engaged that other administration "strong man," Secretary of Agriculture Ezra Taft Benson, to defend the program. In testimony before the House Committee on Agriculture, the White House took a neutral stance; Benson defended the program, while Mitchell argued that the program exerted demonstrable adverse effects upon domestic workers and should be abolished (Craig, 1971: 156-61). Into this breach in the political elite stepped the liberal-labor support coalition. At the same time, the House Committee on Public Welfare opened hearings on health and camp conditions, giving the Cotton Council and the Meatcutters Union a chance to air opposing views.

Initially, the reform effort failed. In March, 1960, Secretary Mitchell withdrew his program, resolving the dispute on the cabinet level. The next month, agribusiness pushed a two-year renewal of the *bracero* program through Congress. But, for the first time, the issue had been debated seriously and a loose coalition of liberal pressure groups (e.g., National Council of Churches, National Advisory Committee of Farm Labor, NAACP) and organized labor had formed. Though the eventual termination of the *bracero* program did not undermine growers' ability to break strikes (there were other substitutes, e.g., "green card" commuters, illegal aliens), the fight against the program

did refocus the concern of liberals and organized labor on the structural problem of farm worker powerlessness.

The reform coalition sustained the campaign over the next three years. In 1960, the Democratic platform condemned the *bracero* program. Once in office, the New Frontiersmen, though demanding no important statutory changes, did vow to enforce fully the laws restricting *bracero* use (Craig, 1971: 174). By renewal time in 1963, the Kennedy Administration was in the pursuit of a public issue ("poverty") and courting minority-group votes. For the first time, the White House went formally on record against the program. Only at the last minute was a pressure campaign, mounted by Governor Pat Brown of California and the Department of State, responding to Mexican diplomatic pressure, able to save the program temporarily. Amid promises from Congressional farm bloc leaders that this was the last time the program would be renewed, a one-year extension was granted.

In addition to the efforts of the reform coalition, which played a critical role in other reforms of the same period, and the new elite-level neutrality, the fall of the *bracero* program stemmed from the narrowing power base of the Congressional farm bloc. Congressional reapportionment had visibly shaken the conservative farm bloc leaders. Searching for items in the farm program that could be scuttled without damaging the main planks, the farm bloc leaders fixed on the *bracero* program. The mechanization of the Texas cotton harvest had left California growers of specialty crops the main *bracero* users. When the test came, *bracero* users, as a narrow, special interest, could be sacrificed to keep the main planks of the farm program intact (Hawley, 1966).

Period II, then, emerges from this analysis as a period of reform and political realignment that dramatically altered the prospective fortunes of insurgents. Reforms, stemming from elite-level conflicts and a pressure campaign conducted by liberal public-interest organizations and organized labor, came about in the virtual absence of activity by farm worker insurgents. The activism of several key liberal organizations depended, in turn, upon

broad economic trends, especially the growth of middle-class disposable income that might be invested in worthy causes (McCarthy and Zald, 1973). Insurgents did not stimulate these changes in the national polity. Rather, they were to prove the beneficiaries and, if anything, were stimulated by them.

Period III: The UFW Success (1965–1972)

During the NFLU period, the number of insurgent actions reported totalled 44. Most of these were symbolic in character, only 27% being concrete. Insurgency was brief, concentrated in a four-year period (1948–1951). However, in the third period, insurgency became sustained. Insurgent actions reached a new peak and remained at a high level throughout the period. A total of 143 actions conducted by farm worker insurgents were recorded. Significantly, 71% of these were concrete in character. By the end of the period, the success of the United Farm Workers was unmistakable. Over a hundred contracts had been signed; wages had been raised by almost a third; union hiring halls were in operation in every major agricultural area in California; farm workers, acting through ranch committees set up under each contract, were exercising a new set of powers.

The key to this dramatic success was the altered political environment within which the challenge operated. Though the potential for mobilizing a social base was slightly more favorable than before, the UFW never was able to launch effective strikes. Though the UFW cadre was experienced and talented, there is little reason to believe that they were markedly more so than the NFLU leadership; neither did the tactics of the challenge differ. The boycotts that secured success for the UFW also had been tried by the NFLU, but with quite different results. What had changed was the political environment—the liberal community now was willing to provide sustained, massive support for insurgency; the political elite had adopted a neutral stance toward farm workers.

As before, external support played a critical role in launching the challenge.

The initial base for the United Farm Workers was Cesar Chavez's National Farm Workers Association (NFWA) and remnants of the AWOC still receiving some support from the AFL-CIO.⁹ During the 1950s, Chavez had been director of the Community Service Organization, an Alinsky-styled urban community-organization with strong ties to civil rights groups, liberal churches and foundations. Frustrated by the refusal of the CSO Board of Directors to move beyond issues salient to upwardly-mobile urban Mexican-Americans, Chavez resigned his post in the winter of 1961 and set out to organize a community organization among farm workers in the Central Valley of California. Drawing on his liberal contacts, Chavez was able to secure the backing of several liberal organizations which had developed a new concern with poverty and the problems of minority groups. The main sponsor was the California Migrant Ministry, a domestic mission of the National Council of Churches servicing migrant farm workers. During the late 1950s, the Migrant Ministry followed the prevailing policy change within the National Council, substituting community organization and social action programs for traditional evangelical ones (Pratt, 1972). By 1964, the Migrant Ministry had teamed up with Chavez, merging its own community organization (the FWO) with the NFWA and sponsoring the Chavez-directed effort.¹⁰

By summer, 1965, NFWA had over 500 active members and began shifting directions, expanding beyond economic benefit programs (e.g., a credit union, cooperative buying, etc.) to unionization. Several small "job actions" were sponsored. Operating nearby, the remaining active group of the AWOC, several Filipino

⁹ For a detailed discussion, see Jenkins (1975: chs. 7–8).

¹⁰ There was also a brief challenge launched in 1965 among black tenant farmers in the Mississippi Delta region (the Mississippi Freedom Labor Union). The dynamics of that challenge are virtually identical to the UFW—sponsorship by liberal churches, labor union, etc. (for a history, see Hilton, 1969). Given the low event-count for this challenge, though, the statistics reported pertain to the UFW.

work-crews, hoped to take advantage of grower uncertainty generated by termination of the *bracero* program. The AWOC launched a series of wage strikes, first in the Coachella Valley and then in the Delano-Arvin area of the San Joaquin Valley. With the AWOC out on strike, Chavez pressed the NFWA for a strike vote. On Mexican Independence Day, September 16th, the NFWA joined the picket lines (Chavez, 1966; Dunne, 1967; London and Anderson, 1970).

Though dramatic, the strike soon collapsed. Growers refused to meet with union representatives; a sufficient number of workers crossed the picket lines to prevent a major harvest loss. Over the next six years, the same pattern recurred—a dramatic strike holding for a week, grower intransigence, police intimidation, gradual replacement of the work force by playing upon ethnic rivalries and recruiting illegal aliens (cf. Dunne, 1967; London and Anderson, 1970; Matthiessen, 1969; Kushner, 1975; Taylor, 1975). What proved different from the NFLU experience was the ability of the insurgents, acting in the new political environment, to secure outside support.

Political protest was the mechanism through which much of this support was garnered. By dramatic actions designed to capture the attention of a sympathetic public and highlight the "justice" of their cause, insurgents were able to sustain the movement organization and exercise sufficient indirect leverage against growers to secure contracts. The UFW's use of protest tactics departed from that of rent strikers analyzed by Lipsky (1968; 1970). Though the basic mechanism was the same (namely, securing the sympathy of third parties to the conflict so that they would use their superior resources to intervene in support of the powerless), the commitments of supporting organizations and the uses to which outside support was put differed. Lipsky found that protest provided unreliable resources, that the news media and sympathetic public might ignore protestors' demands (cf. Goldenberg, 1975) and that, even when attentive, they often were easily satisfied with symbolic palliatives. Though the UFW experienced these problems, the presence of

sustained sponsorship on the part of the Migrant Ministry and organized labor guaranteed a stable resource base.

Nor were the uses of protest-acquired resources the same. Lipsky's rent-strikers sought liberal pressure on public officials. For the UFW, protest actions were used to secure contributions and, in the form of a boycott, to exercise power against growers. Marches, symbolic arrests of clergy, and public speeches captured public attention; contributions from labor unions, theater showings and "radical chic" cocktail parties with proceeds to "*La Causa*" supplemented the budget provided by sponsors and membership dues.

Given the failure of strike actions, a successful outcome required indirect means of exercising power against growers. Sympathetic liberal organizations (e.g., churches, universities, etc.) refused to purchase "scab" grapes. More important, though, major grocery chains were pressured into refusing to handle "scab" products. To exercise that pressure, a combination of external resources had to be mobilized. Students had to contribute time to picketing grocery stores and shipping terminals; Catholic churches and labor unions had to donate office space for boycott houses; Railway Union members had to identify "scab" shipments for boycott pickets; Teamsters had to refuse to handle "hot cargo"; Butchers' union members had to call sympathy strikes when grocery managers continued to stock "scab" products; political candidates and elected officials had to endorse the boycott. The effectiveness of the boycott depended little upon the resources of mobilized farm workers; instead, they became a political symbol. It was the massive outpouring of support, especially from liberals and organized labor, that made the boycott effective and, thereby, forced growers to the bargaining table.

The strength of liberal-labor support for the UFW is indicated by the high level of concomitant activity between insurgents and their supporters. While the correlation of insurgent and liberal activities was modest in Period I (.45), it was strong during the third period (.62). More impor-

tant, liberals were far more concrete in their support for insurgents. In the first period, concomitant activities were almost wholly symbolic (.56 versus .02 for concrete activities); during the UFW challenge, it was concrete activities (.81 versus .06 for symbolic activities). Nor do statistical controls for governmental actions favorable to farm workers reduce the correlation ($r = .64$). Given the fact that liberal activities rarely occurred jointly with pro-worker government activities ($r = .04$), it is clear that liberals directed their efforts toward supporting insurgents rather than pressuring government.¹¹

The more "balanced," neutral posture of government that was the product of the reform period continued. Sixty-nine percent of all official actions were favorable to farm workers (as against 50% and 68% in Periods I and II). Concretely, this meant that court rulings no longer routinely went against insurgents; federal poverty programs helped to "loosen" small town politics; hearings by the U.S. Civil Rights Commission and Congressional committees publicized "injustices" against farm workers; welfare legislation gave farm workers more economic security and afforded insurgents a legal basis to contest grower employment practices. National politicians, such as Senators Kennedy and McGovern, lent their resources to the cause.

The most striking changes in official actions took place on the federal level. Actions favorable to farm workers rose from 46% of federal level activity in the first period, to 63% in the second and 74% in the third. State and local government, more under the control of growers (cf. McConnell, 1953:177; Berger, 1971), followed a different pattern. In Period I, when growers had opposition only from insurgents, only 26% of official actions were judged favorable to workers. In Period II, when farm workers were ac-

quiescent but the liberal-labor coalition was experiencing growing influence in national politics, 67% were favorable, slightly more than on the federal level. But when insurgency reappeared in Period III, the percent favorable dropped to 45%, far lower than the federal level. Government divided on the question, federal actions tending to be neutral, if not supportive, of insurgents while state actions, still under grower dominance, continued to oppose insurgents.

Significantly little of the pro-worker trend in governmental actions during the UFW period is associated with either insurgent or liberal activities. For insurgent and favorable government actions, r is low (.26 versus .63 during the NFLU period); the correlation between liberal organizations and favorable government actions drops to the lowest point in the study (.04 versus .33 and .50 for Periods I and II, respectively). Only organized labor appeared to be performing a pressure function. There is a modest correlation between symbolic activities by organized labor and government (.46), largely centering around the legitimacy of unionism in agriculture ($r = .35$). Official positions had already undergone important changes during the reform period. The termination of the *bracero* program had left government in a neutralized position. No longer a key player in the conflict, but still under the influence of the reform policies, government preserved its neutral stance despite less visible pressure from any of the partisans.¹²

There was, of course, opposition on the part of growers and allied governmental actors. There were numerous instances of police harassment, large-scale purchases of boycotted products by the Department of Defense, and outspoken opposition

¹¹ Despite the fact that help from organized labor was critical to the boycott's success, our correlations hardly document the point. In the NFLU challenge, r was .08; in the UFW period, .16. This relation is weaker than that for liberal pressure groups, we would argue, because much of the supportive labor action was "local" in character and often went unreported in the *Times*.

¹² Corroborating this interpretation, the correlation between insurgent/liberal actions and pro-worker government actions is considerably stronger (.74 and .58, respectively) once insurgent and liberal actions are lagged by one year. As a roughly neutral participant, government followed along a year behind the chief partisans, though not responding directly to pressure as before. Though not conclusive, the fact that this was the only instance in the study in which time-lags produced marked increases in r 's lends the interpretation some plausibility.

from Governor Reagan and President Nixon.

However, growers had lost their entrenched political position. Public officials no longer acted so consistently to enhance grower interests and to contain the challenge. An indication of the sharpness of the displacement of growers is given by the levels of concomitance between grower actions and pro-grower governmental actions. In Period I, r for grower-government activity was .75; in Period II, .62. But, during the UFW challenge, the correlation dropped to a negligible .05. By the time the United Farm Workers struck in 1965, agricultural employers were no longer able to rely upon government, especially at the federal level, to be fully responsive to their interest in blocking unionization.

Conclusion

The critical factor separating the National Farm Labor Union failure from the United Farm Worker success was the societal response to insurgent demands. In most respects, the challenges were strikingly similar. In both instances, the leadership cadre came from outside the farm worker community; external sponsorship played a critical role in launching both insurgent organizations; both movements confronted similar obstacles to mobilizing a social base and mounting effective strikes; both resorted to political protest and boycotts. What produced the sharp difference in outcome was the difference in political environment encountered. The NFLU received token contributions, vacillating support for its boycott and confronted major acts of resistance by public authorities. In contrast, the UFW received massive contributions, sustained support for its boycotts and encountered a more "balanced," neutral official response.

The dramatic turnabout in the political environment originated in economic trends and political realignments that took place quite independent of any "push" from insurgents. During the reform period, conflicts erupted within the political elite over policies pertaining to farm workers. Elite divisions provided the

opening for reform measures then being pressed by a newly active coalition of established liberal and labor organizations. Though the reforms did not directly effect success, the process entailed by reform did result in a new political environment, one which made a successful challenge possible.

If this analysis is correct, then several assumptions found in the classic literature are misleading. Rather than focusing on fluctuations in discontent to account for the emergence of insurgency, it seems more fruitful to assume that grievances are relatively constant and pervasive. Especially for deprived groups, lack of collective resources and controls exercised by superiors—not the absence of discontent—account for the relative infrequency of organized demands for change. For several of the movements of the 1960s, it was the interjection of resources from outside, not sharp increases in discontent, that led to insurgent efforts.

Nor does the political process centered around insurgency conform to the rules of a pluralist game. The American polity had not been uniformly permeable to all groups with significant grievances (cf. Gamson, 1975). Government does not act as a neutral agent, serving as umpire over the group contest. Public agencies and officials have interests of their own to protect; interests that often bring them into close alignment with well-organized private-interest groups. When insurgency arises threatening these private interests, public officials react by helping to contain insurgency and preserve the *status quo*. But if an opposing coalition of established organizations decides to sponsor an insurgent challenge, the normal bias in public policy can be checked. Sponsors then serve as protectors, insuring that the political elite remains neutral to the challenge.

The implications for other challenges are rather striking. If the support of the liberal community is necessary for the success of a challenge by a deprived group, then the liberal community is, in effect, able to determine the cutting edge for viable changes that conform to the interests of those groups still excluded from American politics. Moreover, there is the possibility of abandonment. Since

liberal support can fade and political elites shift their stance, as has happened to the UFW since 1972, even the gains of the past may be endangered. The prospects for future insurgency, by this account, are dim. Until another major realignment takes place in American politics, we should not expect to see successful attempts to extend political citizenship to the excluded.

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SOCIAL CHANGE AND THE FAMILY: LOS ANGELES, CALIFORNIA, 1850-1870*

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Using Marx's description of "the so-called primitive accumulation" which he associates with the development of capitalism in the West, a theoretical formulation is presented which explores the impact on the family of changes in the individual's access to actual and potential wealth. A multivariate analysis, based on the individual census schedules for the city of Los Angeles in 1850 and 1870, is used to explore the changing relationships between economic, demographic and other structural variables on household structure. The findings suggest that a dynamic, Marxian model can help explain the effects of social change on the family.

Theories of social change in relation to the family have received considerable attention from social scientists. (see, for instance, Parsons, 1959; Linton, 1959; Goode, 1963; Skolnick, 1973: ch. 3; Hareven, 1976). Until recently, however, historical research has been lacking which

would permit an empirical assessment of these theories. This paper, which presents a macro-structural analysis of household organization in a developing nineteenth-century American city, hopes to make a contribution to this assessment and to suggest a new conceptual framework for understanding the family in historical perspective.

Studies of social change can attend not only to outcomes—such as variations in the distribution of family types in different time periods—but also to the processes by which these changes came about. The longitudinal design of the research discussed in this paper provides an opportunity to empirically assess process as well as results. The conclusions of studies of

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process, however, are likely to be varied, in part because of historical and structural differences between units of analysis. Brenner (1976), for instance, presents a brilliant analysis which traces variations in the transition to capitalism in different European societies. The findings of a study of social change in Los Angeles, whose history differs from other (and older) American communities, can be compared to the ways in which other social units experienced change historically. The conceptualization developed in this paper suggests a model for understanding variations in the processes of social change as they relate to the family. A brief description of Los Angeles in the nineteenth century provides the historical context within which this analysis can be understood.

California in the Nineteenth Century

California, which had been ceded by Mexico in 1848, became part of the United States in 1850. At that time, the main economic activity in the southern part of the state was cattle ranching. Although individual ownership of land had been restricted during the period of Spanish domination, under Mexican rule this policy was reversed and private ownership was encouraged. By 1846, nearly all of Southern California's arable and pasture land was owned by private proprietors with holdings running from less than 100 to more than a million acres (Fogelson, 1967:7-8).

Social organization prior to 1850 centered around the life of the ranchos whose production was geared primarily to its own consumption needs. Given a mild climate, the richness of the natural environment, a virtual absence of taxation and an almost cost-free labor force of indigenous Indians, domestic units were able to achieve considerable economic and social autonomy. (see Fogelson, 1967; Cleland, 1969; McWilliams, 1946).

This situation did not last long after the American takeover. By 1870, although the area was still primarily agricultural, considerable land subdivision and diversification of both occupations and economic activities had taken place. Sheep farming

assumed some of the emphasis which had been given previously to cattle and vineyards; and citrus fruit, as well as other types of agricultural produce, began to make a greater mark on the economy than they had done previously.

Commerce and manufacturing also developed during this period. The census of 1850 listed only one product of industry in the area: 42,280 loaves of bread valued at \$8,560 (Newmark and Newmark, 1929:21). By 1870, the value of the products of manufacturing in the county had risen to \$725,036 (U.S. Census Office, 1872:814).

Population growth was also a central feature of the changes that were affecting the city during this period: the population of Los Angeles increased from 1,610 in 1850 to 5,728 in 1870 (Fogelson, 1967:21), and the number of households grew from less than 300 in 1850 to 1,949 in 1870. Migration was a significant factor in the magnitude and rapidity of this population growth. Although the Civil War was a major event on the national scene at this time, its impact in the western states was less than in other areas of the country. An event of greater importance for Los Angeles than the Civil War was the establishment of rail connections to San Francisco and New Orleans, facilitating migration into the area. These factors led to greater ethnic and occupational diversity within the city and an increasing variety in its social institutions.

Within the twenty years under consideration here, Los Angeles developed from an isolated, self-sustaining and largely barter economy to a trade-dependent, money system. As Fogelson (1967:14) describes it, the city was shifting from "self-sufficient agriculture to market agriculture—from varied production for immediate consumption to specialized production for distant consumers."

Social Change and Family Structure

Marx's discussion of "the so-called primitive accumulation" suggests some useful guidelines through which to understand the changes occurring in Los Angeles during the period under investigation. According to Marx (1967:714):

The capitalist system pre-supposes the complete separation of the labourers from all property in the means by which they can realise their labour. . . . The process, therefore, that clears the way for the capitalist system, can be none other than the process which takes away from the labourer the possession of his means of production; a process that transforms, on the one hand, the social means of subsistence and of production into capital, on the other, the immediate producers into wage-labourers. The so-called primitive accumulation, therefore, is nothing else than the historical process of divorcing the producer from the means of production.

This framework draws attention to two interrelated structural changes, which began in Los Angeles, particularly after 1850: (1) changes in the producer's relationship to the economic structure, i.e., their transformation from possessors of the means of production and subsistence into wage workers and (2) consequent changes in the macro-economic structure of social organization, i.e., the transformation of the means of production and subsistence into "capital." From a Marxist perspective, these changes created the preconditions for the development of capitalism in Los Angeles and the eventual incorporation of Southern California into a national (and ultimately international) economic system. The formulation developed here will explore the implications of these structural transformations for understanding changes in the family which the empirical analysis documents.

The decline in proprietorship and increase in the proportion of wage workers in the twentieth century has been noted by many sociological observers. (for instance, Blau and Duncan, 1967). The consequences of these changes in economic organization for the political order and stratification system have been widely discussed. Their impact upon the family, however, has received less attention. Although some scholars have discussed in general and/or theoretical terms the effects of "industrialization" (Parsons, 1959; Linton, 1959), "modernization" (Goode, 1963) and the development of capitalism (Zaretsky, 1973; Shorter, 1975) upon the family, detailed empirical studies

have been rarer.¹ The empirical analysis to follow, however, provides an opportunity to investigate the utility of Marx's conception of "the so-called primitive accumulation" for understanding family organization in nineteenth-century Los Angeles. This will be done by focusing on the dynamics and effects of variation in individual accumulation of wealth as a means of interpreting changes in household structure over time.

It needs to be emphasized that the model presented here does not focus merely on the effects of the changing distribution of individual wealth on the family, but interprets changes in this distribution in the context of changes in the macrosocial structure associated, in Marx's view, with the development of capitalism. While evidence will not be available in this analysis to fully test the formulation presented, a Marxian perspective provides a basis for understanding the process and effects of social change in relation to the family which seems to account, at least in part, for changes in the family that occurred in nineteenth-century Los Angeles.

Two types of individual wealth—actual and potential—are a central focus of the formulation developed here. The unequal distribution of material economic assets—such as real estate—creates variation in the actual wealth which individuals possess. Variation also may occur in the potential for the accumulation of wealth through such things as differential access to the socioeconomic rewards of upward mobility. The data used in this analysis provide three individual-level measures of economic resources: occupation, real estate ownership and personal estate. Real and personal estate will be interpreted as

¹ Few full-length empirical monographs of the family in historical perspective in the early stages of industrialization are available. Some exceptions are Sennett (1970), Anderson (1971) and Katz (1975). Other recent books on the subject, such as Demos (1970) and Greven (1970) deal with the colonial period of American history. Farber (1972) covers a transitional period between the colonial era and the mid-nineteenth century. Hall (1974) looks at the effects of changing business practices on the family between 1700 and 1900.

indicators of actual wealth, and occupation will be used as an indicator of potential wealth. While the meaning of real and personal estate to measure variation in actual wealth is clear, the use of occupation to measure potential wealth requires some elaboration.

Occupational groups vary in terms of the range of opportunities available to their members. For instance, unskilled laborers may have low potential for accumulation from their wages and a limited range of opportunities for mobility while professionals may have higher potential accumulation from earnings but also have a limited range of mobility outcomes, since most movement through the occupational hierarchy is over short distances. (See Blau and Duncan, 1967, for the twentieth-century occupations; Thernstrom, 1964 and Katz, 1975, provide findings which suggest a similar pattern for the nineteenth century.) However, other occupations—proprietors and skilled craftsmen appear to have fallen into this category in the nineteenth century—may have had relatively high (although not equal) potential for earnings but greater variation in the range of mobility outcomes associated with their labor force participation.² Further research is necessary to empirically assess these differences between occupational groups. Conceptually, however, the formulation developed here uses these sources of variation in an individual's access to the accumulation of wealth to understand changes in household and family structure over time.

Both actual and potential accumulation have a temporal dimension which is important for understanding processes of change both through history and over the individual life cycle: prior accumulation can, over time, be reduced, maintained or increased and the potential for accumulation may or may not be realized. The suc-

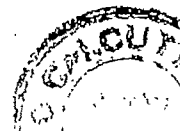
cess of converting the potential for accumulation into actuality (as well as success in maintaining or increasing resources already accumulated) is likely to depend both on attributes of the individual and changes in the macro-social structure which defines the range of opportunities available to the individual.

Historical research has shown that household and family structure is related to individual levels of actual accumulation. Berkner (1972) and B. Laslett (1975) found, for instance, that extended family living was positively related to the wealth of the household head in eighteenth-century Austria and nineteenth-century Los Angeles, respectively. The effect of an individual's potential for accumulation on household structure is more likely to be felt through the burden of dependency, i.e., through the demands which different types of family (and/or household) composition impose upon resources.³ Households with higher burdens of dependency relative to their resources are less likely to accumulate wealth than households with lower dependency ratios. B. Laslett's (1975) analysis of household structure in Los Angeles in 1850 shows that the burden of dependency varies by household type, with the highest burden being found among simple family households, i.e., those which were nuclear in their kinship organization and did not include unrelated persons (such as boarders) as members of the co-resident domestic group.

There are, then, both theoretical and empirical reasons to expect an individual's actual and/or potential for accumulation to explain, at least in part, variation in household structure cross-sectionally and over time in the following ways. (1) Individuals with high actual as well as potential accumulation can afford household organizations with a relatively greater

² See Blau and Duncan (1967: ch. 2) for a discussion of such differences among twentieth-century American occupations. Slosser (forthcoming) is working on a study of intragenerational mobility in late nineteenth-century Los Angeles which will provide data on the mobility potential of different occupational groups.

³ The traditional functionalist paradigm also discusses dependency in relation to social change and the family. From that perspective, however, attention is given to the supposedly greater ease of mobility which nuclear, compared to extended, families permit. It does not discuss the ratio of dependents to resources, which is another feature of the same attribute.



burden of dependency.⁴ (2) The type of co-resident domestic group which would provide persons with high potential for accumulation but low levels of actual wealth with the greatest opportunity to transform their potential into actuality would be those in which there was a low dependency ratio. This could be achieved by postponing either marriage or child bearing or by limiting fertility. (3) The household structure of persons with high actual and low potential accumulation would, over time, be expected to reflect attempts to lower their burden of dependency. (4) Individuals with low actual and low potential accumulation might be more likely to view the benefits of family life in terms other than those having to do with the accumulation of wealth.

An additional elaboration is required in order to discuss life-cycle variation in household organization. To suggest that some social categories have greater potential for accumulation than others is not to maintain that all members of that category actualize their potential. Participation in a household or family structure meant to maximize accumulation does not ensure that accumulation will occur. There should, therefore, be life-cycle variation in household structure which reflects changes in the types of wealth the individual possesses—with potential being a more powerful predictor early in an individual's life and actual accumulation being a more powerful predictor later in the life cycle.

If this formulation is correct, the distinction between actual and potential accumulation provides one basis for a dynamic interpretation of the relationships between individual attributes and household organization. The effect of the social changes at the macro-structural level which Marx identified—the separation of the means of production and subsistence from the direct producers and the consequent emergence of “capital” (and capitalism) on the one hand and wage workers on the other—should affect the distribution and direction of these rela-

tionships over time through their impact on the range of opportunities which structure the individual's access to accumulation.

Methodology

The data for this analysis were taken from the individual census schedules for the City of Los Angeles in 1850 and 1870.⁵ Some of the problems involved in working with these data have been discussed elsewhere (B. Laslett, 1975). They will be reviewed briefly below, followed by additional issues relevant to the present analysis.

One of the features of studying family structure through census records is that the household, rather than the family, is the unit of analysis. Family structure as measured here reflects relationships among persons linked by conjugal or parent-child ties who shared a given residence on the date of census enumeration. It excludes persons who may be part of the same kinship group but who live in other households. This limitation could provide findings which truncate the actual kinship linkages among families who lived in Los Angeles during the period under investigation.

The most serious problem involved in using the U.S. federal census schedules to study family structure prior to 1880 is the absence of information on intra-household relationships through which the household structure variable is constructed. It was necessary, therefore, to infer these relationships through a series of coding rules based on surname identification and the instructions circulated to the federal census enumerators which specified the way in which different types of household members were to be listed (see Wright, 1900; B. Laslett, 1975). Persons with the same surname as the household head were coded as kin, and the order in which they were listed as well as sex and age were used to infer the specific nature of the relationship. Persons with different sur-

⁴ In 1850, extended family households were found to have a dependency ratio only slightly lower than simple family households (see B. Laslett, 1975).

⁵ The 1850 data were available in Newmark and Newmark (1929). The 1870 data were derived from microfilm of the original census schedules obtained from the National Archives in Washington, D.C.

names were coded as unrelated to the household head.⁶

This coding system is admittedly crude and, in particular, may underestimate the presence of kin related along the female line. In view of the limitations of the data, no coding system which eliminates this deficiency could be devised. The 1850 and 1870 schedules were coded in the same way and, while there may be error in inferring some intra-household relationships, the results are comparable.

For 1850, the entire population of households within the city was included in the analysis. The population had grown sufficiently large by 1870 so that sampling was appropriate and approximately 1,000 ($N=1,003$) were randomly chosen from the total number ($N=1,949$) listed on the census schedules. A final N of 929 from this sample forms the basis of the 1870 analysis.⁷

All intra-household relationships and household types in the 1850 data received a reliability score which indicates the degree of agreement between two independent coders.⁸ Eighty percent of the relationships and household types in 1850 received a code indicating the highest

inter-coder agreement. Inferences on the 1870 data therefore were checked through sampling. During the first coding operation (done by the author), each household type and each relationship within that household was given a confidence code of 1, if the coder was virtually certain of the inference made in terms of the coding rules; of 2, if there was considerable confidence in the inference made but some uncertainty (usually due to some minor deviation from the coding rules or when randomization had been used; see footnote 7); and 3, if there was considerable uncertainty about the inference made.

All of the 1870 households which received confidence codes of 2 and 3 and a five percent sample of those which received confidence codes of 1 were double coded and the two codings then were compared. In virtually every case in which the original coder had assigned a confidence code of 1, the second coder agreed. For households and relationships which had received a confidence code other than 1, the final code reflects an inter-coder consensus. All households receiving a confidence code of 3 were excluded from the analysis, and the confidence code itself is one of the independent variables included within it.

The following information is available in the 1870 census schedules: the number of each dwelling house and family in order of visitation, the name of each person whose place of abode was within that family, their age, sex, color and occupation,⁹ the dollar value of the real estate and personal estate owned by each person listed (as given by the respondent), their place of birth as well as whether their mother and father were foreign born, the month of their birth or marriage (if born or married within the census year), whether or not they could read or write, whether they were deaf and dumb, blind, insane or idiotic, whether they were male citizens of the U.S. of 21 years of age or older and whether or not, among this group, their

⁶ Coding all persons with different surnames as unrelated could underestimate actual family relationships by excluding persons involved in common-law unions and children from previous marriages whose surname was that of their natural father rather than their stepfather. In order to overcome this difficulty, the following procedure was used: where there was good reason to believe that one or the other of the two possibilities mentioned had occurred (which was usually suggested by the order in which persons were listed on the schedules), they were randomly assigned to the kin or non-kin category.

⁷ This N represents the deletion of 22 households headed by Orientals whose names were incorrectly listed so that it was impossible to infer relationships from them, 35 households headed by blacks and Indians, and 18 households whose structure was indeterminable. Most of the Indian population resident within the city in 1870 was listed on separate schedule sheets and insufficient information made it impossible to distinguish households from each other or to infer relationships. Therefore, it was necessary to exclude them from the analysis.

⁸ I wish to acknowledge with thanks the work of John E. Lawson, Jr. and James E. Smith, who acted as second coders for the 1850 and 1870 data, respectively. Smith also made a major contribution to the analysis presented in this paper.

⁹ Only the occupations of males 15 years of age and older were collected in 1850. In 1870, most women reported their occupation as "keeping house." In the analysis of occupations, both responses—none in 1850 and "keeping house" in 1870—were treated as NAs.

right to vote had been denied or abridged on any grounds other than rebellion or crime.

The first ten items were also available on the 1850 schedules, with the exception of personal estate information. This paper will, in the first instance, present the results of analyzing comparable material for 1850 and 1870, aggregating the second data set into the same categories used to analyze the earlier one. The second set of findings for 1870, which includes some of the additional information available on the schedules for that year, is based upon categories more appropriate to the distributions within the sample.¹⁰

The analysis which follows looks at four dependent variables which represent households with varying types of structure.¹¹ They are non-family households, simple family households, simple family households which also included additional non-kin (to be referred to as "simple family plus others" households) and extended family households. The non-family households, as defined here, were those without marital or parent-child ties. They include persons living alone as well as households whose members were coded as being unrelated to the household head.¹² Simple family households were composed either of husband and wife, husband, wife and child(ren), or single parent and child(ren) families. The "simple family plus others" households were made up of a simple family, as defined above, plus other persons coded as unrelated to the head which might include servants, boarders, employees or any persons with different surnames. Extended family households were composed of a

simple family plus any additional relatives; they might also include unrelated "others." It should be remembered that since kin relationships were determined primarily through surname identification, the non-family and "simple family plus others" households may include some units that were extended along the female line.

The analysis relies primarily on the results of dummy variable regression techniques which have been converted into the class and adjusted means generated by Multiple Classification Analysis (see Andrews et al., 1975). Both the dependent and independent variables were treated in their dummy variable form and a separate equation for each type of household was calculated. Tables 1, 2 and 3 include all four dependent variables: each household type differentiated from all others.¹³ Statistically, the means presented in these tables were derived by computing the deviation of the category mean from the grand mean for each dependent variable and then adding that deviation to the relevant grand mean. The deviations of the category means from the grand mean are equivalent to unstandardized dummy variable regression coefficients. Possible interaction effects between independent variables have not been explored.

Tables 1, 2 and 3 report the class and adjusted means for each independent variable in relation to each type of household organization. Class means indicate the zero-order relationships between the independent and dependent variables, while the adjusted means represent the relationships that exist when all other variables in

¹⁰ B. Laslett (1975) analyzes the data for Los Angeles City and County combined. The 1850 data analyzed here is for the city alone. The only boundary changes affecting the city between 1850 and 1870 was an extension of 1.2 miles at its southern end in 1859 (see L.A. City Engineering Dept., 1928).

¹¹ See P. Laslett (1972) and Hammel and Laslett (1974) for a discussion of these household categories. Some deviations from the P. Laslett-Hammel typology have been used for analyzing the Los Angeles data (see B. Laslett, 1975).

¹² This category also includes households in which siblings were living without parents or spouse, since such units would contain neither conjugal nor parent-child ties; N=3 in 1850 and N=31 in 1870.

¹³ Knoke (1975) discusses some of the problems of using regression techniques on dummy dependent variables when the distribution is highly skewed. Under these conditions, an independent variable can have a negative effect on a dependent variable, which would be statistically meaningless. Knoke believes that regression techniques should only be used when each category of the dummy variable includes at least 25% of the cases. The data used in this analysis do not always meet this criterion. In 1850, only 16% of the households had an extended family structure and in 1870, 16% and 7% of the households were "simple family plus others" and extended, respectively. Although two negative results are generated in the analysis, they are extremely small. Furthermore, they are adjusted means, not actual ones, which reduces the problem of their interpretation.

the equation were controlled. When the dependent variable is dichotomous, as is the case here, the means can be read as the proportion or percent of households in that category of the independent variable that were (in the case of the class means) or would have been (in the case of the adjusted means) living in the specified type of household, and comparisons can be made across household types. Tables 1 through 3 also report significance levels, which indicate when a given class or adjusted mean is significantly different from the grand mean for that dependent variable.¹⁴ The results discussed in the text refer, except where indicated otherwise, to statistically significant adjusted means.

Extended family households were significantly more likely to have received lower reliability and confidence codes than any other household type. For this reason, it is important to use caution in accepting the results of this part of the analysis. Such a concern, however, need not affect consideration of the other household types. The individual characteristics analyzed are those of the household head—not of all individuals resident within a household. Distributions of these characteristics, therefore, refer to household heads only.

Family Structure in Los Angeles, 1850 and 1870

Comparing the distributions of household types in 1850 and 1870 (see the grand means in Tables 1 and 2) indicates some interesting changes over time. In general, there was an increase in the proportion of households with simpler structures and a decrease in the proportion of those with more complex structures. Non-family households increased from 23 percent to 31 percent, and simple family households increased from 29 percent to 46 percent. Simple family households which included

non-kin declined from 32 percent to 16 percent, and extended family households declined from 16 percent to seven percent. The nuclear or simple family was clearly the central form of family organization in both periods.

Economic Factors

The distribution of occupations¹⁵ in 1850 presented in Table 1 shows that 55 percent of the households were headed by persons in occupations not specifically agricultural (such as coopers, grocers, carpenters, blacksmiths, lawyers, physicians, merchants, etc.) while 18 percent and 19 percent were laborers and ranchers or farmers, respectively. Despite the economic growth and increased occupational differentiation within the city in 1870 compared to 1850, a smaller proportion of household heads were in non-agricultural occupations in the later period. The proportion of farmers increased slightly (from 19 percent to 21 percent) while the proportion of laborers almost doubled (from 18 percent to 30 percent). If the interpretation associated with occupation is correct, the potential for accumulation of wealth was reduced in the twenty years under consideration—was concentrated in the hands of a smaller proportion of the population (of household heads); changes consistent with the development of "capital" as Marx described it.

In terms of actual wealth as measured by real estate ownership, Table 1 shows that more than half of the population of household heads in 1850 owned some land, although the proportion owning land of the highest value (\$3,000 and over) was only 16 percent. A higher proportion of

¹⁴ These were computed from Andrews et al. (1975:51). Thanks are given to Susan Shuckett for her work on this part of the project. It should be noted that the significance tests computed on the difference between a category mean and the relevant grand mean are not independent between equations, since the four dependent variables are, in fact, different categories of a single nominal variable.

¹⁵ A set of almost 100 categories were used to code the occupational titles listed on the census schedules. They reflect differences in skill, status, income and education and are comparable to SES scales for modern occupations. In the 1850 and comparable 1870 analysis, persons reporting themselves as agriculturalists, farmers or ranchers were grouped together; persons reporting themselves as laborers were differentiated from other occupational titles and all other occupations listed were grouped together into a non-agricultural category. This last group was differentiated into professional and white-collar and craftsmen and semi-skilled workers for the reanalysis of the 1870 data.

Table 1. Effects of Seven Independent Variables on Four Types of Household Structure: Los Angeles, 1850 (N=257)

Independent Variables		%	N	Household Type							
				Non-Family (N=60)		Simple Family (N=75)		Simple Plus Others (N=81)		Extended (N=41)*	
Characteristics of household (reliability) and household head				Class Mean	Adjusted Mean	Class Mean	Adjusted Mean	Class Mean	Adjusted Mean	Class Mean	Adjusted Mean
Reliability:											
1	76		196	.278 ^a	.249	.263 [§]	.281	.342	.343	.117	.127
2	24		61	.098 ^a	.183	.377	.327	.230	.225	.295 ^o	.264 ^a
Sex:											
Male	82		210	.248	.264	.295	.293	.335	.330	.119	.114
Female	18		47	.170	.099 ^o	.277	.287	.213	.251	.340 ^a	.363 ^a
Age:											
Under 30	14		37	.568 ^a	.467 ^a	.135	.190	.189 ^b	.203 ^a	.108	.140
30-39	37		96	.229	.208	.292 ^a	.300	.385	.386	.094	.107
40-49	23		59	.203	.230	.271	.240	.356	.364	.169	.166
50-59	16		40	.050 ^a	.084 ^a	.475 ^a	.480 ^a	.275	.250	.200	.186
60 plus	10		25	.120 ^b	.234	.280	.233	.200 ^a	.201 ^a	.400 ^a	.331 ^a
Real Estate \$:											
None	47		120	.425 ^a	.386 ^a	.242 ^a	.279	.208 ^b	.190 ^o	.125	.145
Less than \$3000	37		95	.074 ^a	.118 ^c	.389 ^a	.357	.358	.358	.179	.167
\$3000 and over	16		42	.048 ^a	.059 ^a	.214	.181 ^a	.524 ^a	.577 ^a	.214	.184
Ethnicity:											
Spanish-surname	70		181	.138 ^a	.239	.354 ^a	.258	.320	.357	.188	.146
Anglo	30		76	.461 ^a	.221	.145 ^o	.373	.303	.215 ^a	.092	.192
Occupation:											
Agriculturalists-farmers	19		49	.041 ^a	.152	.388	.373	.367	.245	.204	.229
Laborers	18		47	.085 ^a	.048 ^a	.447 ^a	.399 ^a	.362	.403	.106	.150
Others	55		142	.345 ^a	.342 ^a	.218	.239	.275	.285	.162	.134
N.A.	07		19	.263	.091 ^o	.211	.213	.368	.501 ^a	.158	.195

Table 1. Continued

Independent Variables	Household Type												
	%	N	Non-Family (N=60)			Simple Family (N=75)			Simple Plus Others (N=81)			Extended (N=41)*	
			Class Mean	Adjusted Mean		Class Mean	Adjusted Mean		Class Mean	Adjusted Mean		Class Mean	Adjusted Mean
Characteristics of household (reliability) and household head													
Birthplace:													
California	39	101	.109 ^a	.225		.356	.348		.327	.277		.208	.149
Mexico	25	65	.138 ^a	.141 ^a		.400 ^a	.435 ^c		.277	.228		.185	.195
Other	35	91	.440 ^a	.308		.143 ^a	.127 ^a		.330	.419 ^a		.088	.145
Grand Mean				.234			.292			.315			.160
R ²				.372			.141			.134			.141

^a $p < .05$; ^b $p < .02$; ^c $p < .01$; ^d $p < .002$. Significance levels refer to differences between that category of the independent variable and the grand mean.

* The small N in this category suggests the need for caution. The data, however, refer to the population (not a sample) of household heads.

household heads was landless in 1870 than in 1850, despite the fact that land subdivision was already well underway. (see Tables 1 and 2). This may indicate the decreasing importance of land in a more urbanized environment, but since Los Angeles' economy was still primarily agricultural, it also implies that fewer households owned the means of production of their own subsistence, which had been such an important feature of California's earlier history.

In 1850, household heads with no land were most likely to live in non-family households (and those who owned any amount of land were significantly less likely to do so). Few landowners with estates of the highest value headed simple family households, and a significantly higher proportion than any other land-owning group headed simple family units which included additional non-kin. The latter relationship was found also in 1870. Although household heads owning the largest amount of land in 1870 were least likely to head non-family households, the proportions (rather than the significance levels alone) indicate that more land owners headed non-family households in 1870 than in 1850.

The relationship between land ownership and heading an extended family household changed in the twenty years under consideration: in 1850, whether or not the household head owned land was not significantly related to heading this type of residential unit. In 1870, household heads who owned land of the highest value were significantly more likely to head extended families. In the earlier period, landless as well as land-owning household heads had access to the means of subsistence for extended family living; this was no longer the case twenty years later.

In terms of variation in potential for the accumulation of wealth as measured by occupation, laborers in 1850 (see Table 1) were least likely to head non-family households and more likely to head simple family households than any other occupational group; non-family households were most often headed by persons with non-agricultural occupations. Having a specific occupational skill can be viewed

Table 2. Effects of Seven Independent Variables on Four Types of Household Structure: Los Angeles, 1870 (N=929)*

Independent Variable	%	N	Household Type							
			Non-Family (N=282)		Simple Family (N=428)		Simple Plus Others (N=150)		Extended (N=69)	
Characteristics of household (reliability) and household head			Class Mean	Adjusted Mean	Class Mean	Adjusted Mean	Class Mean	Adjusted Mean	Class Mean	Adjusted Mean
Confidence Code:										
1	90	833	.315	.308	.471	.475	.161	.161	.054 ^a	.056
2	10	96	.208 ^a	.266	.375 ^c	.337 ^a	.167	.165	.250 ^a	.233 ^a
Sex:										
Male	86	799	.294	.333 ^c	.464	.412 ^a	.166	.163	.075	.092
Female	14	130	.362 ^a	.121 ^a	.438	.761 ^a	.131	.155	.069	-.036 ^a
Age:										
Under 30	20	185	.519 ^a	.494 ^a	.295 ^a	.289 ^a	.124	.144	.059 ^a	.074
30-39	35	328	.271	.265	.500	.490	.162	.167	.067 ^a	.078 ^a
40-49	27	250	.220 ^a	.227 ^c	.520 ^a	.536 ^c	.200	.185	.060 ^a	.052 ^a
50-59	11	101	.218 ^c	.254	.525 ^a	.516	.168	.158	.089 ^a	.072
60 plus	07	65	.308	.331	.400 ^a	.426	.108	.099 ^a	.185 ^a	.145 ^a
Real Estate \$:										
None	63	581	.358	.352	.456	.445	.126	.140	.060	.063
Less than \$3000	25	230	.265	.280	.461	.485	.191	.162	.083	.073
\$3000 or more	13	118	.110 ^a	.111 ^a	.483	.492	.280 ^a	.265 ^a	.127	.133 ^a
Ethnicity:										
Spanish-surname	38	354	.237 ^c	.240 ^c	.503	.542 ^a	.153	.142	.107	.076
Anglo	62	575	.344 ^a	.343 ^a	.435	.410 ^c	.167	.174	.054	.073
Occupation:										
Agriculturalists-farmers	21	191	.178 ^a	.207 ^a	.429	.446	.261 ^a	.248	.126	.099
Laborers	30	278	.295	.254	.525 ^b	.569 ^a	.126	.143	.054	.034
Others	35	328	.354 ^a	.296	.442	.503	.143	.137	.061	.065
N.A.	14	132	.379 ^c	.567 ^a	.417	.149 ^a	.129 ^a	.136 ^b	.076	.147 ^b
Birthplace:										
California	17	159	.277	.279	.434	.408	.189	.217	.101	.096
Mexico	20	182	.214 ^a	.244 ^a	.538 ^c	.469	.126	.172	.121	.115
Other	63	588	.338	.329	.444	.473	.165	.143	.053	.056
Grand Mean			.304	.304	.461	.461	.162	.162	.074	.074
R ²			.122	.122	.057	.057	.039	.039	.083	.083

* See note on Table 1 for levels of significance.

as a resource which might enable migration to a more favorable labor market if the local one was not satisfactory. If involvement in kinship ties was thought of as an impediment to mobility, family formation may have been postponed. Laborers, however, had fewer occupational skills. Migration, for these workers, was less likely to have the same potential for improving their economic position compared to non-migration. Thus, they may have been less willing to limit their family formation.¹⁶

In 1850, farmers were not significantly more likely than the average to head one type of household rather than another. This finding suggests that the functionalist explanation for the presence of a high proportion of extended family structures in past agricultural societies requires some specification. An extended family, the argument goes (see Parsons, 1959; Linton, 1959), provided a necessary agricultural labor force in the past; decline in the demand for this resource under more urbanized conditions reduced its functional necessity and, therefore, its frequency. The findings presented in Table 1, however, remind us that (1) agricultural activities vary in the degree to which they are labor-intensive; (2) that if land was plentiful and/or other economic alternatives were available, extended family living may not have been as necessary; (3) that several mechanisms are available for satisfying a demand for labor. High fertility¹⁷ and the utilization of an indigenous or migratory population of non-kin can substitute for the labor which extended family members could provide.

Los Angeles was particularly well-favored in terms of the availability of an almost cost-free Indian population which, after the secularization of the missions established by the original Spanish coloniz-

ers of the area, left a population with few skills either for self-maintenance or the ability to return to tribal life (see Fogelson, 1967; McWilliams, 1946). Indians formed an important part of the labor force on the Mexican ranchos prior to the American take-over and continued to do so through the early years of the American administration. Cleland (1969:58) describes the situation as follows:

As early as 1850 the *ayuntamiento*, or town council, confronted by the problem of feeding the multitude of Indian prisoners arrested every week—none of whom had enough money to pay even a nominal fine—hit upon the happy expedient of farming out the services of such prisoners to the highest bidders. Landowners, especially those who had large vineyards, quickly took advantage of the opportunity, and soon the use of enforced Indian labor on the ranchos became a matter of common practice. . . .

Under these circumstances, kin were not as necessary for pursuing agricultural activities as they might have been and, as Table 1 indicates, farmers were not significantly more likely than the average to head extended family households.¹⁸

The interpretation of these empirical results suggests that between 1850 and 1870, the importance of actual wealth increased and the importance of potential wealth declined. In the later period compared to the earlier one, persons with non-agricultural occupations (in contrast to the other occupational groups) were no more likely to head one type of household than another. Whether this reflects increasing opportunities within the growing city of Los Angeles so that migration—and postponing family formation—was no longer as important or as necessary for accumulation, or whether this finding reflects a decline in the degree to which potential would result in actual wealth cannot be answered by the data used in this analysis.

Some support for the second explanation is provided by the following quotation from a local newspaper in 1869 (Cleland, 1969:59):

¹⁸ It should be noted, however, that farmers were more likely to head extended family households than any other occupational group.

¹⁶ One way to test this interpretation would be to see if marriage age was lower for laborers than for persons with more specific occupational skills (net of other factors). Census data do not permit an analysis of this question. Research on the structure of marriage in nineteenth-century Los Angeles is now in progress.

¹⁷ This analysis will not discuss fertility. Attention to this subject is planned for the future.

Table 3. Effects of Eleven Independent Variables on Four Types of Household Structure: Los Angeles, 1870 (N=929)*

Independent Variable		%	N	Household Type							
				Non-Family (N=282)		Simple Family (N=428)		Simple Plus Others (N=150)		Extended (N=69)	
				Class Mean	Adjusted Mean	Class Mean	Adjusted Mean	Class Mean	Adjusted Mean	Class Mean	Adjusted Mean
Characteristics of household (reliability) and household head											
Confidence Code:											
1		90	833	.315	.308	.471	.475	.161	.161	.054 ^a	.156
2		10	96	.208 ^a	.265	.375 ^a	.335 ^a	.167	.165	.250 ^a	.235 ^a
Sex:											
Male		86	799	.294	.332 ^b	.464	.414 ^a	.166	.164	.075	.090
Female		14	130	.362 ^a	.129 ^a	.438	.745 ^a	.131	.148	.069	-.023 ^a
Age:											
Under 30		20	185	.519 ^a	.489 ^a	.295 ^a	.289 ^a	.124	.145	.059	.077
30-39		35	328	.271	.266	.500	.489	.162	.168	.067	.078
40-49		27	250	.220 ^a	.229 ^a	.520 ^a	.535 ^a	.200	.186	.060	.050
50-59		11	101	.218 ^a	.254	.525 ^a	.520 ^a	.168	.156	.089	.070
60 plus		07	65	.308	.334	.400 ^a	.426	.108	.093 ^a	.185 ^a	.148 ^b
Real Estate \$:											
None		63	581	.358 ^a	.339	.456	.448	.126	.141	.060	.072
Less than \$3000		25	230	.265	.301	.461	.470	.191	.158	.083	.071
\$3000 and over		13	118	.110 ^a	.207 ^a	.483	.505	.280 ^a	.270 ^a	.127	.093
Personal Estate \$:											
None		51	474	.376 ^a	.353 ^a	.449	.442	.124	.159	.051	.047
\$100-199		11	105	.229 ^a	.239 ^a	.562 ^a	.552 ^a	.162	.157	.048	.053
\$200-499		15	143	.210 ^a	.234 ^b	.503	.513	.196	.173	.091	.081
\$500-1999		13	120	.267	.275	.367 ^a	.396 ^a	.250 ^a	.204	.117	.125
\$2000 plus		09	87	.207 ^a	.270	.460	.459	.184	.102 ^a	.149 ^b	.169 ^a
Ethnicity:											
Spanish-surname		38	354	.344 ^a	.232 ^a	.435	.543 ^a	.153	.144	.054	.081
Anglo		62	575	.237 ^a	.348 ^b	.503	.410 ^a	.167	.173	.107	.070

Table 3. Continued

Independent Variable	Household Type							
	Non-Family (N=282)		Simple Family (N=428)		Simple Plus Others (N=150)		Extended (N=69)	
	%	N	Class Mean	Adjusted Mean	Class Mean	Adjusted Mean	Class Mean	Adjusted Mean
Occupation:								
Characteristics of household (reliability) and household head								
Profess'l/white collar	20	181	.337	.308	.403 ^a	.458	.182	.174
Farmers	21	191	.178 ^a	.234 ^b	.429	.436	.267 ^a	.238 ^c
Craftsmen and semi- skilled workers	16	147	.374 ^b	.295	.490	.548 ^c	.095 ^a	.100 ^a
Laborers	30	278	.295	.246 ^a	.525 ^b	.567 ^d	.126	.142
N.A.	14	132	.379 ^c	.531 ^d	.417	.179 ^d	.129	.142
Birthplace:								
California	17	159	.277	.314	.434	.365	.189	.213
Mexico	20	182	.214 ^d	.202 ^d	.538 ^c	.511	.126	.175
Other US	29	268	.313	.355 ^b	.444	.432	.187	.151
Other	35	320	.359 ^a	.313	.444	.504	.147	.137
Literate:								
No	25	230	.248 ^a	.325	.500	.443	.157	.165
Yes	75	699	.322	.297	.448	.467	.163	.160
Foreign-born Father:								
No	41	381	.281	.233 ^c	.462	.563 ^d	.189	.155
Yes	59	548	.319	.353 ^b	.460	.390 ^d	.142	.166
Foreign-born Mother:								
No	44	404	.287	.327	.450	.418	.191	.173
Yes	56	525	.316	.286	.469	.494	.139	.153
Grand Mean			.304	.304	.461	.461	.162	.162
R ²			.134	.134	.073	.073	.047	.047

* See note on Table 1 for levels of significance.

"The Los Angeles *Semi-Weekly News* condemned the system of using enforced Indian labor, not on humanitarian grounds, but because degraded and inefficient Indians were 'being brought into competition with that class of labor that would prove most beneficial to the country,' checks immigration, and retards the prosperity of the country.'"

Nineteenth-century Los Angeles was successful in attracting "more efficient" labor through high rates of "Anglo" immigration. If a large proportion of these new in-migrants replaced the Indian laborers used earlier, they were less likely to have the same potential for further mobility that might have been available to them in the earlier period.

Table 3 presents a reanalysis of the 1870 data which includes personal as well as real estate among the independent variables.¹⁹ The results provide additional support for interpreting changes in household structure in terms of the economic preconditions necessary for the development of capitalism included in Marx's discussion of "the so-called primitive accumulation." Personal estate is more moveable wealth than real estate and, as such, can be viewed as a potential source of capital rather than simply as a means of subsistence. The transformation of the means of production into capital is, as we have seen, a central feature of Marx's analysis of the development of capitalism. Its impact on household organization is, then, of particular importance for the interpretations presented in this paper.

¹⁹ Table 3 includes four new independent variables: a valuation in dollars of the household head's personal estate, literacy, and whether the head's mother and father were foreign-born or native-born. (Literacy information was available on the 1850 schedules but was not included in the original analysis.) Some of the data analyzed earlier has been aggregated into different categories which reflect changes in the distribution of these characteristics over the two decades.

The effects of personal estate on household structure are discussed in the text. None of the other additional variables add much to understanding household organization in 1870 Los Angeles except the effect of father's birthplace. A native-born (compared to foreign-born) father seems to have been an "inheritance" which provided a greater opportunity for family formation. Mother's birthplace, however, was not important in this respect. Literacy did not have an effect.

Table 3 shows that the value of the real estate owned by the household head had a significant effect only in relation to the non-family and "simple family plus others" households and did not affect the extended family unit. This difference can be accounted for by the availability of the value of the household head's personal estate in the analysis presented in Table 3.²⁰ The impact of actual wealth on heading an extended family household reflects the relationship between the highest valuation of personal estate, not land, the effect of which disappeared when personal wealth was entered into the equation. Since these two indices of wealth are highly correlated ($r=.301$; $p<.001$), the distinction between them was obscured in the analysis presented in Table 2. Wealth appears to have been necessary in order to maintain complex households, as Goode (1963) has suggested. In nineteenth-century Los Angeles, however, extended family households do not seem to have been the result of landowning and/or the agricultural services which extended kin might perform; it was due, rather, to the availability of personal estate which could be used to hire wage laborers as well as to support the additional burden of dependency which extended kin might represent.

Table 3 also allows us to see changes in the relationship between occupation and household structure when personal estate is included in the analysis and the non-agricultural category is differentiated into white-collar and upper blue-collar occupations. This analysis of the 1870 data shows both farmers and laborers to be less likely than the average to head non-family households while laborers, as well as more skilled manual workers, were more likely to head simple families. While the analysis in Table 2 shows that household heads in non-agricultural occupations were not more likely to head one type of household organization than another, this was in fact due to the effect of the white-collar group within this category; house-

²⁰ Enumerators were instructed to list the value of the personal estate only if it was \$100 or more (see Wright, 1900). The categories presented in Table 3 reflect that instruction.

hold heads in upper blue-collar occupations were not only more likely to head simple family households but they were less likely to head simple families that included additional non-kin. It was the household heads in white-collar occupations only who were no more likely to head one kind of household rather than another. If occupational groups differ in their potential for accumulation and this variation affects household organization (as the conceptual model suggests), its impact was felt among manual but not white-collar workers—at least in the stage of development that Los Angeles had reached by 1870.

In the second analysis of the 1870 data, farmers remained more likely than the average to head households which included "others"; non-kin appear to have been the primary labor supplement to nuclear family members among farm households, regardless of their personal wealth. Craftsmen and semi-skilled workers were less likely to head households which included additional non-kin, while laborers were not significantly different from the average in this respect. These findings suggest that the wage levels of laborers may have been sufficiently low to lead some of them to supplement their incomes by taking in boarders while this appears to have been less true for other, perhaps more highly skilled and better paid manual workers. (Income data, not available on the census schedules, is obviously necessary to test this interpretation directly.) Thus, while the potential for accumulation through mobility may have become more equal between manual workers, this was not necessarily true for their actual wealth from wages.²¹

Looking at the effects of personal estate itself, Table 3 shows that household heads with no personal wealth were more likely than the average to head non-family

households, while most groups who owned any personal estate at all were less likely to do so. This finding suggests that the increased proportion of non-family households in 1850 compared to 1870 was due, at least in part, to a larger proportion of poor people in the population, i.e., those without any personal wealth. This was also true, as Table 1 indicates, of household heads who owned no real estate in 1850 but not in 1870. That wealth measured by personal estate should have become a more important determinant of household structure than wealth from real estate in 1870 compared to 1850 is not surprising given the more urbanized nature of the community in the later period. The changing distribution of household types over time, however, seems to have been due to the decreasing availability of wealth of any kind.

Household heads with the smallest amount of personal estate were more likely to head simple family households, while the opposite was true for the wealthier group. Owning the greatest amount of personal wealth was positively related to heading an extended family household and negatively related to heading a simple family household that included non-kin but not extended kin. Despite the economic capacity for resident servants in the wealthier households, only four percent of all the households included in this analysis had servants specifically mentioned as resident members.

The economic variables included in this analysis have provided the basis for interpreting changes in household structure in nineteenth-century Los Angeles that are consistent with macro-economic developments which Marx associated with the growth of capitalism. Changes in an individual's actual and/or potential for the accumulation of wealth, it was suggested earlier, is one way to link structural changes associated with "the so-called primitive accumulation" and household organization. While questions might be raised about the measurement of individual accumulation in the data analyzed here, the findings provide significant and consistent support for the interpretations suggested by the formulation developed in this paper.

²¹ An increasing differentiation in actual wealth among manual workers is likely to be of considerable importance for understanding working-class behavior, especially as it may relate to the development of a split labor market (see Bonacich, 1972). Tygiel (forthcoming) discusses some of the effects of working-class differentiation on household composition, residence patterns and politics in nineteenth-century San Francisco.

The Family Life Cycle

Recent historical research (see, for example, P. Laslett, 1972; B. Laslett, 1975; Demos, 1968; 1970; Greven, 1966; 1970; Parish and Schwartz, 1972) has demonstrated the predominance of nuclear family households in preindustrial agricultural societies. One explanation for these findings is demographic (Burch, 1972; Levy, 1965; Berkner, 1972). Societies with late marriage ages and high mortality, characteristic of western Europe in earlier times (Hajnal, 1965), were limited in the degree to which extended families could occur, except for brief periods at the beginning and/or end of the family life cycle. Variation in demographic factors is, therefore, important for understanding changes in household structure over time. In relation to the economic interpretation presented in the preceding section, it is necessary to see whether and how demographic factors are related to them.

Limitations of the data used here make it impossible to address directly several of the issues mentioned above. Neither mortality information nor age-at-marriage are available on the census schedules.²² Previous historical research on the family, however, has used the age of the household head as a way to investigate the family life cycle (see, for instance, Berkner, 1972; Herlihy, 1972; B. Laslett, 1975). This procedure has been followed in the analysis presented here. The relationships between the age of the household head and family structure in both 1850 and 1870 indicate the effects of social change on the family life cycle.

The youngest household heads were more likely to head non-family house-

holds in both 1850 and 1870. In 1850, however, heading a simple family was a feature of late middle age, persons 50-59 years old were the only group significantly more likely than the average to do so. The proportion of simple family households headed by all persons 30 years of age or older, however, increased between 1850 and 1870, and only households headed by those in the 40-49 age group were significantly more likely than the higher average to head them.

The previous section suggested that the potential for the accumulation of wealth declined between 1850 and 1870. Actual wealth is also likely to have narrowed; a smaller proportion of household heads were landowners and the percent of household heads in unskilled labor occupations increased. Since wealth was associated with more complex household types, the decreasing access to this economic resource could, in part, explain the changing distribution of household types over the twenty-year period. The changing relationships between age and household type provide a further explanation for these different distributions. Heading a simple family household was no longer one phase of a differentiated pattern of family life-cycle stages but was the dominant organizational form of family life throughout its entire span. In 1870 as in 1850, the oldest age group was significantly more likely to head an extended family unit but the proportions (in their adjusted form) had declined from 33 percent to 14.5 percent. Furthermore, the simple family was the modal category for the eldest as it was for all but the youngest household heads in 1870, which was not the case in 1850.

One further observation can be made when looking at the changing effects of age on household organization over this twenty-year period. The smaller proportion of simple families which included additional non-kin in 1870 compared to 1850 suggests that persons who had not formed family alliances of their own in 1850 were, nevertheless, likely to live within a family (compared to a non-family) context; by 1870, such persons were more likely to remain outside of a familial unit throughout their entire adult lives.

²² Data on marital status were not available on the U.S. federal census schedules until 1880. It is therefore impossible to know how many persons heading non-family households were divorced, separated or widowed. Since non-family households did not include a marital link to the head, some inference can be made about the marriage ages of non-family household heads: 56.8% of all households headed by persons under 30 were non-family in 1850 and 51.9% were non-family in 1870. This indicates that residents of Los Angeles also may have followed the western European pattern of later marriage ages than has commonly been assumed.

If migration potential was considered to be a resource for the accumulation of wealth and kinship ties were seen as antagonistic to it, more persons may have postponed marriage in the hope of improving their economic positions. For those who were unsuccessful in turning potential into actual accumulation (and it was the least wealthy who were most likely to head non-family households), their attractiveness within the marriage market was likely to decline with age. Thus, what may have been a decision to postpone marriage in earlier years could have resulted in a limited ability to establish one's own family or to live within a family at all at a later age.²³

In general, then, there seem to have been two major shifts in the life cycle of nineteenth-century Los Angeles families between 1850 and 1870, one of which relates to the placement of kin and the other to the placement of non-kin. In 1870 compared to 1850, non-kin were less likely to live in households which included conjugal and/or parent-child relationships within them and kin were more likely to live within their own simple families rather than being connected to another, related kin group. Thus the proportion of both simple and non-family households increased and the proportion of "simple plus others" and extended family households declined. One consequence of this change is that as the structure of the family life cycle became simplified, the ability of families to respond to changing life-cycle needs through structural adaptations, such as taking in boarders, was reduced. Wages, therefore, became more important for family subsistence.

Sex Roles and Household Structure

Household units have been, and still are traditionally headed by men. Although this is not a universal phenomenon either in time or across cultures (see Boulding, 1976), women in western societies have tended to head households less by choice than by circumstances, such as widowhood, divorce or separation. In Los Angeles, 18 percent and 14 percent of the

households analyzed in 1850 and 1870, respectively, were headed by women. Although the proportion of female-headed households was not markedly different in the two time periods, the relationships between sex and household structure changed. The following discussion provides an opportunity to see whether or not the macro-economic developments already described can account for these changes as well as providing a basis for exploring sex roles in a developing nineteenth-century community.

The role of women in nineteenth-century California can also be viewed in terms of the actual and/or potential access to the accumulation of wealth which has been applied in earlier sections of this analysis. Both actual and potential wealth historically has been available to California women through their ability to inherit property. Women in non-industrial societies also have an additional potential for the accumulation of wealth through trading or agriculture varying, however, by the type of agricultural technology. (Boserup, 1970).

The earlier analysis of household structure in mid-nineteenth-century Los Angeles (B. Laslett, 1975) shows that women were more likely to head extended families than any other type, while this was not true for men. Since this empirical result controlled for the effects of actual land ownership, it suggests that a generalized authority may have been available to women in a society in which their inheritance rights were legally protected and they had access to economically productive roles.

To have access to both wealth and authority at one point in time, however, does not necessarily ensure its continuation. Recent discussions of women in contemporary third world countries suggest that their position has declined with modernization (see, for instance, Tinker, 1976). Whether or not this was also true in the development of western nations only recently has begun to be explored (see, Shorter, 1975; Scott and Tilly, 1975; Hartmann, 1976). The data available in this analysis provides the opportunity to address this question in one nineteenth-century American city.

²³ Tygiel (forthcoming) reports a similar pattern in late nineteenth-century San Francisco.

Table 4. Relationship of Sex of Household Head to Type of Simple Family: Los Angeles, California, 1870

Household Type	Household Head		N
	Male %	Female %	
Married Couple Alone	100	0	77
Married Couple with Child(ren)	98.2	1.8	275
Single Parent and Child(ren)	31.6	68.4	76

$p < .0001$.

Table 1 shows that, in 1850, women were less likely than average to head non-family households and, as already mentioned, were more likely to head extended family households, independent of the other variables included in the analysis. By 1870 (see Table 2), women were still less likely to head non-family households, but they were also less likely to head extended family households. Table 2 also shows that, in the adjusted form of the relationship, 76 percent of women heads of households were in simple families compared to 41 percent of men.

This result masks another finding, since the simple family category includes households composed of husband-wife alone, nuclear families with children and single parent-child(ren) families. Women were most likely to be designated as the household head when a spouse was not present (see Table 4). They were, then, heading single parent-child families more than any other type. While a similar distribution is found among simple family households in 1850, this was not the modal household category headed by women.

These empirical results refer to the adjusted form of the relationship between sex and household structure and are independent of the effects of land ownership. The changing relationship between sex and household structure was not, therefore, caused by differences in the actual wealth of female heads of households as measured by real estate ownership. For 1870, however, another measure of actual wealth is available—personal estate—and, as suggested earlier, personal estate may have been of greater economic im-

portance in the later period. The inclusion of personal estate in the analysis, however (see Table 3), does not affect the relationships between sex and household structure already discussed. Since differences in women's access to actual wealth does not seem to account for the 1870 relationships, changes in their potential for the accumulation of wealth may offer a tentative explanation.

Wage-paying work for women was severely limited in this period of Los Angeles' history. Factory jobs, a source of female employment in the more industrialized areas of New England, were not available and Indian women who served as domestics in the pre-American period (Fogelson, 1967:9) may have been a cheap source of household service workers in the later period as well. Women's access to the new skills associated with industrializing economies historically has been more limited than men's (Hartmann, 1976) and appears to be a continuing pattern in currently developing societies (Tinker, 1976; Youssef, 1974). It seems reasonable, therefore, to infer that women's potential access to the accumulation of wealth declined between 1850 and 1870, and that this affected the observed relationships between sex and household structure.

The problem of women's restricted access to economic roles is not characteristic of capitalist societies only (see Scott, 1974). Thus, while the changing position of women in nineteenth-century Los Angeles may have been affected by the macro-economic changes which Marx associated with the early stages of capitalist development, they do not appear to be limited to them.

Migration and Ethnicity

High rates of geographic mobility have often been used to explain changes in the distribution of family types over time since kinship ties, it has been suggested, are more attenuated in modern societies than in the past (which is assumed to have been more geographically stable). In this analysis it is, in fact, impossible to disaggregate the net effects of ethnicity and

migration²⁴ from each other, since most Anglos were also migrants (and thus, for this group, there is virtually no variation on migration) while most Spanish-surname persons were born in California or Mexico (and thus there is almost no variation on ethnicity).²⁵ While this statistical problem inhibits interpretation of the net effects of ethnicity and birthplace on household structure independent of each other, it also suggests some possible sources of specification which may contribute to understanding the effects of these variables in nineteenth-century Los Angeles and, perhaps, elsewhere as well.

All types of migration are not equally disruptive of family ties. Overall migration includes at least two potentially different types: (1) first-generation migration into a new area of settlement and (2) later migration from the same place of origin. Later migrants from areas of earlier migration are likely to have an easier time becoming part of the existing social networks in the new place of residence than earlier ones. If this distinction between earlier versus later migration can help to explain household structure, its effect should weaken over time as migrant groups establish more extensive linkages within the community.

Ethnicity often has been used to measure cultural differences between groups; it can, however, also be viewed in other ways. As such, the effects of ethnicity may depend on structural attributes of the ethnic group within a given social context

such as its size, socioeconomic position and degree of segregation.²⁶

The class means in Table 1 show that, in 1850, persons born in Mexico and California were less likely to head non-family households, despite the fact that one was a migrant and the other a non-migrant group, while migrant heads born elsewhere were more likely to do so. Mexican-born (migrant) heads were also more likely to live in simple families, while this is not true for other migrant heads. The migration status of the household head seems to be related only to simpler, not more complex, forms of family organization but in varying ways. The zero-order effects of ethnicity show that Anglos were more likely and Spanish-surname persons less likely to head non-family households, with the reverse set of relationships being true for simple family households.

While the class means report the actual zero-order relationships, only adjusted means can measure the net effects of these independent variables. Yet it is precisely this which cannot be done from these data. In discussing the adjusted means, therefore, the interpretations are suggestive only, since the data cannot address them directly.

In the adjusted form of the relationships between birthplace and household type, Table 1 shows that households headed by persons born in Mexico were more likely than average to head simple family households while other migrant heads were significantly less likely to do so, which repeats the relationships observed in the class means. Migrant heads born elsewhere, on the other hand, were significantly more likely to head simple family households which included non-kin.²⁷

²⁴ Birthplace is being used as a proxy variable for migration, with all California-born household heads considered as non-migrants and all household heads born elsewhere considered as migrants. Using birthplace data for this purpose may underestimate actual migration into Los Angeles by persons born elsewhere in California.

²⁵ The first party of Anglo pioneer settlers arrived in California in 1841 (Caughey, 1970: 136), while migration from Mexico dated back to the eighteenth century. In both 1850 and 1870, a small number of Spanish-surname household heads were, however, born in South America and Spain, and an even smaller number of Anglos were born in California. Thus, no birthplace-ethnicity cells were empty. While the net effects of ethnicity and migration cannot be disaggregated, the adjusted means can be discussed as independent of the effects of the other independent variables included in the computations.

²⁶ Although ethnic and/or migration groups may vary in socioeconomic status, the adjusted means for ethnicity discussed in the text are net of the economic variables included in the analysis. This factor therefore cannot be used to explain the results.

²⁷ The only significant adjusted relationship between ethnicity and household type shows Anglos to have been less likely to head "simple family plus others" households, which contradicts the last result mentioned and is a statistical artifact of the non-independence of the migration and ethnicity variables. These contradictions are also a consequence of the distribution of cases on the independent varia-

These findings suggest that the two different specifications reviewed earlier in this section are relevant to understanding the effects of ethnicity and migration on household structure. Second or subsequent-generation migrants taking historically traditional routes may (1) be more likely to travel within simple family groups and/or (2) may find it easier to establish linkages with the local population which will facilitate their chances within the available marriage market. (Earlier marriage, however, may have a negative impact on the accumulation of wealth.) However, first-generation migrants, especially if they differ culturally from the resident population, may tend to be segregated along ethnic lines.²⁸ Thus the ethnic structure of the receiving area and the migration history of incoming groups may be relevant to understanding the effects of these variables on household organization.

In 1870, Los Angeles had a larger proportion of immigrant household heads than in 1850: 60 percent were born outside California in the earlier period and 83 percent in the later one. As in 1850, both the class and adjusted means show that Mexican-born migrant heads were less likely to head non-family households. No other adjusted relationship between birth-place and household structure reaches a statistically significant level; effects of migration seem to have disappeared. If migration has an impact on household structure, it may operate only in the earliest years of settlement by a new immigrating group.

The proportion of households headed by members of the two major ethnic groups in the area was almost completely reversed in the two decades between 1850 and 1870. More than two-thirds were Spanish-surname in the earlier period, while almost the same proportion were Anglo by the later one. In 1850, the ethnic-

ity of the household head was almost wholly unrelated (via statistically significant adjusted means) to household type. In 1870, however, more significant relationships can be observed. Spanish-surname household heads were less likely than average to head non-family households and more likely to head simple families. The opposite pattern can be seen for Anglo household heads.²⁹

Whether or not change in the proportion of the two major ethnic groups within Los Angeles between 1850 and 1870 and the increasing political and economic dominance of Anglos over the Spanish-surname population during this period (Pitt, 1970) served to heighten cultural differences between them, at this point, can only be a matter for speculation. It should be noted, however, that ethnicity, to the extent that it does affect family structure, continues to be associated only with the simpler, not the more complex types of households.

Summary and Conclusions

The materials presented in this paper are both complex and varied. Among other things, they point to the richness of census data for studying changes in family structure over time. With this one source of easily obtainable documentary data, it has been possible to consider a range of theoretical and empirical questions relevant to family structure and social change. In this respect, scholars owe thanks to the Cambridge Group for the History of Population and Social Structure for their pioneering work in developing quantitative family history.

The overall pattern of findings reported in this paper suggest that changes in the macro-economic structure which Marx associated with the development of capitalism can contribute to understanding family structure in nineteenth-century Los Angeles. The changing relationships of individual attributes to household organization, particularly as they reflect ac-

bles, a factor which affects other results presented in the tables as well.

²⁸ The 1850 Los Angeles data show that boarding was indeed segregated along ethnic lines: 91% of Spanish-surname boarders lived in households headed by Spanish-surname persons and 96% of Anglo boarders lived in Anglo-headed households. The 1870 percentages are similar.

²⁹ The 1870 analysis is affected by the relationship between ethnicity and migration as it was in 1850. The discussion of these results, therefore, only can be suggestive.

tual and/or potential access to the accumulation of wealth, can be interpreted consistently in terms of the transformation of the means of production into capital and the increasing dependence of workers on wage labor for their means of subsistence. These factors are likely to have affected the adaptability of the residential family to the demands which life-cycle variation and economic fluctuations made upon it and have major implications for understanding both the nineteenth- and twentieth-century American family. Further elaboration of the theory developed here must take account of these changes in the macro-economic structure in order to understand both the outcomes of social change in relation to the family and the processes by which these changes came about.

The nature and effects of social class in relation to the family during the development of capitalism has not been specified in this discussion. It seems likely that class relationships would reflect conflict over the efforts of groups which differed on the actual/potential dimensions to maximize or maintain their wealth and/or status positions. Easterlin's (1976) explanation for reductions in nineteenth-century American farm fertility, which emphasizes the decline in opportunities for capital accumulation, is consistent with the positions developed in this paper. Further empirical research is required to more fully understand changes in the family as capitalism developed.

No attempt has been made in this presentation to statistically analyze the temporal sequencing of the relationships discussed. It appears likely, however, that as changes took place in the social organization of economic activities, their impact was felt in the distribution of household types and in changes in the family life cycle and sex roles, particularly in relation to women, as well as other features of family life which this analysis has not been able to address. As the macro-economic institutions were changing from means of subsistence into capital, actual wealth became a relatively more important determinant of household structure and the impact of potential wealth seems to have declined.

An adequate causal model which would attempt to specify these processes, however, must not look at the effects of economic variables alone. As the analysis here suggests, other factors such as sex, life cycle and networks of social integration (which may reflect migration history and ethnicity) may be important intervening variables. The explanation would be tested, however, by asking—at each stage of analyzing the temporal process—whether or not changes in the actual or potential accumulation of wealth affected by these intervening variables can satisfactorily explain the observed results and, furthermore, whether or not changes in wealth accumulation are significantly affected by the economic and political structure of capitalist property and class relationships.

This paper discusses the general theoretical issues about social change and the family which have concerned sociologists and historians for many years. Criticisms of Engels' (1972) analysis of the family over time has led, perhaps, to a premature rejection of a Marxian approach for analyzing this problem. If the interpretations of changes observed in nineteenth-century Los Angeles are correct and can be replicated through studies of other areas and time periods, further attention to a Marxian perspective would be appropriate. The analysis presented here suggests that the variables and dynamics implied by this perspective have considerable explanatory potential for understanding both social change and the family.

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DETERRENCE AS SOCIAL CONTROL: THE LEGAL AND EXTRALEGAL PRODUCTION OF CONFORMITY*

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Previous work dealing with the deterrent effects of legal sanctions has lacked an appropriate sociological context. This paper adopts a theoretical perspective which views legal threats as only one mechanism which may produce conformity. Our framework, which is consistent with both the social control and social influence literature, emphasizes the possible importance of extralegal factors in the production of conformity. We illustrate the methodological implications of our approach empirically with a test of the deterrence doctrine that focuses on the use of marijuana. A multiple regression analysis of data obtained from a sample of both marijuana users and nonusers in a jurisdiction with severe penalties for the use of marijuana indicates that the criminal law receives support from certain extralegal inhibitory influences. Moreover, when the relative efficacy of these extralegal influences are compared to the controlling effects of legal threats, the extralegal influences are found to be the more important.

Studies of the deterrent effects of law have recently enjoyed a renaissance of both scholarly and public interest. Because of its importance for public policy, deterrence research has been prominent in current legislation and litigation, as well as in forums devoted to crime.¹ However, despite the flurry of research in this area and the sanguinity with which many sociologists view the deterrence doctrine, writers agree that we are still some way from being able to specify fully the conditions under which legal sanctions produce a deterrent effect (Tittle and Logan, 1973;

Tullock, 1974). This unsatisfactory state of knowledge has been attributed both to methodological differences in research and measurement techniques and to the ideological predisposition of analysts (Erickson and Gibbs, 1973; Wilson, 1975: ch. 3).

One path toward addressing unresolved issues would be to strive for more adequate research; that is, to conduct studies which are not only methodologically sound, but also operationally comparable. A quite different strategy, however, recently has been proposed by Gibbs (1975) who recommends a moratorium on conventional studies of deterrence. In Gibbs' view, what is needed now is a restatement of the deterrence doctrine as a sophisticated sociological theory. This requires integrating deterrence research with what else is known about "preventive mechanisms"—including a consideration of punishment properties other than, and in addition to, the severity and certainty of the sanction—and especially the extralegal conditions that also may inhibit criminal behavior.

The contention of this paper is that important conceptual and methodological issues surrounding deterrence have yet to be addressed, and that the resolution of these issues requires departures from previous deterrence formulations. This paper proposes an alternative conceptualization

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¹ One study, for instance, of the deterrent effects of capital punishment has figured prominently in arguments before the Supreme Court on the constitutionality of the death penalty (Ehrlich, 1975). The results of this study—that, on the average, every execution in the United States during the period 1933–1967 deterred eight potential murders—has become the topic of discussion not only in professional and academic circles, but also in popular speculations about crime and punishment. A critique of this study can be found in Bowers and Pierce (1975).

of deterrence, one which anchors deterrence in a larger context of social control where legal threats constitute just one mechanism that may produce conformity.² The shift in perspective recommended here is consistent with the emergence of "control" theories of crime and delinquency. Whereas conformity previously was viewed as a given, control theories view it as problematic; the question "Why do they do it?" is replaced by a new question which inquires about the conditions of conformity, i.e., "Why don't they do it?" (Hirschi, 1969: 10-1). One answer, of course, is deterrence. Finally, after introducing this alternative approach, we present a preliminary empirical application to one widely violated criminal prohibition: the possession and use of marijuana.

DETERRENCE IN DOGMA AND THEORY

The deterrence doctrine, as formulated within criminology, is strikingly atheoretical both in its philosophical origins and its historic inattention to developments in the social sciences. As Gibbs (1975: 5) notes, deterrence is more a doctrine than theory, "a vague congerie of ideas with no unifying factor other than their being the legacies of two major figures in moral philosophy, Cesare Beccaria and Jeremy Bentham." In criminology, the philosophical origins of the deterrence doctrine apparently discouraged subsequent sociological scrutiny of the concept's ontological status. The hypothesized deterrent effect was not generated by systematic observations of the relationship between legal sanctions and behavior, but from a body of social thought which invoked the concept of deterrence as a justification for an alternative legal system. Consequently, although classical deterrence doctrine implicitly contains claims about human motivation (Taylor et al., 1973:

2-3), these claims were developed to justify reform, not to explain behavior. Ultimately, criminology granted deterrence the status of a scientific concept or, at least, as a researchable hypothesis, although subsequent conceptual development of this hypothesis again occurred outside the behavioral sciences. Johannes Andenaes, a legal scholar, is usually credited with stimulating modern interest in deterrence. More recent conceptual activity was similarly influenced by an essentially jurisprudential orientation (Hawkins, 1969; Zimring, 1971; Zimring and Hawkins, 1973; Andenaes, 1974; 1975).

In contrast, the role of social scientists generally has been restricted to nonexperimental tests of the formulations developed by students of jurisprudence. Such tests have dealt narrowly with the impact of legal sanctions on criminal behavior, where legal sanctions are the independent variables and crime rates the dependent variable (see Andenaes, 1974: passim). The empirical energy directed toward this relationship shows an uncritical acceptance of the deterrence doctrine without a corresponding theoretical rationale that is based on established principles of social influence and control. This restricted conception of the deterrence doctrine is evident both in the more formal definition of deterrence and the kinds of empirical problems to which this conception traditionally is directed.

Deterrence, Control and Compliance

In criminology, the concept of deterrence is used to describe the prevention of criminal behavior through the use of, or by the threat of, legal sanctions. The criminal law, through its content and application, designates those acts which are proscribed and identifies the legal penalties associated with them. "To the extent that these stimuli restrain citizens from socially undesired actions which they might otherwise have committed, a general preventive effect is secured" (Andenaes, 1974: 35). Deterrence, then, is intended to refer to compliance produced by the existence and administration of the criminal law rather than some other source. So defined, deterrence refers to a

² We use the term "social control" to apply to all social mechanisms invoked to establish, inhibit or change human behavior. To us, social control does not connote any fundamentally different concerns than the term "social influence," except that the former is more often used by sociologists and the latter by social psychologists.

particular *relationship* between the existence, or perception, of legal sanctions and subsequent behavior. Deterrence refers not to sanctions and behavior in isolation but, rather, to one of their possible relationships. The specific relationship of interest is one in which a particular sanction functions to prevent the occurrence of an act; the sanction is thought to decrease the probability of the act's occurrence.

Stated in this way, the deterrence doctrine may be integrated with two related bodies of literature—social control and social influence-compliance. The social control literature describes mechanisms which preserve social order and perpetuate social organization. The concept of social control is often used to account for conduct that is not wholly explained by socialization and situational interaction (e.g., LaPiere, 1954:v). Others use the concept to refer to both internal mechanisms, such as the internalization of norms, and external mechanisms that control behavior, including the manipulation of sanctions—rewards and punishments (Olsen, 1968:124–9). It is clear that the deterrence doctrine refers to this more general sanctioning-behavior relationship, but is more limited in its focus on legal sanctions to the exclusion of others. There is no theoretical reason, however, why the relationship between legal sanctions and behavior should be examined differently than other sanction-behavior relationships.

Most discussions of deterrence blur the potentially important distinction between the intended and actual consequences of control mechanisms. While the former refers to mechanisms which are intended to inhibit, regulate or control behavior, the latter pertains only to those mechanisms which actually do inhibit, regulate or control behavior. The labeling perspective of deviance, for instance, clearly adopts the intended conception of social control; if these mechanisms actually did control behavior, there would be no reason to speak of secondary deviance since, by definition, the deviance would have been controlled in its primary stages.

The actual consequences conception places deterrence in a broader context,

and directs attention to the role of deterrence in the more general context of social control. If deterrence is only a *possible* source of conformity, the question becomes whether or not the fear of legal sanctions is a mechanism of social control. Moreover, the anticipation of legal sanctions, obviously, is not the only possible influence on behavior, nor can it be understood without explicit reference to other social controls because "there is always a complicated interplay between the law and the multitude of other factors which shape our attitudes and behavior" (Andenaes, 1974:1). In short, the task for the student of deterrence is to explain the relationship between behavior, on the one hand, and both legal and extralegal sanctions on the other.

The parallel between deterrence and social influence theory is also manifest. In principle, the deterrence doctrine is basically no different from other sanction-behavior relationships in social influence situations, though an essential difference is that deterrence is concerned with a particular *source* (the legal sanction), a *signal* (a threat) and a *target* (violators or violations of the sanctions). The deterrence doctrine—like social influence theory—anticipates that compliance is determined by certain characteristics of the source such as severity, certainty and celerity, and characteristics of the target such as personality, behavioral commitment, and the instrumental and symbolic dimensions of the act (Chambliss, 1967; Tittle and Logan, 1973; Zimring and Hawkins, 1973).

Perhaps the most useful parallel to be drawn, however, is in the concept of *threat*. The criminological literature implies that the specific mechanism which deters is the threat of detection, apprehension and punishment for non-compliance. Thus, deterrence occurs when "... a threat causes individuals who would have committed the threatened behavior to refrain from doing so" (Zimring and Hawkins, 1973:71). Indeed, the deterrence literature differentiates threats that are communicated by the experience of others who suffer the consequences of violation ("general deterrence") from threats which are com-

municated through personal experience of these consequences at an earlier time ("specific deterrence"). In this sense, criminology's deterrence doctrine pertains to just one potential determinant of compliance: the unilateral, contingent threat.³ The legal threat becomes only one of several kinds of signals or messages that may influence behavior; and, in either area of inquiry, the relevant research question is not whether the signal or sanction actually deters, but under what conditions it does so.

Are Crime Rates Relevant?

These conceptual matters suggest two important empirical considerations for nonexperimental research designs. First, the concept of deterrence must be anchored within a broad context of human motivation. The legal threat may indeed generate conformity, but there is no reason to presume that the law operates in a social influence vacuum. In this regard, the task for deterrence research is somewhat larger and more difficult than anticipated. The deterrence hypothesis *requires* that the multiple sources of conformity be identified; only one of these is legal threat. Furthermore, the deterrence hypothesis requires the estimation of the relative efficacy of these different factors. For these reasons, the conventional bivariate conceptualization of deterrence as the relationship between legal sanctions and crime is insufficient.⁴

The second implication is that the study of deterrence must depart from the traditional unit of observation—the crime rate. Previous research seems not to have recognized that deterrent effects cannot

be indicated by crime rates. Indeed, crime rates may be logically irrelevant to the study of deterrence insofar as they can only indicate who has *not* been deterred. Research on deterrence must utilize observations of both compliance and non-compliance.

It is noteworthy that deterrence research has proceeded rather differently. Typically, deterrence research has examined the relationship between some measure of a sanction characteristic and some measure of criminal behavior, using a bivariate correlational analysis, or a variant of a chi-square test, to assess the strength of the association.⁵ With some variation, this has been the conventional approach to this problem both by sociologists (see, e.g., the summary contained in Gibbs, 1975: ch. 5), and by economists (see, e.g., the papers contained in Becker and Landes, 1974). Once the matter was so defined, the remaining issues have been seen principally as solutions to various methodological problems that relate either to the measurement of the criminal behavior, the inclusion of other indicators of the sanction characteristics, such as perceptual measures, the operationalization of the sanction characteristics, or the appropriate mode of analysis, such as the alternatives offered by the econometricians.

When the relationship between the characteristics of a sanction and the rate of crime is scrutinized for evidence of deterrence, substantial inferences must necessarily be made. It must be presumed, for example, that the reciprocal of the crime rate—the rate of nonviolation—constitutes the "deterrence rate." But, the rate of nonviolation may actually reflect *two* sources of compliance: (1) compliance produced by influences other than a legal threat and (2) compliance produced by legal threats—deterrence. This

³ An interesting analogue to deterrence research is found in the experimental analysis of punitive contingencies explored in studies of interpersonal risk (Marwell and Schmitt, 1975) and threats (Tedeschi, 1970; 1972). This experimental approach appears to be a major alternative methodology to that used in this study.

⁴ There is no basis for presuming that other (extra-legal) influences are somehow "controlled" when the bivariate relationship between legal sanctions and crime is measured. The important question which is not addressed by such studies is: when is compliance the result of legal threats, and when is it the result of other factors?

⁵ If Andenaes set the conceptual precedent, the empirical prototype can be found in the pioneering work of Gibbs (1968) and Tittle (1969). These two studies set the pattern for subsequent research by specifying criminal behavior as the dependent variable. Indeed, the title of Tittle's paper ("Crime Rates and Legal Sanctions") neatly summarizes—albeit in reverse causal order—what is usually taken as the deterrence hypothesis in its null form.

Table 1. Inferring Deterrence from Incidence of Offenses/Offenders: Hypothetical Data *

Category	A	B	C	D
1.	800	600	600	600
2.	100	200	150	250
3.	75	150	175	100
4.	25	50	75	50
Total	1000	1000	1000	1000

* The hypothetical data presented here are meant to depict non-experimental, cross-sectional data.

Legend: Category 1. Number of nonviolations/nonoffenders where extralegal factors produced compliance with law.
 2. Number of nonviolations/nonoffenders where legal threats produced compliance with law.
 3. Number of offenses/offenders which are undetected.
 4. Number of offenses/offenders which are detected.

is why, apparently, deterrence may be regarded as "inherently unobservable and hence immeasurable" (Gibbs, 1975:13).

These considerations are illustrated in Table 1 with hypothetical data. The total audience for the legal threat is divided into groups of nonoffenders or nonviolations (categories 1 and 2) and offenders or offenses (categories 3 and 4). The distinction between categories 3 and 4 represents the difference between "hidden deviance" and "officially known deviance" (the crime rate). Persons in category 1 are inhibited from crime by something other than the threat of legal sanctions.⁶ Category 2 includes those acts or actors that are deterred by threat of legal sanctions; here, extralegal controls have not inhibited criminal behavior, but legal threats have been able to do so.

⁶ Zimring and Hawkins (1973: 120) speak of this group as being characterized by "the strongly socialized individual who will obey commands out of a desire to do right, quite independent of the specific consequences of wrongdoing." The behavior of these persons is uninfluenced by the threat of legal sanctions, except perhaps indirectly, when their values are reinforced by the law. This group, however, is inhibited by factors such as moral sentiments, a simple distaste for the behavior, or the fear of other extralegal consequences from the prohibited behavior.

The hypothetical data have been ordered to extend over four separate time periods, denoted by A, B, C and D. This diachronic example illustrates a situation where the incidence of offenses has increased in each of the three time periods subsequent to time A. An increase of this nature commonly would be attributed to the depreciating deterrent efficacy of the legal sanctions attached to that offense. While this certainly is one interpretation, it is hardly the only one, as the data show. Columns A and B indicate a change in all four categories. When categories 3 and 4 are combined, they illustrate an increase of 100 in the total incidence of crime. However, category 2 also increased by 100, denoting that more cases are being deterred. This particular increase occurs because of a reduction of cases in category 1. Thus, a group whose behavior was previously constrained by extralegal factors is now potentially criminal and, hence, "eligible" for deterrence.⁷ Therefore, even though the incidence of offenses increased, there was a concomitant and equal increase in the *absolute*⁸ deterrent effect, as shown in the increase in category 2. The *net* deterrent effect is calculated by combining categories 3 and 4

⁷ It should be noted that this may be precisely what is happening with respect to marijuana use in the United States. Although the numbers in category 1 for the crime of homicide (a large number, given the strong moral and personal barriers to this offense) and for parking violations (a small number, since there are few moral barriers to the behavior) might be expected to remain relatively constant, there are a number of borderline offenses where moral sentiments can, and do change, independent of changes in legal status. While the numbers in category 1 might be decreasing for marijuana use, other offenses might be showing an increase, such as white-collar and corporate crimes, including environmental and consumer fraud offenses.

⁸ The *net* deterrent effect of a particular sanction can be defined as the total number of threatened behaviors the penalty inhibits less those it fails to inhibit. This definition differs slightly from that of Zimring and Hawkins (1973: 71) who define net deterrence in terms of the numbers of threatened behaviors the penalty prevents less those it creates. While this latter definition may be useful for some purposes, it is felt that the important consideration is the law's ineffectiveness rather than its "labeling" effect. The *absolute* deterrent effect is the number of threatened behaviors the penalty prevents without regard to those it fails to prevent.

and comparing this figure with that in category 2. This net result is 0 (an increase of 100 in incidence, balanced by an equal increase of 100 in absolute deterrence). Consequently, although an increase in incidence is observed, there is also an increase in absolute deterrence. The net result, of course, is *no change* in deterrent efficacy. By performing similar calculations with figures in the remaining columns, parallel conceptual points can be seen. Thus, calculations with columns A and C show a *net loss* of deterrent efficacy, while columns A and D show a *net gain*.

Thus, although each of the three situations shows an increase in the incidence of crime from time A, this information alone does not indicate the actual amount of deterrence because the group where deterrence occurs is not represented in the incidence of crime. Similar conclusions would be drawn if the data were reconstructed to show a decrease in crime. Therefore, knowledge that the incidence of crime has changed does not necessarily lead to correct inferences about the deterrent efficacy of the legal sanctions attached to that behavior; it is inappropriate to infer the behavioral influences of nonoffenders from the actual behavior of offenders.

PERSPECTIVE AND METHOD

These considerations suggest some new directions for nonexperimental research on deterrence. First, such research could benefit from more fully elaborated and explicit theories of legal and extralegal sources of conformity. Since the threat of legal sanctions is only one possible source of compliance, it would be useful if this determinant were differentiated from others, both conceptually and empirically. The relationships between extralegal factors and compliance deserve investigation, not only to permit isolation of the deterrence "effect" but also because they represent basic features of social control. Second, such studies should look for deterrent effects in a more general population of offenders and nonoffenders. That is, deterrence should be investigated by examining a population of persons or

acts that includes both compliance and noncompliance. If evidence of deterrence is to be found, it will be found by examining deterrable acts or actors, not a population of offenders who, by definition, were not deterred.⁹ We have attempted to pursue these implications by formulating a preliminary model of compliance to criminal law that specifies several simple additive causal relationships suggested by prior research. The model postulates that compliance/noncompliance is determined by both legal factors (knowledge of law, legal threat, perceived certainty and severity of punishment) and extralegal factors (social support, social influence, attitudes and several social background characteristics).

Our exploratory test of this model involved a method that differed in some important respects from previous studies. The survey data we utilize contain respondent reports of criminal activity including reported reasons for its *nonoccurrence*. We are sensitive about the obvious problems associated with interpreting self-reported criminal activity, and we recognize the arguable utility of respondent accounts of their own motivational states. For now, however, we are willing to tolerate these potential shortcomings in order to pursue certain conceptual and empirical alternatives to conventional deterrence studies.

The substantive focus of this test is on one criminal offense, a criminal statute for which there is widespread public ambivalence and which is widely violated: possession and use of marijuana. This focus on marijuana crime seems to us an especially appropriate context for examining the relative effects of legal and extralegal controls. Unlike other crimes, the marijuana offense may be expected to involve more variance in terms of its occurrence and in terms of the social distribution of its offenders. This greater variability makes it analytically less difficult to establish relationships in the context of our model. Moreover, the marijuana offense has been

⁹ A similar point has been made by van den Haag (1969) for the crime of homicide and the sanction of capital punishment. See also Zimring (1971: 18-9) for a discussion of this confusion.

said to be one of those criminal offenses where one might expect to find more evidence of deterrence compared to other crimes (Waldo and Chiricos, 1972).

Data utilized in this study were collected by Response Analysis Corporation as part of a national survey conducted for the National Commission on Marijuana and Drug Abuse (1972). The data were gathered in 1971 via personal interviews and self-administered questionnaires within a national probability sample of adults, age 18 and over; these procedures resulted in a weighted *N* of 632. A technical discussion of the sampling field and weighting procedures is provided by Abelson et al. (1972). The data presented here were obtained from respondents in Cook County (Chicago), Illinois, which was oversampled.¹⁰ At the time of the data collection, Cook County was governed by comparatively severe statutory penalties for the use of marijuana (depending on the amount, penalties for possession provided for life imprisonment in the extreme); one might thus expect that legal threats would be a relatively important social control in this area, at this time.

The dependent variable—reported marijuana use/nonuse—was coded into a three-category index based on self-administered questionnaire items: (1) marijuana “never-users”; (2) marijuana “ever-users,” persons who have used marijuana, but not in the past two years; (3) marijuana “current-users,” persons who were using marijuana at the time of the data collection. The measure of use/nonuse is an ordinal level variable which is treated as interval for purposes of this analysis.¹¹

Four classes of independent variables are examined: legal factors, social support

factors, background factors and selected attitudinal factors. Five indicators of legal factors were developed; two of these pertain to *statutory knowledge*. These variables were measured by asking the respondent if he or she knew about federal and state laws prohibiting marijuana use. Responses were dichotomized into yes (or think so) and no. *Perceived certainty* of punishment was measured by asking respondents, “How strictly are marijuana laws enforced in this community?” Responses were coded either as (1) not enforced very strictly or (2) strictly enforced. *Perceived severity* of punishment was measured by asking respondents, “How severe are the courts in this community for this offense?” Responses were coded (1) not strict enough, (2) about right or (3) too strict.

Legal threat was measured by asking the nonusing respondents to indicate the reason(s) for their nonuse. Twelve response alternatives were available, and those respondents who mentioned either “fear of arrest” or “fear of jail” were regarded as having identified a legal threat. All nonusing respondents were given a score of 2 (both items identified), 1 (one of them identified), or 0 (neither item identified). A confirmatory factor analysis, not reported here, justified these procedures.

We developed two measures of social support for marijuana use. One indicator, *number of friends*, was measured by asking respondents how many of their friends use marijuana, at least once in a while. Four categories of response were coded: none, almost none, less than half and more than half. A second social support indicator, *significant other pressure*, was derived from the question about the reason(s) for nonuse discussed earlier. When respondents designated “pressure from family” and “pressure from friends,” they were given a score of 2 (both items identified), 1 (one item identified) or 0 (neither item identified).

Seven background indicators were analyzed: age, gender, race, education, occupation, income and religiosity. *Gender* was dichotomized. *Race* was coded white or nonwhite. *Age* was coded as a six-category ordinal variable: (1) 18 to 25;

¹⁰ We also tested this model on two other jurisdictions, a low penalty area (Omaha, Nebraska) and a penalty area intermediate between Cook County and Omaha (Washington, D.C.). The results of these other applications are not presented here, partly for reasons that will become clear shortly.

¹¹ Labovitz (1967; 1970) has demonstrated that this is not a serious violation of assumptions and, in fact, should be done when one can assume a monotonic relationship between the measurement scale and the underlying scale. Moreover, regression analysis has proven itself a particularly robust technique with these measurement procedures.

(2) 26 to 34; (3) 35 to 39; (4) 40 to 49; (5) 50 to 59; (6) 60 years and older. *Educational level* is a five-category ordinal variable: (1) 8th grade or less; (2) some high school; (3) 12th grade; (4) some college; (5) college graduate or beyond. *Occupation* is a nine-category variable: (1) laborer; (2) farm; (3) service; (4) operative; (5) clerical; (6) craftsman/foreman; (7) sales; (8) managerial, official, proprietor; (9) professional and technical. *Income* was coded into eight categories: (1) under \$2,000 to \$2,999; (3) \$3,000 to \$4,999; (4) \$5,000 to \$6,999; (5) \$7,000 to \$9,999; (6) \$10,000 to \$14,999; (7) \$15,000 to \$24,999; (8) \$25,000 and more. *Religiosity*, measured in terms of frequency of church attendance, was coded: (1) not at all; (2) once in a while; (3) sometimes; (4) regularly.

Two additional variables were developed from responses to the question requesting reasons for nonuse. *Fear of physical consequences* from using marijuana was tabulated in three categories: fear of damage to body, fear of damage to mind and fear of becoming an addict. This variable was coded according to the number of relevant responses indicated. A second variable was based on whether respondents either did or did not cite as a reason for nonuse the *belief that marijuana use was an immoral activity*. The intercorrelations among the measured variables are shown in Table 2.

Our analysis employed multivariate regression and correlation techniques since regression analysis is especially well-suited for multiple factor issues (Tullock, 1974). The model we employ is relatively crude by some standards, but it seems to be a natural starting point for the model-building process. In this respect, the observation of even weak relationships between the legal and extralegal factors and compliance to marijuana laws may stimulate consideration of certain intervening variables and the question of direct and indirect effects (Hauser, 1976). We view marijuana use/nonuse as the dependent variable with four blocks of independent variables. First, we examine the simultaneous effects of all four blocks of independent variables; then the analysis is conducted separately for each block.

FINDINGS

Table 3 shows the standardized, partial regression coefficients for marijuana use/nonuse on all independent variables taken together, as well as by legal and extralegal blocks. The organization of the predictor variables into blocks was not only sensible conceptually, but also was performed to reduce the problem of redundancy (Gordon, 1968) which is associated with analyses that involve a large number of predictor variables. Some coefficients are small because of certain metric decisions whereby interval measured variables are coded as categorical.

The data show, first of all, that despite the measurement problems, the model accounts for a large proportion of the variance in marijuana use/nonuse in this jurisdiction; the total sum of squares (R^2) explained by the model is 72%. Examination of the coefficients, however, indicates that the predictability of marijuana use/nonuse is a function of only a few variables. Thus, the number of friends who use marijuana is the best single predictor ($\beta = .780$). From the sign of the coefficient, the influence of friends' use on respondents' use/nonuse is probably a reinforcing rather than a controlling effect. The central focus of this analysis, however, is not so much the general predictability of marijuana use/nonuse but, rather, the relative contribution of legal and extralegal factors to it. Attention now turns to these effects.

Legal factors. The block of legal variables accounts for 12% of the variance in marijuana use/nonuse, but this effect largely is produced by one variable, perceived severity of marijuana statutes ($\beta = .348$). Although this variable has the strongest effect, the direction of the coefficient is the opposite of what would be expected from the deterrence hypothesis, with the data showing that with increasing perceived severity of these penalties, use of marijuana increases. We have checked and rechecked these data for errors and find none. Our initial interpretation was simply that users were more likely than nonusers to know about the relatively high penalties in Cook County.

Table 2. Correlations between All Variables in Sample

	A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	Q
A																	
B	-30																
C	-26	28															
D	-34	06	15														
E	23	-11	06	18													
F	-15	-06	07	30	-10												
G	03	12	-09	02	14	07											
H	02	16	-13	-10	02	-10	-07										
I	00	25	-02	-11	06	-19	-08	69									
J	-25	-01	06	04	-23	09	-09	-16	-13								
K	-34	00	-01	21	-05	04	04	-03	00	43							
L	-56	05	08	25	-19	07	-06	-01	02	20	54						
M	-06	-09	-01	03	-04	-04	-13	-10	-08	19	06	-08					
N	05	-03	-11	-02	16	-13	06	13	-02	-04	-20	-26	35				
O	-20	03	24	01	-05	18	-02	02	04	-02	02	09	40	-05			
P	-01	14	-19	-04	-11	09	17	14	07	-15	-18	-19	15	24	-03		
Q	-46	06	12	19	-05	07	-07	08	10	17	48	81	-04	-28	01	-21	

Note: Decimal points omitted. Item identifications are: A-Age; B-Sex; C-Race; D-Education; E-Occupation; F-Income; G-Religiosity; H-Knowledge of Federal Laws; I-Knowledge of State Laws; J-Perceived Certainty; K-Perceived Severity; L-Number of Friends Who Use Marijuana; M-Legal Threat; N-Physical Fear of Marijuana; O-Significant Other Pressure Not to Use Marijuana; P-Feeling that Marijuana Use is Immoral; Q-Marijuana Use Behavior Pattern.

Table 3. Partial Regression Coefficients in Standard Form for Marijuana Use/Nonuse Regressed on All Independent Variables Together, and for Each Class

	All Independent Variables	Social Background	Legal Factors	Social Support	Attitudinal
Age	-.007	-.407			
Gender	.005	-.072			
Race	.067	.053			
Education	-.087	.029			
Occupation	.149	.156			
Income	.091	.034			
Religiosity	.007	-.050			
Legal Threat	.180		-.040		
Knowledge of Federal Laws	.116		-.063		
Knowledge of State Laws	.031		.061		
Perceived Certainty	-.023		-.021		
Perceived Severity	.019		.348		
Number of Friends Who Use Marijuana	.780			.812	
Significant Other Pressure	-.191			-.048	
Physical Fears of Marijuana	-.155				-.247
Feeling that Use is Immoral	-.057				-.168
Coefficient of Determination *	.72	.21	.12	.66	.10

* Coefficient of Determination corrected for degrees of freedom.

But, after examining the relationship between these same variables in other jurisdictions with milder penalties, we were persuaded that this particular question probably tapped the sense of moral outrage and injustice that marijuana users attribute to these laws in general. Perceived certainty of punishment, on the other hand, shows essentially no effect on marijuana use/nonuse which, again, is contrary to what we had expected and is inconsistent with previous research on this offense (see Waldo and Chiricos, 1972).

The most noteworthy finding, however, is the noncontributory nature of the indicator of legal threat. This variable's coefficient ($\beta = -.040$) shows a measurable, but essentially trivial influence on marijuana use/nonuse. When legal threat is examined jointly with all other predictor variables, the coefficient is slightly larger ($\beta = .180$), reflecting a relationship (Table 2) with physical fear ($r = .35$) and with significant other pressure not to use marijuana ($r = .35$) and with significant other pressure not to use marijuana ($r =$

.40). Although legal threat exerts no appreciable direct effect, it appears to indirectly affect use/nonuse through other variables.

Extralegal Factors. The social background variables, as a block, account for 21% of the variance in marijuana use/nonuse; the largest effects are associated with age ($\beta = -.407$) and occupation ($\beta = .156$). The strong influence of age is consistent with an interpretation that treats age as a proxy for varying degrees of commitment to the conventional social order (e.g., Hirschi, 1969).

Of all extralegal variables, social support generates the strongest effect on marijuana use/nonuse ($R^2 = .66$). Again, the influence of number of marijuana-using friends is striking ($\beta = .812$).

The belief that marijuana use is immoral ($\beta = -.168$) and the perceived physical consequences of using marijuana ($\beta = -.247$) together account for 10% of the variance in marijuana use/nonuse, and the signs of the coefficients suggest that each factor functions to inhibit marijuana use. These two factors, as indicated earlier,

are positively correlated with legal threat, which further suggests that these three factors jointly may inhibit marijuana use.

In summary, the data pertaining to the relative influence of legal and extralegal factors on marijuana use/nonuse suggest that legal threat is a comparatively important source of compliance. Legal factors generate some explanatory power, but most of this influence appears to be indirect. Compared to other variables, the legal block is relatively unimportant. The factors—legal or extralegal—that appear to functionally inhibit marijuana use are age, fear of physical consequences of such use, and beliefs that marijuana use is immoral.

SUMMARY AND DISCUSSION

In preceding sections, we argued that contemporary deterrence research might profit by pursuing certain new conceptual and empirical directions. We recommended that the concept of deterrence be anchored in, and integrated with, certain principles associated with the social influence-compliance and social control literature and, especially, with prior research pertaining to the mechanisms of threat. We also argued for conceptualizing deterrence more broadly to include a variety of legal and extralegal factors which ultimately may be included in a general model of social control.

In reviewing certain problems associated with prior nonexperimental tests of the deterrence effect, we observed that the traditional use of official crime statistics is not only misleading but probably irrelevant to identifying the conditions under which legal threats actually deter. As an alternative focus, we suggested that deterrence research examine the circumstances and experiences of nonoffenders—the criminally compliant. Finally, after constructing a preliminary model of social control, we presented the results of an exploratory test. We recognize that we have not developed a fully specified theory of social control, particularly one that considers the effect of simultaneous or reciprocal influence among the variables. We also recognize that our preliminary application of the

model, using self-report data pertaining to marijuana use or nonuse as well as respondent reports of reasons for non-use, may be challenged in terms of the well-known measurement effects produced by response error. In our view, these possible shortcomings are offset by the need to explore conceptual and empirical alternatives in understanding the deterrence process.

Our empirical alternative may raise other questions. First, although the data presented provide little support for the deterrence hypothesis as ordinarily formulated, we found that marijuana use or nonuse is a relatively orderly or, at least, predictable phenomenon. That is, we found substantial evidence of social control-compliance produced by ordinary and extralegal processes and influences. The findings suggest the importance of age-graded norms, and this interpretation seems generally consistent with traditional sociological arguments concerning the relative efficacy of informal versus formal social control mechanisms. In this regard, despite contemporary predisposition toward the importance of legal sanctions, our findings are at least consistent with the accumulated literature concerning the primacy of interpersonal influence.

A second question that may be raised pertains to the suitability of marijuana possession or use as an appropriate test of deterrence. We have encountered two general objections to testing a deterrence model with marijuana crime. First, there is currently an inclination to relegate marijuana crime to the gray area of deviance; marijuana use is no longer viewed as “really” criminal and, hence, it is thought to be an inappropriate activity for testing deterrence hypotheses. Although we do not argue with the fact of apparent changing social definitions of marijuana use, we also do not subscribe, on an *a priori* basis, to the viewpoint that it is different in kind from other crimes. At the time the data reported in this study were collected, marijuana offenses were still defined as criminal and a substantial number of marijuana arrests were made in the jurisdiction(s) examined. In this regard, the marijuana possession statute was not a “dead letter” law, although its

enforcement was selective and perhaps uneven. In any case, marijuana crime is behavior that was still vulnerable to the application of criminal sanctions upon detection at the time these data were collected.

Still another matter concerns the apparent inconsistency between the findings produced here and the comparative study of deterrence in marijuana and theft offenses by Waldo and Chiricos (1972). The Waldo and Chiricos study compared the relative deterrent efficacy of the laws governing two crimes, one with substantial social support (theft) and one with considerably less support (marijuana use). This research addressed the contention that for offenses that are widely practiced or condoned, legal threats produce more conformity because the law "stands alone"; it was hypothesized that compliance to marijuana statutes was less likely to be a function of extralegal influences (Andenaes, 1974; Zimring, 1971: 44-5). Waldo and Chiricos found that marijuana use was more likely than theft to be deterred, and they interpret this to indicate that the law is relatively more effective (by default) given the absence of other influences on behavior. The data presented here do *not* support the contention that the law stands alone for marijuana; instead, we found that certain extralegal factors not only influence marijuana use and nonuse, but exercise a stronger effect than do legal factors. We also wish to emphasize that the jurisdiction to which these data pertain had relatively severe penalties at the time of data collection.

The single jurisdictional focus of this analysis may suggest questions concerning the generalizability of these findings. In this regard, we should report that the model presented also has been tested with data from two other jurisdictions with different statutory penalties, Omaha, Nebraska, and Washington, D. C. These analyses showed that the relative impact of the variables examined was generally similar; we also found differences, however, in the coefficients for specific contributions of certain variables, although we presently are unable to account fully for these jurisdictional differences (in-

teractions) short of invoking a "jurisdictional effect," a practice we wish to avoid.

Finally, and in light of the caveats mentioned, we wish to emphasize that the results of this application of deterrence theory would not have been identified nor appropriately interpreted within the more conventional deterrence research paradigm. It is essentially this contrast that persuades us that traditional formulations of the deterrence effect require conceptual scrutiny and the consideration of alternative methods. It is essential that we explore new research directions that conceptually anchor the deterrence effect among other mechanisms of social control and empirically focus on the relevant population parameters of noncompliance and compliance.

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THE DETERRENCE DOCTRINE AND THE PERCEIVED CERTAINTY OF LEGAL PUNISHMENTS*

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This research on the deterrence doctrine differs from previous investigations by focusing on perceived properties of legal punishments rather than objective properties. Data from a survey of 1,700 high school students in Arizona show a close inverse relation among 15 types of crimes or delinquencies between the median or mean perceived certainty of punishment (arrest or reformatory) and rates of self-reported acts. However, the same relation holds between the rates and the perceived seriousness of acts, which is taken as indicative of the social (extra-legal) condemnation of crime. Furthermore, the perceived certainty of punishment and perceived seriousness are so highly collinear that their effects on the rates cannot be differentiated. Although the findings cannot be viewed as conclusive evidence against the deterrence doctrine, they raise doubts about previous interpretations of the inverse relation among states between the objective certainty of imprisonment and crime rates. The relation has no bearing on the perceptual assumptions that enter into the deterrence doctrine, and the present findings indicate that the relation could reflect differential social condemnation of crime.

In reconsidering the deterrence doctrine, investigators have not emphasized that the subject touches on larger issues in sociology. A fair reading of Bentham (1962) and Beccaria (1963) clearly reveals that the deterrence doctrine is a *psychological* theory. To be sure, a penal system is social; but unless deterrence is defined in psychological terms, the definition is alien to Bentham's or Beccaria's conceptualizations and extends the meaning of deterrence to virtually any action by legal officials that prevents crimes by one means or another (e.g., incapacitation). A narrow definition of deterrence (e.g., Bedau, 1970) is more in keeping with Bentham or Beccaria, and the terms (fear, perceived risk, pain) employed in those definitions denote psychological phenomena.

The most obvious implication of such definitions is that perceptual variables must be taken as paramount in testing the deterrence doctrine. Briefly, the doctrine reduces to the assertion that when a crim-

inal act is contemplated the *perception* of a high risk of a swift and severe reaction by legal officials is a *sufficient* condition for omitting that act. If that brief version of the doctrine is accepted, then an empirical relation between objective properties of punishment (e.g., the proportion of alleged crimes that actually result in the arrest and/or imprisonment of a suspect) and the crime rate is *at most* only a necessary condition for corroboration of the deterrence doctrine.¹ So commentators have rightly criticized deterrence investigations for their exclusion of perceptual variables (see especially Geerken and Gove, 1975; Gibbs, 1975; Henshel and Silverman, 1975).

Although perceptual considerations are truly central in the deterrence doctrine, sociologists who work in the Durkheimian tradition have no enthusiasm for theories that deal with psychological phenomena. Yet some sociologists (e.g., Homans,

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¹ A truly narrow interpretation of the deterrence doctrine would assert that the objective properties of legal punishment are irrelevant, meaning that the doctrine is a purely *perceptual* theory. Yet both Bentham and Beccaria evidently believed that objective properties are relevant (though their statements along that line are obscure); moreover, without that supposition the deterrence doctrine has no policy implications.

1974) are predisposed to such theories, and the contrast in preferences appears irresolvable. In any case, a preference for some kind or level of theory is one issue, while the assessment of the empirical validity of a particular theory (whatever the kind or level) should be recognized as a separate issue. The point is that sociologists who prefer macroscopic or structural-functional theories are correct if they identify the deterrence doctrine as alien to their interest, but that identification has no bearing whatever on the empirical validity of the deterrence doctrine. Yet it does not follow that psychologically-oriented sociologists necessarily accept the deterrence doctrine. They may object to the doctrine by alleging that it assumes rationality or free will (e.g., Schuessler, 1952); but defenders of the doctrine (see especially van den Haag, 1969; Jeffrey, 1965) question those allegations and suggest a completely opposite interpretation—that the doctrine is thoroughly deterministic.

Basic Alternative Strategies for Deterrence Research

There are at least three basic strategies for nonexperimental research on general deterrence: (1) an examination of the relation between properties of punishment and crime rates *among jurisdictions*, (2) an examination of the relation *among types of crimes* in the same jurisdiction, and (3) an examination of the relation between perceived properties of punishment and the frequency of criminal acts *among individuals*.² Even if one should argue that only the third strategy (comparisons of individuals) can provide definitive evidence, the other two are surely relevant. However, most purported tests of the deterrence doctrine at the aggregate level have been restricted to comparisons of jurisdictions (e.g., states of the U. S.). While that limitation may not be as serious as the exclusion of perceptual variables, evidence concerning deterrence is incomplete without the second research

strategy—that is, comparisons of types of crimes.

With a view to expanding the range of evidence on the deterrence doctrine, the research reported here focused on a comparison of types of criminal or delinquent acts as regards (1) the perceived certainty of punishment and (2) the estimated incidence of the act. As such, the research differs from previous investigations in that types of acts rather than jurisdictions are units of analysis and the perceived certainty of punishment rather than the objective certainty is the principal independent variable.

The Perceived Certainty of Punishment

Although various perceptual properties of punishment are seemingly relevant in testing the deterrence doctrine (Gibbs, 1975), the present focus on *perceived certainty* is justified by two considerations: first, numerous statements by Beccaria and Bentham suggest that the certainty of punishment is more efficacious (as regards deterrence) than is the severity of punishment; and, second, the emphasis on certainty is entirely consistent with the findings of recent investigators, most of whom report that *among states* the crime rate is much more inversely related to the objective certainty of imprisonment than to the presumptive severity (length) of prison sentences (see surveys by Tittle and Logan, 1973; Zimring and Hawkins, 1973; Silver, 1974; Gibbs, 1975). Those findings may be pertinent in assessing the relative deterrent efficacy of the certainty and severity of *actual* punishments, but it is dubious to construe the inverse relation between the certainty of imprisonment (or any kind of punishment, for that matter) and the crime rate as corroborating the deterrence doctrine. First and foremost, it is dubious not because of arguments about purely statistical artifacts (see Chiricos and Waldo, 1970; Logan, 1971; 1972) or even questions about the reliability of official crime rates. Rather, the evidence is disputable because of the very nature of the deterrence doctrine.

Granted that the deterrence doctrine is not a systematic theory (Gibbs, 1975), any reasonable interpretation would recognize

² Logan's research (1972) is an instance of the first kind of investigation, and Waldo and Chiricos' research (1972) is an instance of the third type.

that the doctrine encompasses at least two interrelated premises pertaining to differences (1) among jurisdictional populations as regards some type of crime or (2) among types of crime in the same jurisdiction.

Premise I: The greater the objective certainty of punishment, the greater the perceived certainty of punishment.

Premise II: The greater the perceived certainty of punishment, the less the crime rate.

The issue at hand is not the choice of a covariational rather than a causal language in stating the premises. Whatever the language, the objective certainty of punishment is asserted to be associated with the crime rate only through the perceived certainty of punishment. By the use of the sign rule and assuming that the relations asserted in the premises (I and II) are close, it is possible to deduce an assertion of an inverse relation between the objective certainty of punishment and the crime rate from the two premises. However, if the punishment is imprisonment, the same relation can be deduced from alternative premises.

1. The greater the objective certainty of punishment, the greater the incapacitation of potential repeaters.
2. The greater the incapacitation of potential repeaters, the less the crime rate.

The rationale for the alternative premises is scarcely less plausible than the deterrence premises, as no one is likely to deny that some crimes (e.g., auto theft) are a difficult achievement in prison. Accordingly, recent findings of an inverse relation between the objective certainty of punishment and the crime rate are, at most, only a *necessary condition* for corroboration of the deterrence doctrine, as the relation could obtain through incapacitation even if *both* deterrence premises (I and II) were patently false. The only way to bring less conjectural evidence to bear on the deterrence doctrine is by a direct

test of the premises, and the present paper reports such a test of Premise II: The greater the perceived certainty of punishment, the less the crime rate.

Data Employed in the Present Research

Previous deterrence investigations focused on the *objective* certainty of punishment because enormous resources would be required to gather data on perceptions of certainty in each of several jurisdictions. However, if the deterrence doctrine is tested by comparing types of crimes or delinquencies, data on the perceived certainty of punishment can be gathered by a survey in one jurisdiction.

Although comparisons of types of acts (crimes or delinquencies) are more feasible than jurisdictional comparisons, both strategies are questionable if the tests of the deterrence doctrine are based on official crime rates. Doubts about the reliability of those rates are legion. Accordingly, with a view to expanding the range of evidence on the deterrence doctrine, the present research compared types of acts in the same jurisdiction as to (1) the perceived certainty of punishment and (2) the number of acts reported by a sample of residents of the jurisdiction.

Data for juveniles. To obtain data for tests of the second deterrence premise, questionnaires were administered to 1,700 juveniles in six Arizona high schools, three in a metropolitan area and one each in three small Arizona towns. The students were asked several questions about legal punishment and their commission of criminal or delinquent acts.³ However, before considering the questions that are relevant for present purposes, three issues are considered.

In a literal sense, the deterrence doctrine makes assertions about crimes rather than delinquent acts by juveniles. How-

³ No sampling of students was attempted, which is to say that coverage was complete except for (1) some 10% of the students who were absent from school the day data were collected or (2) those who were not given parental permission to participate in the survey. The questionnaires were administered in group sessions, and each student made entries only after extensive instructions. The "codeable response rate" is no less than 90 percent for each item in the questionnaire.

ever, the distinction is dubious, especially if a strictly legal conception of delinquent acts is accepted. Delinquent acts are subject to punishment in much the same sense as are crimes; and the fact that individuals above some minimum age can commit criminal but not delinquent acts is not crucial, for all manner of "crimes" are peculiar to or contingent upon particular statuses. No less important, the laws of a jurisdiction may be such (as in Arizona) that juveniles who commit crimes are liable in *principle* to the same punishments as adults. So the argument is that tests of the deterrence doctrine can justifiably consider acts by juveniles, whether those acts are crimes or delinquencies.

The use of data pertaining to "self-reported crimes and delinquencies" raises an even more critical issue. Even though use of such data is now virtually a convention, the reliability and validity of the data are nonetheless subject to question (see Reiss, 1973). For any given body of self-reported data (crimes or delinquencies),

there is no truly defensible and feasible basis for identifying or estimating the proportion of respondents who (1) misunderstood the description or denotation of acts in the self-report questions, (2) did not remember having committed the act, or (3) deliberately gave an incorrect answer. Nonetheless, granted that there is no evidence that would justify any definite conclusions about the reliability of self-reported data on crimes or delinquency, it does not tax credulity to assume that self-reports are at least as reliable as estimates of the frequency of criminal or delinquent acts based on apprehension records or court records.⁴ The more general point is

⁴ The same arguments could be made for the use of "victimization" data. However, unlike self-reported data, victimization data cannot be used to test the deterrence doctrine by examining the relation *among individuals* between perceived properties of punishment and frequency of criminal or delinquent acts. The present data can be used to conduct such a test; but work along that line has not been completed, and space limitations preclude reporting even preliminary results.

Table 1. Questions Answered by Students in Six Arizona High Schools, 1974^a

Question ^b	Corresponding Legal Label
1. Take money or something by threatening someone with a weapon (gun, knife, etc.)?	1. Armed robbery
2. Steal something worth more than \$100 (<i>not counting shoplifting</i>)?	2. Grand theft
3. Take someone's car without their permission?	3. Auto theft
4. Take money or something from someone by just threatening them (without a weapon)?	4. Unarmed robbery
5. Beat up or hurt someone on purpose?	5. Assault-battery
6. Break into a place to do something illegal?	6. Burglary
7. Steal something worth less than \$100 (<i>not counting shoplifting</i>)?	7. Petty theft
8. Take something from a store on purpose without paying for it ("shoplifting")?	8. Shoplifting
9. Use any marijuana?	9. Marijuana use
10. Run away from home?	10. Runaway
11. Get into any fist fights or brawls (<i>not counting the times you beat up or hurt someone on purpose</i>)?	11. Fighting
12. Drink any beer, wine or liquor (<i>not counting sips your parents let you have</i>)?	12. Drinking
13. Disobey or defy your parents?	13. Defiance
14. Skip school without an excuse?	14. Truancy
15. Smoke or chew tobacco?	15. Smoking

^a Three schools in an SMSA and one each in three small-town high schools.

^b Each question was preceded by the phrase: "During the last 12 months, how many times did you..."

that tests of the deterrence doctrine necessarily employ data that are inherently questionable. So, at present, there can be no definitive tests of the deterrence doctrine, and the only hope is that various kinds of data on the incidence of crimes or delinquencies yield more or less the same results.

Questions posed. Table 1 shows each of the questions pertaining to self-reported acts and corresponding "legal labels" that are used in subsequent tables. Two measures of central tendency, the mean and the median, were used to describe the responses of students to each question. Those values are shown in columns 3 and 4 of Table 2 and, henceforth, they are referred to as "mean" or "median" acts. In recognition that for some types of crimes only a few students reported any commissions, still another "rate" was computed. It is simply the proportion of students who reported *at least one* commission of the offense and, hence, it corresponds to what Gibbs (1975) has designated as the "categorical rate." Those rates are shown in column 5 of Table 2.

In a subsequent section of the questionnaire, two additional questions were asked about each of the same 15 acts, with the acts described as in the first column of Table 1. The additional questions were worded as follows: "Out of the last 100 times a juvenile (*description of the act*): (1) How many would you guess resulted in an arrest? (2) How many would you guess resulted in a juvenile being sent to a state reformatory?" Answers to each question were analyzed as percentages (i.e., the base is 100) and they are taken as the perceived certainty of the designated type of legal reaction, either arrest or incarceration.⁵ Two "certainty values," the mean

and the median, are shown in Table 2 for each type of legal reaction (see columns 1-2 and 6-7). Henceforth, those values are referred to as "mean certainty" or "median certainty."

The frequency of crimes or delinquencies may reflect two conditions independent of the character of legal punishments, those two being the "extralegal inhibitory" and the "extralegal generative" (Gibbs, 1975). Those terms denote strictly logical possibilities; that is, they do not give rise to falsifiable generalizations about particular empirical correlates of crime or delinquency. Moreover, deviance theories are not such as to justify the identification of specific extralegal generative variables; but in the case of the extralegal inhibitory there is a longstanding, purely normative argument—that the frequency of a particular type of act in a population varies inversely with the social (extralegal) condemnation of that act. With that argument in mind, the students were asked a question about each of the 15 types of acts in the following form: "If the number 100 was used to indicate how serious stealing something worth less than \$100 is, what number would you give to (*description of the act*)?" The median value of responses (magnitude ratios) to the question for each type of act is shown in column 8 of Table 2. The use of medians for magnitude ratio data are now conventional (see Hamblin, 1974).

Needless to say, there are various ways that such a "seriousness" question could be worded (see, e.g., Sellin and Wolfgang, 1964; Rossi et al., 1974), with some of the alternative key terms being: disapprove, personally disapproves, bad, and disapproved by others. Only experimentation can reveal whether the wording of an evaluative question makes any substantial difference, but there is a rationale for the use of the key term "serious." It is the most general term possible, since it

the frequency of crime but also with the relation between objective and perceptual properties of legal punishment. In any case, the kind of perceptual variable (if any) that is related to the crime rate remains to be determined, and the deterrence doctrine cannot be construed as a testable theory until (*inter alia*) it is extended to a specification of the kind of relevant perceptions of the certainty of punishment and procedures for gathering related data.

⁵ Needless to say, the exact wording of such a perceptual question could make an appreciable difference in the outcome of tests, and some of the alternatives can be conceptualized. Rather than ask questions that pertain to perceived *aggregate* certainty, the questions could be worded so that they pertain to perceived *personal* risk of punishment. Although some commentators on deterrence research (e.g., Teevan, 1975) evidently presume that only perceptions of personal risk could be relevant, it is difficult to see how that can be known *a priori*, especially if investigators are concerned not only with the relation between perceived certainty and

Table 2. Responses by Arizona High School Students to Questions about 15 Types of Acts That Are Crimes or Delinquencies, 1974^a

Types of Acts	Perceived Certainty of Being Arrested		Self-Reported Rates of Criminal or Delinquent Acts			Perceived Certainty of Being Sent to a Reformatory		Median Perceived Seriousness Values
	Mean	Median	Mean Number	Median Number	Categorical Rate ^b	Mean	Median	
	Col. 1	Col. 2	Col. 3	Col. 4	Col. 5	Col. 6	Col. 7	Col. 8
1. Armed robbery	51.28	50.18	.12	.01	.020	29.76	20.50	159.70
2. Grand theft	48.17	50.35	.24	.03	.058	26.93	19.85	146.21
3. Auto theft	45.31	45.65	.39	.06	.114	22.35	10.32	109.53
4. Robbery	34.52	30.08	.23	.02	.047	14.16	5.47	100.42
5. Assault	33.34	25.09	1.09	.08	.131	15.47	5.42	100.31
6. Burglary	44.73	45.88	.84	.08	.137	18.97	10.45	100.26
7. Petty theft	37.29	33.90	1.48	.12	.189	14.47	8.21	100.00
8. Shoplifting	30.52	24.03	4.34	.29	.364	11.20	4.84	99.50
9. Marijuana use	34.77	25.12	32.82	.33	.395	15.59	4.99	96.67
10. Runaway	32.91	24.33	.58	.05	.085	12.13	4.63	76.41
11. Fighting	21.36	10.41	1.42	.17	.255	7.56	1.46	75.95
12. Drinking	21.03	10.26	39.91	5.21	.731	5.52	.41	50.27
13. Defiance	12.97	3.33	28.77	2.04	.571	5.25	.23	50.15
14. Truancy	11.77	1.59	13.58	1.43	.577	3.47	.18	49.71
15. Smoking tobacco	9.82	.40	53.46	.37	.422	2.23	.10	24.84

^a See Table 1 for a more detailed description of the types of acts and see text for a description of the procedures employed in gathering the data.^b Proportion of respondents reporting one or more instances of the act.

suggests not only a personal evaluation of the act but also awareness of a collective evaluation (i.e., how others evaluate it).

Comparisons of Types of Acts

Table 2 reports all of the data needed for tests of the second premise of the deterrence doctrine, which asserts an inverse relation between the perceived certainty of punishment and crime rates. Specifically, the prediction is an inverse relation between each of the alternative measures of the perceived certainty of punishment (columns 1, 2, 6, and 7 of Table 2) and each of the alternative self-reported rates (columns 3–5 of Table 2).

Findings on the perceived certainty of arrest. Regardless of the alternative measures of the two variables or the division of the student population considered (metropolitan versus small town), all correlation coefficients in column 1 of Table 3 are consistent with the second deterrence premise. However, the degree of association is clearly relative to the type of self-reported rate, with the correlation consistently greater for the categorical rate.

The differences among the correlation coefficients within each panel of column 1 (Table 3) reflect nonlinear relations between the perceived certainty of arrest and the mean or median frequency of self-reported acts. Specifically, when the data are plotted in a scatter diagram with certainty values on the horizontal axis and the mean or median acts on the vertical axis, the form of the relation is negative logarithmic;⁶ hence, the log correlations (column 6, Table 3) tend to be greater than the linear correlation (column 1, Table 3). The log correlations also vary much less than the linear correlations (compare columns 1 and 6 in *each panel* of Table 3), which indicates that the substantial variation in the linear correlations (column 1 within each panel) is due to extremely

high self-reported rates for particular types of crimes or delinquencies. However, relatively extreme values are more conspicuous for the median or mean frequency of acts (especially the mean) than for the categorical rate; therefore, only in the case of the categorical rate is the linear correlation greater than the log correlation.

The difference between the categorical rate and the other measures of the frequency of self-reported acts takes on theoretical significance in connection with types of deterrence. The findings suggest that if the correlations are manifestations of deterrence, the type is "absolute deterrence" (see Gibbs, 1975), which has to do with the distinction between individuals who have never committed the act and those who have committed it at least once.⁷ The categorical rate corresponds to that distinction, while the mean number of acts is a version of the conventional crime rate, that is, the number of offenses as a ratio to the *total* population (those individuals who have never committed the acts *and* those who have). However, the argument is not that the findings somehow demonstrate absolute deterrence. A large proportion of individuals who have never committed a particular type of criminal or delinquent act is only a *necessary* condition for *inferring* substantial absolute deterrence. Some individuals may never so much as contemplate the type of act in question; and of those who do contemplate it, some may refrain in recognition of the social condemnation of the act and/or the possibility of extralegal sanctions. That consideration makes the perceived seriousness values in Table 2 relevant in the present analysis.

As shown in column 4 of Table 3, the relation between the perceived certainty of arrest (mean or median values) and the frequency of self-reported acts (mean, median, or categorical rate) is quite different when the seriousness values of acts are partialled. All of the partial correla-

⁶ That is, beyond some point any further increase in the perceived certainty of punishment is associated with only very small decreases in the mean number of self-reported acts. The form of the relation may largely reflect a "floor effect" (i.e., the number of self-reported acts cannot be negative) and the fact that for several types of acts the modal self-reported number is zero.

⁷ Given an individual who has never committed a particular type of crime but one who has contemplated it and refrained out of fear of punishment at least once, that individual is an instance of absolute deterrence (relative to the type of crime and some point in time).

Table 3. Correlations among 15 Types of Acts (Crimes or Delinquencies) between Values Pertaining to the Perceived Certainty of Arrest, Frequency of Self-Reported Acts, and Perceived Seriousness of Acts: Data from Responses of Arizona High School Students, 1974^a.

Location of High Schools	Measure of Perceived Certainty of Arrest (p)	Measure of Frequency of Self-Reported Acts (f)	Linear Zero-Order or First-Order Partial Correlation Coefficients ^b					Zero-Order or First-Order Partial Correlation Coefficients, with Logarithmic Transformation of Values ^b				
			ipf	tsf	ips	ipf.s	tsf.p	ipf	tsf	ips	ipf.s	tsf.p
			Col. 1	Col. 2	Col. 3	Col. 4	Col. 5	Col. 6	Col. 7	Col. 8	Col. 9	Col. 10
Total	Mean	Mean	-.67	-.70	.93	-.04	-.31	-.78	-.80	.94	-.16	-.31
	Mean	Median	-.48	-.51	.93	-.02	-.19	-.75	-.73	.94	-.28	-.11
	Mean	Categorical ^c	-.79	-.76	.93	-.36	-.10	-.74	-.73	.94	-.24	-.16
	Median	Mean	-.66	-.70	.92	-.06	-.33	-.74	-.80	.94	.04	.45
	Median	Median	-.49	-.51	.92	-.07	-.17	-.64	-.73	.94	.18	-.47
Metropolitan (SMSA)	Mean	Categorical ^c	-.79	-.76	.92	-.36	-.14	-.65	-.73	.94	.15	-.46
	Mean	Mean	-.70	-.69	.92	-.23	-.19	-.79	-.83	.94	-.03	-.42
	Mean	Median	-.47	-.48	.92	-.09	-.14	-.72	-.75	.94	-.08	-.29
	Mean	Categorical ^c	-.78	-.75	.92	-.37	-.12	-.72	-.76	.94	.01	-.37
	Median	Mean	-.68	-.69	.88	-.21	-.27	-.75	-.83	.92	.06	-.54
Small Towns (Three small-town high schools combined)	Median	Median	-.49	-.48	.88	-.15	-.13	-.66	-.75	.92	.13	-.49
	Median	Categorical ^c	-.79	-.75	.88	-.41	-.18	-.65	-.76	.92	.20	-.55
	Mean	Mean	-.59	-.69	.90	.11	-.46	-.72	-.70	.92	.27	-.13
	Mean	Median	-.45	-.52	.90	.05	-.30	-.71	-.65	.92	-.38	.02
	Mean	Categorical ^c	-.79	-.75	.90	-.38	-.16	-.73	-.65	.92	-.44	.08
	Median	Mean	-.56	-.69	.89	.17	-.51	-.67	-.70	.97	.01	-.25
	Median	Median	-.47	-.52	.89	-.01	-.26	-.58	-.65	.97	.27	-.45
	Median	Categorical ^c	-.76	-.75	.89	-.29	-.26	-.61	-.65	.97	.08	-.28

^a See Table 2 for the values that entered into the computation of the correlation coefficients ($N=15$ for each coefficient).^b The letter "s" denotes the median perceived seriousness value for a type of act.^c Proportion of students who reported committing the act one or more times.

tions are negligible, with no more than 17 percent of the variance in the frequency of self-reported acts explained by variance in the perceived certainty of arrest. Those partial correlations reflect two relations: (1) a very close direct relation between the seriousness of acts and the perceived certainty of arrest, as shown in column 3 of Table 3; and (2) a substantial inverse relation between the seriousness of acts and frequency of acts, as shown in column 2 of Table 3. The effect of partialling seriousness cannot be attributed to the previously noted nonlinear relations, since the same effect obtains when the log correlations are considered (see column 9 of Table 3). Indeed, in that case, partialling even more dramatically diminishes the relation between the perceived certainty of arrest and the frequency of self-reported acts.

On the whole, the foregoing summary observations apply in much the same way to both major divisions of the student population, that is, metropolitan and small-town high schools. The similarities are noteworthy since it is virtually an article of faith among commentators on the deterrence doctrine (e.g., Zimring and Hawkins, 1973) that some populations are more deterrable than others. That may be so, but the present findings suggest that deterrability is not invariantly associated with the rural-urban distinction.

Perceived certainty of incarceration. Table 4 is exactly like Table 3 except the perceived certainty values that enter into the correlations pertain to "being sent to a state reformatory" rather than to arrest. Although the outcome of tests of deterrence propositions could be contingent on the type of legal punishment, that is clearly not the case for the present findings. With two exceptions, *all* of the observations and interpretations of the coefficients of correlation in Table 3 apply to those in Table 4.

The most obvious exception has to do with the effect of partialling the seriousness of acts when examining the relation between the perceived certainty of punishment and the frequency of self-reported acts (crimes and delinquencies). There is one set of partial correlation coefficients in which most of the coefficients

are negative and substantial. That one set comprises the partial log correlations for small-town high schools (see column 9 of Table 4, bottom panel). At least four of the six correlation coefficients *appear* to represent support for the second premise of the deterrence doctrine even though the seriousness of acts is partialled.

The second exception has to do with the difference between measures. One of the log partial correlation coefficients in *each* panel of column 9 (Table 4) is negative and substantial by any reasonable standard, and those coefficients pertain to the relation between the *median* perceived certainty of incarceration in a reformatory and the *median* self-reported rate.

A declaration that either of the two exceptions is convincing evidence in support of doctrine would be most disputable. There is nothing in any version of the deterrence doctrine that would anticipate positive test results only for the small-town population, and that is all the more the case since even for that population positive test results obtain only for log correlations. Positive test results are not peculiar to one population if only the median values of the two variables are considered; but there is no theoretical rationale for identifying those particular measures as crucial, let alone why they provide support for the deterrence doctrine only in the case of log correlations.

Even if there were a theoretical rationale for anticipating positive tests of the second deterrence premise only for particular kinds of measures of crime and/or particular kinds of populations, there would still be the problem of accounting for the differences between Tables 3 and 4. Whereas the partialling of seriousness of acts virtually eliminates the inverse relation between the perceived certainty of *arrest* and self-reported rates regardless of the kind of measures or population (see Table 3), that is not the case for the inverse relation between the perceived certainty of *incarceration* and self-reported rates (see Table 4). Defenders of the deterrence doctrine may reason that the contrast is consistent with a conventional belief about deterrence—that the certainty of punishment is important only to the extent that the punishment

Table 4. Correlations among 15 Types of Acts (Crimes or Delinquencies) between Values Pertaining to the Perceived Certainty of Incarceration in a Reformatory, Frequency of Self-Reported Acts, and Perceived Seriousness of Acts: Data from Responses of Arizona High School Students, 1974^a

Location of High Schools	Measure of Perceived Reformatory (p)	Measure of Frequency of Self- Reported Acts (f)	Linear Zero-Order or First-Order Partial Correlation Coefficients ^b					Zero-Order or First-Order Partial Correlation Coefficients, with Logarithmic Transformation of Values ^b				
			r _{pf}		r _{sf}		r _{ps}	r _{pf}		r _{sf}		r _{ps}
			Col. 1	Col. 2	Col. 3	Col. 4	Col. 5	Col. 6	Col. 7	Col. 8	Col. 9	Col. 10
<i>Total</i>	Mean	Mean	-.61	-.70	.96	.32	-.53	-.80	-.80	.97	-.16	-.19
	Mean	Median	-.49	-.51	.96	-.00	-.16	-.76	-.73	.97	-.35	.07
	Mean	Categorical ^c	-.77	-.76	.96	-.23	-.11	-.77	-.73	.97	-.34	.06
	Median	Mean	-.55	-.70	.92	.36	-.61	-.82	-.80	.96	-.31	-.10
	Median	Median	-.45	-.51	.92	.07	-.28	-.81	-.73	.96	-.57	.27
<i>Metropolitan (SMSA)</i>	Median	Categorical ^c	-.72	-.76	.92	-.06	-.37	-.77	-.73	.96	-.37	.04
	Mean	Mean	-.64	-.69	.95	.12	-.38	-.81	-.83	.96	-.04	-.34
	Mean	Median	-.49	-.48	.95	-.12	-.06	-.76	-.75	.96	-.23	-.08
	Mean	Categorical ^c	-.77	-.75	.95	-.28	-.08	-.75	-.76	.96	-.10	-.22
	Median	Mean	-.59	-.69	.91	.14	-.47	-.83	-.83	.94	-.24	-.28
<i>Small Towns (Three small- town high schools combined)</i>	Median	Median	-.46	-.48	.91	-.05	-.18	-.82	-.75	.94	-.50	.10
	Median	Categorical ^c	-.73	-.75	.91	-.17	-.30	-.77	-.76	.94	-.22	-.20
	Mean	Mean	-.52	-.69	.86	.20	-.56	-.73	-.70	.92	-.33	-.08
	Mean	Median	-.45	-.52	.86	-.02	-.29	-.76	-.65	.92	-.55	.19
	Mean	Categorical ^c	-.77	-.75	.86	-.36	-.28	-.80	-.65	.92	-.67	.35
	Median	Mean	-.48	-.69	.79	.15	-.58	-.76	-.70	.95	-.45	.12
	Median	Median	-.43	-.52	.79	-.03	-.33	-.78	-.65	.95	-.66	.42
	Median	Categorical ^c	-.74	-.75	.79	-.36	-.41	-.79	-.65	.95	-.71	.48

^a See Table 2 for the values that entered into the computation of the correlation coefficients ($N=15$ for each coefficient).

^b The letter "s" denotes the median perceived seriousness value for a type of act.

^c Proportion of students who reported committing the act one or more times.

is severe. While incarceration in a reformatory is undoubtedly perceived as more severe than arrest, the argument in question is not consistent with the zero-order correlations (linear or log) in Tables 3 and 4. Specifically, it simply is not the case that the inverse relation between the self-reported rate and the perceived certainty of punishment is closer when the punishment is incarceration rather than arrest. True, it could be that the relation is contingent on the severity of the punishment when the social condemnation of crime is constant (i.e., seriousness of acts is partialled), but that possibility is not even suggested by any version of the deterrence doctrine. The more general consideration is that the statement of the deterrence doctrine as a sophisticated theory may be far more difficult than any version of the doctrine suggests.

Contrary interpretations of the test findings. Since the zero-order correlation coefficients in columns 1 and 6 (linear and log) of both Tables 3 and 4 are consistent with the second deterrence premise, the issue hinges largely on the results obtained when the seriousness of acts is partialled. Defenders of the deterrence doctrine cannot justifiably dismiss the partials on the grounds that the values representing the perceived certainty of punishments and the frequency of self-reported acts are unreliable, for the same data enter into the zero-order correlations that *are consistent* with the second deterrence premise. Alternatively, defenders of the doctrine can focus on the close direct relation between the seriousness of acts and the perceived certainty of punishment (see columns 3 and 8 in Tables 3 and 4), for when that relation is controlled the correlation between the perceived certainty of punishment and the frequency of self-reported crimes or delinquencies is not consistently substantial.

One argument is that individuals tend to view a type of crime or delinquency as "serious" only to the extent that they perceive the legal punishment of it as certain, and the argument extends to the claim that perceptions of legal punishment generate or maintain the social condemnation of crime. Yet there is no systematic evidence on the nature of the relation be-

tween the perception of punishment and variables pertaining to the extralegal evaluations of types of crimes, such as the present "seriousness" values. Although defenders of the deterrence doctrine may argue that perceptions of legal punishment for a type of crime as "certain" lead individuals to view that crime as serious, the opposite argument is scarcely less plausible. Specifically, it may be that individuals tend to perceive the certainty of punishment in terms of "what ought to be"; that is, legal punishment of serious crimes *should be certain*. The argument is all the more plausible since it is difficult to see how the typical individual has a sufficient experiential basis for even relatively accurate estimates of the certainty of punishment. Nonetheless, conventional versions of the deterrence doctrine clearly assume a relation between perceptual properties and objective properties of punishments, perhaps because without such a relation the doctrine would have no implications for penal policy.

Even if it is granted that the perceived seriousness of crimes or delinquencies is a function of the perceived certainty of legal punishment, there is still the question of the inverse relation between perceived seriousness and the crime-delinquency rates (see columns 2 and 7 in Tables 3 and 4). That relation also enters into what has been construed as negative test findings (i.e., the partialling of seriousness values), but defenders of the deterrence doctrine can make an argument about the relation.

If perceived seriousness of criminal or delinquent acts is a function of the perception of legal punishment and if individuals refrain from a particular act to the extent they perceive it as serious, then the crime rate is an *indirect* function of perceptions of legal punishment. However, even granting such an argument, the function of punishment is no longer deterrence but normative validation (Gibbs, 1975), meaning the generation and/or maintenance of the social (extralegal) condemnation of crime. Surely there is some significance in the difference between (1) an individual refraining from a criminal or delinquent act out of fear of legal punishment and (2) an individual refraining from or not even contemplating such an act because he or

she condemns it. The difference is significant even if prescribed or actual legal punishments somehow maintain the condemnation of crime. Nonetheless, as a last resort, defenders of the deterrence doctrine can argue that normative validation is only a secondary function of legal punishment, meaning that variation in the crime rate largely reflects the direct effects of deterrence. That argument is scarcely incredible, but it goes far beyond any existing data.

Whatever the plausibility of the contending arguments, the present data offer no basis for resolving them. It is the familiar problem of collinearity, in this case a very close direct relation between the perceived certainty of punishment and the seriousness of types of acts. Hence, as shown in Tables 3 and 4, when the perceived seriousness of acts is partialled, the relation between the perceived certainty of punishment and the frequency of self-reported crimes or delinquencies becomes negligible; but when the perceived certainty of punishment is partialled, the relation between seriousness of acts and the frequency of self-reported acts becomes negligible. Since one partial correlation cannot be justified to the exclusion of the other, the findings cannot be taken as conclusive evidence for or against the deterrence doctrine.

Nonetheless, the findings clearly cast doubts on the deterrence doctrine, or at least on one of its premises. *Until defenders of the doctrine show that the relation between properties of legal punishments and the crime rate holds independently of the social condemnation of crime*, then all purported evidence of general deterrence is suspect. In that connection, previous tests of the deterrence doctrine through interstate comparisons not only ignored perceptual variables but also failed to control for the social condemnation of crime.

Of course, the deterrence doctrine can be restated to stipulate the *kinds* of conditions in which deterrence operates independently of or is more important than social condemnation as a determinant of the crime rate. A comparison of columns 9 and 10 in Table 4 suggests one such condition. In several instances, the inverse relation between the perceived certainty of

punishment and the crime rate remains substantial when the perceived seriousness of acts is partialled, but the inverse relation between the rate and perceived seriousness disappears when perceived certainty of punishment is partialled. Observe, however, that such contrasts in the partial correlation are contingent on (1) the kind of measures (e.g., mean or median), (2) kind of values (log or absolute), (3) kind of punishment, and/or (4) kind of population. Moreover, even if the findings do show that deterrence operates independently of or is more important than the social condemnation of crime in some instances, no set of instances constitutes a generalization, let alone a theory.

Implications for Interpretation of Previous Findings

This research has considered only the perceived seriousness of types of crime as a facet of social condemnation, and the comparisons have been restricted to types of crimes or delinquencies in the same jurisdiction. Yet the findings suggest that the inverse relation between the objective certainty of imprisonment and crime rates *among states* (e.g., Logan, 1972) may reflect differences among states as regards the social condemnation of crimes. That is so because the fundamental argument applies both to differences among types of acts and to differences among jurisdictions. The argument is that a criminal law is seldom violated if the type of act is socially condemned to a marked degree, and that very condemnation prompts legal officials to pursue arrests and press for convictions. Those official activities could result in a greater objective certainty of punishment than would otherwise be the case (i.e., if the social condemnation of the type of crime were not intense), and the greater objective certainty may be perceived by the public, or they may persist in perceiving punishment in terms of "what ought to be." In either case, the combination of a low crime rate (official or unofficial) and high objective or perceived certainty of legal punishment would reflect the social (extralegal) evaluations of particular types of crimes, *not deterrence*. That possibility is all the more

important since there is no basis to assume that the social condemnation of particular types of crime (e.g., homicide) is even approximately constant from one state to the next.

All past research on the deterrence question at the aggregate level (comparisons of jurisdictions in particular) failed to control for social evaluations of criminal acts; at most, it has been recognized only conjecturally (see, e.g., Gibbs, 1968). The present findings, though limited to the perceived seriousness of types of acts, represent something more than conjecture. They suggest that research on the question of general deterrence is grossly incomplete unless it incorporates perceptual variables *and* controls for the social evaluation of crimes or delinquencies.

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STATUS, INEQUALITY AND INNOVATION: THE GREEN REVOLUTION IN ANDRA PRADESH, INDIA *

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The manner in which status influences the trial of innovations in agriculture has important implications for agrarian development policy. Analysis of data from samples of cultivators in 84 agrarian communities in Andhra Pradesh, India, found little support for Cancian's theory that rank inhibits risk-taking, even under conditions specified as necessary for such effects. Analysis of variance results suggested that increases in status among the poor might result in as much or more of the innovation expected to increase food production as would the present concentration of new opportunities among the rich. While these results appeared to be fairly consistent across different kinds of communities, innovation increased with increased inequality in the distribution of wealth (controlling for the effects of individual status). However, knowledge in inequality slightly inhibited innovation, apparently because relatively simple information about modern farming practices was widely diffused.

Explanations of individual adoption of new ideas and behaviors in the sociological and social psychological literature have included the effects of individual attitudes and motivation, individual resources (i.e., different dimensions of status) and various contextual or community level variables. Different research traditions have emphasized different combinations of these variables. On the one hand, more psychologically oriented researchers (e.g., Triandis, 1971) have seen the trial of innovations as a function of the congruence between cognitive and affective components of attitudes, perceptions regarding the worth of the innovation and certain personality characteristics (aspirations, need achievement, self-direction, and so on). This line of reasoning has stressed the motivation necessary to behavioral change. The successful introduction of new technology is seen as crucially dependent on special personal characteristics, net of the effects of indi-

vidual social position and of social context.¹

On the other hand, the observation that greater individual resources provide the opportunities necessary to risking the adoption of new ideas and behaviors has been one of the most widely reported findings in the sociological literature.² Here,

¹ Despite pleas for attention to macrostructural variables (Summers et al., 1970), relatively few investigators have dealt with the effects of social context, particularly the community (Rogers and Shoemaker, 1971). Even where system level variables have been included in the study of innovation, design and analysis often have left much to be desired. In the recent literature on innovation in India, Saxena (1971) compared system and individual effects, but ignored aggregation effects, only looked at correlations; and one of his two samples (India) consisted of only eight communities. Sandu and Allen (1974) included both community and individual level variables in a study of Punjabi cultivators, but their sampling design included only one individual in over one-third of the villages sampled. One of the largest studies of modernization yet reported (Inkeles and Smith, 1974) concluded that variables such as education, factory experience and urbanism had the same effects on individual modernism in a wide variety of cultures. In fact, Inkeles (1969:225) concluded that macrostructural variables, despite their attractiveness to theorists, had yet to "earn their place" empirically in the study of modernization.

² Rogers and Shoemaker (1971) provide a total of 869 citations dealing with the association between innovation and education, social status, and operation size, 27% of which failed to confirm a positive association. Given the tendency for nonsignificant

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the common argument has been that personality orientations intervene between individual social position and innovation (Kohn, 1969; Rogers, 1969; Inkeles and Smith, 1974; Rogers and Shoemaker, 1971). While retaining this focus on the effects of social position, other researchers (Homans, 1961; Cancian, 1967; 1972; Becker, 1970; Prattis, 1973) have placed less emphasis on individual personality traits as the intervening factor between status and innovation. Instead, these authors have cast their explanations of innovative behavior in terms of individual risk-taking. They argue that status affects the utilities associated with events in a payoff matrix, and that individuals act to maximize the expected value of their outcomes. Different arguments have been proposed for the relationship between status and perceived utility with the attendant prediction of different functional forms of the relationship between status and innovation. Homans (1961:336-58) argued that lower status persons have little to lose by innovating, and those of high status can afford to innovate and are expected to lead. However, persons of middle level status innovate less because they have been rewarded for conformity in the past. Cancian (1967; 1972) has argued that those of upper-middle status will be more conservative in terms of innovation than those of lower-middle status, but that both of these ranks will innovate more than lower status persons and less than higher status persons. This line of reasoning, which suggests a cubic relationship between status and innovation, will be considered in more detail below.

Finally, several authors (Becker, 1970; Flinn, 1970; Marsh and Coleman, 1956; Van der Ban, 1960; Prattis, 1973) have added the notion that community norms and values influence the perceived utilities of innovation. If an innovation is not normatively accepted, persons not subject to normative influence (for example, low status marginals) will innovate first. If the innovation is positively valued by the community, leaders and individuals

located in cooperative organizations designed to spread risks will innovate first.³ This argument parallels that of Homans (1961) in implying a U-shaped relationship between status and innovation. However, little attempt has been made to determine the effects of the level and distribution of community resources on innovation or on the relationship between individual social position and innovation.

Accordingly, in an examination of agricultural innovation in rural communities in Andhra Pradesh, India, the present analysis seeks to answer the following questions: (1) is the functional form of the relationship between status and innovation linear for different dimensions of status; (2) what effects do various statuses have on innovation net of other individual statuses; (3) what are the main effects on agricultural innovation of variation in community opportunity structures (the level of wealth and degree of inequality); (4) are the relationships between status and innovation the same in communities which differ in level of wealth and degree of inequality; (5) what are the policy implications of these findings for government programs aimed at agrarian change?

Given the tremendous investment in new agrarian technologies represented by efforts to introduce the Green Revolution in less developed nations, these concerns have direct policy relevance. If the effects of status on innovation are indeed linear, then equally great gains in innovation could be made by increasing the status of the poor as by directing new resources to the rich. Such a policy would result in greater equality as well. However, if the status-innovation relationship is exponential or U-shaped, then there may be advantages (besides political ones) in a policy that allows the increased concentration of resources in the hands of the rich, even if such a policy exacerbates inequality.

If innovation proved to be exponentially greater in wealthier communities,

results not to find their way into the literature, the number and proportion of such "failures to confirm" probably is underestimated by such a count.

³ This phenomenon also has been seen in the tendency of group situations to produce riskier decisions than individuals acting on their own—the "risky shift" phenomena noted in experimental psychology.

this might help to vindicate government policy directed at introducing new technology where complementary factors of production already exist. If a more equal distribution of wealth and knowledge provided a situation where innovation was higher, then efforts at redistribution might have important direct effects on innovation. If increases in innovation from increases in resources were greater in communities with lower inequality or greater wealth, then new inputs might yield higher returns in such situations.

A Modified Curvilinear Hypothesis

In an examination of status and innovation in agrarian settings, Cancian (1967; 1972) actually began from the hypothesis that, if certain conditions were met, economic risk-taking and innovation would be negatively correlated. "The better your financial position, the more you have to lose and the less you have to gain from taking chances; therefore, insofar as adopting an innovation is risky, the richer you are, the less likely you are to adopt" (Cancian, 1967:912). The higher the social status, the more likely a random change would result in downward rather than upward movement in a stratification hierarchy. However, these "inhibiting" effects of status on innovation would hold only where: (1) all risks were perfectly divisible; (2) knowledge was spread equally over all ranks; (3) the risks necessary to maintain any position in the stratification system were equal; (4) no individual could suffer total loss on a single risk; (5) nobody had so many resources that he was completely protected from loss of rank. Cancian argued that it has been precisely the common lack of such conditions which has resulted in the often observed "facilitating" effect of status on innovation, and it is the specification of these system conditions which makes his argument all the more interesting. Modifying Homans (1961), Cancian (1967:915-6) argued that the inhibiting and facilitating effects of resources on innovation would be manifest as a middle-class conservatism. Those of high status could afford to take "flyers" (lack of condition 5) and are therefore highly innovative, while those of

very low status might actually suffer total economic extinction (lack of condition 4) and are therefore very conservative. Or, perhaps, high status individuals must take calculated risks to maintain leadership (lack of condition 3). The lower-middle class were innovative because they had not been highly rewarded for conformity, while the upper-middle class found the costs of nonconformity to be higher. Despite a positive overall trend, increases in innovation due to increases in status would be lower (or even negative) over the middle part of the resource dimension.

This hypothesis also can be supported by rationales expressed in terms of differences in the opportunities available to those at the lower-middle and upper-middle ranks. The lower-middle status peasant cultivator already produces something beyond subsistence based primarily on his own family's labor. He may be able to take advantage of new technology because he can provide the additional labor requirements using his family. Because upper-middle status peasants utilize their families' labor more fully, they may have to hire labor in order to meet the increased labor demands of new seeds, fertilizers and pesticides. Even if they attempted to mechanize in order to reduce labor requirements, they still would be vulnerable to the influence of the upper strata, who often control hired labor and access to equipment and loans. The relatively well-to-do peasant cultivator may decide not to adopt an innovation because he cannot easily obtain necessary complementary production factors without increasing his vulnerability *vis-à-vis* local elites. Even where this is not a factor, he must risk a change in the source of his labor inputs as well as adopting the new technology (violating condition 3). This reasoning would suggest that the relationship between status and innovation would best be described by an exponential curve or perhaps by Cancian's third degree polynomial.

Following Cancian's work, other authors have noted middle-class conservatism effects (Fliegel et al., 1968; Kivlin and Fliegel, 1968; Bluhm and Fliegel, 1973) or have cited Cancian's demonstration of upper-middle class conservatism

(Stanfield and Whiting, 1972). On the other hand, several replications of Cancian's research (Converse and Hickey, 1975; Gartrell et al., 1973; Morrison et al., 1976) have failed to find evidence to support the presence of inhibiting effects of status on innovation. However, none of these investigations has fully addressed the conditions specified in Cancian's theory.

Research Design

This report begins with a replication of Cancian's hypothesis that those of lower-middle rank will innovate more than those of upper-middle rank, which Cancian (1972:141) claims is at the heart of his theory. It then examines the system conditions specified as necessary to the operation of the inhibiting effects of resources on innovation. Finally, it will reformulate the hypotheses within a regression analysis framework paying particular attention to: (1) the possibility of non-linear effects and (2) the effects of system conditions (here, community characteristics) on innovation.

The setting chosen for the present investigation was agrarian India, and the innovations studied were principally those involved in the Green Revolution—the combination of new seeds, chemical fertilizers, pesticides and improved tools and techniques that was supposed to render India self-sufficient in food grain production. The development efforts directed toward these innovations involved voluntaristic programs relying on persuasion, cooperation and self-help. The incentives and facilities provided often have been distributed through existing stratification systems, and inputs have been concentrated where complementary factors of production, particularly an assured water supply, already existed (Frankel, 1969; 1971). The usual reasoning that this process leads to greater inequality (Gotsch, 1972) needs to be reexamined if Cancian's hypothesis as to the inhibiting effects of status on innovation proves correct. If innovation leads to greater wealth and lower-middle peasants innovate more than the upper-middle ones, then inequality would diminish.

Sampling

The data were collected in a 1970 survey of 84 rural agrarian communities in Andhra Pradesh, India. The state of Andhra Pradesh (40 million people in an area of 106,000 square miles) is located in southcentral India. It includes both extensive areas of dry cultivation on the inland Deccan plateau (Telangana region) and relatively well-developed and densely populated rice-growing areas centered on irrigation from the Godavari, Krishna and Penna river systems along the Bay of Bengal (the Andhra region). Andhra Pradesh thus provided the wide variation in levels of development necessary to the examination of the effects of community characteristics on innovation.

Interviews with 336 knowledgeable informants based on their records and with 84 representative samples of heads-of-household (2,040 in all) were used to generate estimates of community characteristics. Fourteen villages with populations between 200 and 5,000 in 1961 were selected from census lists for each of six districts in Andhra Pradesh. The sample of villages was proportionate within high and low irrigation areas (based on the 1964–1967 average gross irrigated area divided by gross cultivated area) and the districts chosen were those ranked first, fourth and ninth in each of two regions (Telangana and Andhra) on a six-year average (1961–1967) of gross farm income per cultivating household. Since richer districts in Andhra Pradesh have fewer villages, such a sampling design over-represented relatively well-developed villages in order to expand the variance in developmental conditions within the sample.

Within the 84 villages, heads-of-household were selected in proportionate random quota samples within three strata: cultivators, agricultural laborers and others. Sampling fractions decreased as village size increased with samples ranging from 15 out of 40 households to 60 out of 863 households. While the representation of cultivators (the focus of this analysis) was proportionate within each village, the over-representation of richer villages probably insured the over-representation

of richer villagers. This may well be desirable in a situation where the distribution of resources generally is highly positively skewed, since it should yield more accurate estimates of the upper portions of the resource continuum. However, caution should be exercised in generalizing results, even to the state of Andhra Pradesh.

Replication

As we have seen above, Cancian (1967; 1972) hypothesized that those of low-middle status would innovate as much or more than those of high-middle status. In his analysis, status was conceptualized as wealth and measured in different data sets by volume of inputs, size of operation and income (Cancian, 1967:912). Cutting points in Cancian's four-rank scheme were chosen to give equal proportions at each rank (quartiles), and high risk takers were operationally defined as the top quartile of innovators. In the present analysis, the total sample ($n=2,040$) was used in estimating community characteristics, but only the heads-of-household who reported gross farm income from cultivation in 1969-1970 were included in analysing variation in trial of innovations. High risk-taking was operationally defined as the reported trial of ten or more (to a maximum of 32) innovations.⁴ Wealth was operationally defined as the sum of Rs., livestock, tools, land and agricultural buildings owned by the household.

Recognizing that status systems are multidimensional, I included: (1) a measure of jati rank as an indicator of ritual status;⁵ (2) an index of level of living con-

structed by summing the number of modern housing characteristics, luxury goods and manufactured household possessions; (3) Rs. spent by the household on jajmani services in the year before the survey as an indicator of "traditional" consumption levels;⁶ (4) the average number of years of schooling received by adult males (married or 21 years or older) as an indicator of educational resources in the household. Cutting points (Appendix A) defining the four rank levels were not chosen to produce quartiles. Rather, they were defined *a priori* to provide what appeared to be reasonable divisions for the sample as a whole (including noncultivators). Since cultivators are generally the most wealthy villagers and since resource distributions are usually positively skewed, such divisions are more consistent with the structure of village stratification systems than are the rectangular distributions implied in ranks of equal proportions (Morrison et al., 1976).

Table 1 reports the percentage of cultivators at each rank who are high innovators. These results consistently support the hypothesis that individuals of low-middle rank will risk less than those

obtain a common caste ranking for several villages are problematic. Here the jati identification of heads-of-household was obtained in the interview and a post hoc scale for ritual rank was constructed. This process began from the ranking found by Lakshman (1973) through the examination of patterns of interdining, etc. and, with the help of several anthropologists and the project's interviewers, we fitted in the other jatis. The detailed scale ranged from 99 for the highest Brahmins to 10 for the lowest "untouchable." The division used in Tables 1 and 2 (see Appendix A) includes all those with less than Sudra status in the lowest rank, all the Sudras except the highest in the next level, and all the twice-born jatis (Vaisyas, Kshatriyas and Brahmins) in the top level.

⁶ The jajmani system of relationships within Indian villages principally involves the exchange of food for goods and services, although about one-half of the transactions are now in cash. The services involved include those rendered by village artisans such as barbers, carpenters, blacksmiths, potters, weavers, priests and washermen. Links are between families rather than the jatis (castes) concerned. This system of relationships involves much more than the simple economic exchange of food for goods and services, since it includes ceremonial obligations and general obligations of mutual support between the families involved. Factional conflict usually involves the mobilization of jajmani associates.

⁴ Cancian selected the highest (or earliest) quartile of innovators because the first to try something or those who try the largest number of things presumably run the most risks. In the scale used here, scores between 10 and 32 represent the top 21.9% of the distribution. The innovation index was a count of the reported number of items of new technology tried (largely the "Green Revolution" technology of new seeds, fertilizers, pesticides, etc.). Respondents first were asked open-ended questions as to whether they knew anything about various types of new technology (for example HYV seeds) and, if they said "yes," they were then asked to specifically name those they knew about. Of those they correctly identified, the ones they said they had tried were included in the index of trial of innovations.

⁵ As Morrison et al. (1976) point out, attempts to

Table 1. Replication of Cancian's Hypothesis of (Upper) Middle-Class Conservatism

Resource Indicators	Resource Levels*							
	Low		Low Middle		High Middle		High	
	%	N	%	N	%	N	%	N
1. Jati Ritual Rank	4.1	319	12.6	269	32.6	641	37.6	85
2. Adult Male Education Years	9.6	697	14.6	158	39.4	358	55.3	103
3. Rs. Jajmani Services	2.4	207	14.1	681	35.5	335	73.1	93
4. Rs. Capital	4.4	68	4.7	450	18.6	420	49.2	378
5. Level of Living	5.2	232	10.4	531	22.2	302	61.4	251

* % = percentage of those at each resource level who were high innovators.
 N = number of observations at each resource level.

of high-middle rank. There is no evidence to support either Cancian's inhibiting effects of rank or the notion of middle-class conservatism. The relationship between resources and trial of innovations appears to be both positive and monotonic for each of the five different kinds of resources.

Extension: Controlling for the Structure of Stratification Systems

In setting conditions necessary to the operation of the inhibiting effects of resources on innovation, Cancian implied that the relationship would vary across social stratification systems. On examining the divisibility condition, it appeared that most of the elements of the Green Revolution technology aimed at rice (high-yielding variety seeds, chemical fertilizers, pesticides, etc.) were applicable to any amount of land at roughly the same cost per acre (high divisibility). They were certainly far more highly divisible than are the technologies involved in mechanization (tractors) or irrigation (tube wells). Second, Cancian made the inhibiting effects of status conditional on the equal distribution of knowledge across ranks. In this sample, knowledge inequality was measured as the Gini coefficient for the distribution of knowledge about modern agricultural technology across the household sampled in each village,⁷ and could

therefore be statistically controlled. According to Cancian's theory, the inhibiting effects of status should be more evident under conditions of low inequality in the distribution of knowledge.

In addressing Cancian's third condition, that the risks necessary to maintain any position must be equal, it seemed reasonable to expect that risks would be more equal where inequality in the distribution of wealth and knowledge were low. Inequality in the distribution of wealth was operationally defined as the Gini coefficient for the distribution of farm capital (the sum of Rs., land, livestock, machinery and farm buildings) across village household. Presumably, the inhibiting effects of resources on innovation would be stronger where there was lower inequality.

The fourth condition, that no individual be liable to suffer total loss on a single risk, is not usually satisfied in poor villages in underdeveloped countries. In these circumstances, a large proportion of subsistence farmers may suffer total loss of resources or fall into debt, particularly with vagaries of climate and disease (Nair, 1961). The level of wealth in the community can provide a cushion for such losses, besides making it less likely that scarcity will affect any cultivator. However, this is also dependent on the degree of inequality in the distribution of wealth, since there

⁷ The Gini coefficient of concentration was calculated by the algorithm $\sum_{i=1}^n (Y_{i-1}X_i) - (X_{i-1}Y_i)$ where X_i is the cumulative proportion of value in the i th category (Aker and Russett, 1964). However, the coefficient so calculated cannot reach unity and is limited to the value of Y_i where $i=N-1$ which is

$1-1/N$ (Ray and Singer, 1973). While the distortion introduced by this limitation can be serious for grouped data, the problem is easily solved by dividing the Gini coefficient by $1-1/N$. All coefficients used here are multiplied by 100 to express inequality as a percentage of area between the Lorenz curve and the line of equality.

still can be very poor cultivators in rich villages where wealth is very unequally distributed. Control on both these factors should allow us to see the inhibiting effects of status on risk-taking.

Finally, it is unlikely that the rich are completely protected from loss of rank (condition 5). Even with the highly skewed distribution of resources characteristic of many Indian communities, fortunes have changed. The land tenure reforms carried out in the early 1960s did succeed in reducing the absentee landlord (Zamindar, Jagirdar) to a much lower status, just as the combination of low taxation (Joshi et al., 1968; Rao, 1966) and new development inputs have given rise to a new rural elite (Rosen, 1967). The jajmani system has been changing for some time (Lewis, 1965; Wiser and Wiser, 1965) and, with ritual status, has become somewhat less important. However, high inequality remains and with it a situation where the local village elite may be relatively immune, at least in the short run. The greater the inequality in the distribution of wealth, the more protected are the elite and the more likely they are to take "flyers" because they have little to lose (violating condition 5).

As I have interpreted it, Cancian's theory predicts that the inhibiting effects

of rank are most likely to occur in relatively well-to-do communities where inequality in the distribution of wealth and knowledge is relatively low. If this is correct, controlling for these three dimensions of village stratification systems should give Cancian's predictions a better chance of gaining support. In this sense, inequality and level of wealth are "specifier" variables whose control allows specification of the direction of the relationship between status and innovation (Davis, 1971).

Table 2 examines the relationship between different kinds of status and trial of innovation controlling for each of the community characteristics in turn. Cultivators were divided into those who lived in villages with low and high levels of wealth and inequality with cutting points set at the means. Cancian predicted that those of lower-middle rank would innovate more than those of high-middle rank, particularly under low inequality or high resource conditions. Comparisons of the percentage of high innovators at each of these ranks uniformly failed to support this hypothesis for any of the five types of status under any of the three conditions. Even the differences between the percentages were not systematically lower in low inequality or high resource com-

Table 2. Percentage of High Innovators within Resource Ranks Controlling for Community Characteristics One at a Time

		Community Characteristics					
		Gini Index for Agricultural Knowledge		Gini Index for Rs. Capital		Average Rs. Capital per Household	
		10.5- 38.99	39- 78.4	39.4- 69.99	70- 97.8	1,467- 18,273	18,274- 66,733
Resources	Rank	Low	High	Low	High	Low	High
1. Jati	LM	3.35	8.92	7.06	5.58	7.06	5.58
Rank	HM	19.03	13.57	15.60	17.00	12.32	20.28
2. Adult	LM	10.13	4.43	5.06	9.49	5.06	9.49
Male	HM	18.16	21.29	17.60	21.78	13.40	25.98
Education Years							
3. Rs. Jajmani	LM	7.20	6.90	5.87	8.22	4.85	9.25
Services	HM	18.21	17.31	18.81	16.72	11.94	23.58
4. Rs. Capital	LM	2.00	2.44	1.11	3.56	2.22	2.44
	HM	8.81	9.76	12.14	6.43	10.24	8.33
5. Level of	LM	6.40	3.95	5.27	5.08	3.39	6.97
Living	HM	12.58	9.60	13.58	8.61	8.94	13.25

munities. Evidence for either middle-class conservatism or the inhibiting effects of status was conspicuous by its absence.

Cancian's theory might be interpreted to imply that the lack of any of his five conditions was sufficient to allow facilitating effects of rank to predominate over inhibiting effects. Each of the conditions specified would then be necessary but not sufficient for a negative relationship between resources and innovation. Only when all five conditions were met would lower-middle ranks be expected to innovate more than upper-middle ranks. This hypothesis represents what might be labeled a "weak" test of the theory, since it involves only the two middle ranks—a hypothesis which Cancian expected to hold even when these "favorable" conditions were not present.

Table 3 reports the percentage of high innovators by rank for the subsample of cultivators living in relatively wealthy communities where there was also low inequality in the distribution of both wealth and knowledge. These cultivators presumably resided in villages where the structure of the stratification system was most likely to produce inhibiting effects of status. The results in Table 3, however, provided no support for this contention. The relationship between resources and innovation was positive and monotonic with only minor exceptions. Furthermore, none of the exceptions to monotonicity involved differences between low-middle and high-middle ranks. Those with high jati ritual status had a lower percentage of

high innovators than did cultivators of high-middle ritual status. The high ritual status rank included the three twice-born varnas⁸ whose traditional occupational pursuits were not farming, while those of high-middle status (the Sudra jatis) were traditionally cultivators. In fact, religious norms traditionally proscribed active participation in cultivation by Brahmins (the highest varna). More importantly, Sudra jatis in Andhra Pradesh (60.3% of this subsample) are often numerically, politically and monetarily the dominant caste in the village—a position that yields distinct advantages in access to government development programs.

While these tabular results provide a fairly unambiguous replication and extension of Cancian's theories, they do not

⁸ Varna (literally "color") refers to the four broad classes in Indian society (Brahmins, Kshatryas, Vaisyas and Sudras) that crystallized during the Rg Veda period (roughly 1500 B.C. to 900 B.C.) and survive to the present day. The term "caste" (not to be confused with Varna) first was applied by the Portuguese to the many separate groups within the Hindu community (tribes, clans or families). Castes (a system of groups within the four Varnas) are normally endogamous, commensal and roughly craft exclusive (except for agriculture). They are therefore composed of people related by blood or marriage and are usually restricted to a particular linguistic region. Jati is a term from the Indian literature meaning caste. The twice-born castes refer generally to those of the three highest Varnas who were born again (Upanayana) when they were invested with the sacred thread initiating them into the Aryan community. Sudras and lower castes were never allowed to undergo this ceremony, nor were they allowed to hear or learn the most sacred scriptures (Basham, 1959).

Table 3. Percentage of High Innovators at Different Resource Ranks: Cultivators in High-Resource, Low-Inequality Communities (N=292)

Variable		Rank			
		L	LM	HM	H
1. Jati Ritual Status	%	3.9	10.2	29.5	12.5
	N	51	49	176	16
2. Rs. Jajmani Services	%	0.0	12.3	30.5	76.5
	N	26	154	95	17
3. Rs. Farm Capital	%	0.0	0.0	16.2	34.1
	N	6	52	105	129
4. Level of Living	%	12.5	12.7	22.9	35.8
	N	24	118	83	67
5. Adult Male Education Years	%	12.4	14.3	33.3	42.9
	N	161	28	75	28

provide sufficient information to examine the policy-relevant concerns outlined initially. We cannot assess whether or not there would be greater returns of innovation to increases in resources among the rich. Neither can we fully evaluate the independent impact of different dimensions of status and community characteristics on innovation. We have established only that, at first glance, higher status individuals generally appeared to be more inclined to take risks by adopting agricultural innovations regardless of the characteristics of the village stratification system in which they lived.

Regression Analysis: The Functional Form of the Status-Innovation Relationship

As we might expect from the results observed above, the zero-order correlations between the five status dimensions and innovation were all positive (Appendix B). Cancian's (1967; 1972) hypothesis would suggest that where y = innovation and x = resources, a polynomial in the form

$$y = \alpha + \beta_1 x_1 - \beta_2 x_1^2 + \beta_3 x_1^3 + u \quad (1)$$

would yield a more accurate description than would simple linear regression. If such were the case, a program aimed at redistributing land from the rich to the poor (making all holdings middle-sized) might have negative consequences for innovation. Also, a comparison of estimated slopes over different portions of the resource continuum might give some clues to where new inputs might be most profitably invested in order to maximize innovation.

Several modifications to these predictions might also be anticipated. Since the poorest villagers (agricultural laborers) were omitted from the analysis, the observation of a second degree polynomial (quadratic) in the form

$$y = \alpha - \beta_1 x_1 + \beta_2 x_1^2 + u \quad (2)$$

might satisfy Cancian's arguments. This parallels his comments on the studies cited by Homans (1961:336-58) in support of a U-shaped curve. This hypothesis would imply that increases in resources

for cultivators with low status could be counterproductive, while increases in status for the well-to-do would result in increased innovation. One might also extend this reasoning to predict an exponential curve in the form

$$y = e^{\alpha + \beta_1 x_1 + u} \quad (3)$$

if the lowest statuses were omitted from what was, in fact, a U-shaped quadratic relationship (2). In this case, while returns to innovation from increase in status generally would be positive, returns to increases in resources would be greater the higher the level of the status.

Table 4 reports one-way analysis of variance results relating each dimension of status to innovation. While the large sample size tended to yield significant differences between n^2 and r^2 (a test for non-linearity) in several instances, plots of the curves indicated that innovation appeared to be a roughly linear function of both male adult education and level of living. Increases in education or level of living among relatively disadvantaged cultivators would appear to result in approximately the same increase in innovation as would increases in these resources for those who have relatively high status.

On the other hand, the relationship of jajmani services received and wealth with innovation appeared to be best described by the logarithmic function

$$y = \alpha + \beta_1 \log x_1 + u. \quad (4)$$

Log Rs. jajmani services and log Rs. wealth were significantly more highly correlated with trial of innovation than were the untransformed versions of these variables (Appendix B).⁹ As the level of household wealth and jajmani services received increased, the rate of increase in innovation decreased. Increases in these statuses (the only ones measured in monetary terms) would appear to produce more innovation among those of low status than among those of high status. Presumably, redistribution of such resources aimed at

⁹ Comparisons of these correlations with those for the untransformed versions of these variables yielded $t=2.58$ ($P<.002$) for wealth and $t=8.97$ ($P<.001$) for jajmani services received. Other than in these two instances, simple linearizing transformations did not significantly increase the amount of variance shared between status and innovation.

Table 4. Resources and Trial of Innovations: One-Way ANOVA (N=1,314)

1. Jati Rank			2. Jajmani Services			3. Rs. Capital			4. Level of Living			5. Male Adult Ed. Years		
Interval	N	\bar{X}	Interval	N	\bar{X}	Interval	N	\bar{X}	Interval	N	\bar{X}	Interval	N	\bar{X}
Lowest Harijans	213	4.49	0-49	207	3.46	1-999	68	3.52	1-3	49	3.12	0	670	5.12
Middle & Upper Harijans	51	4.12	50-99	318	5.36	1,000-4,999	224	3.69	4-6	183	4.37	.01-1.49	27	7.22
Lower Sudras	64	3.88	100-199	363	6.82	5,000-9,999	226	5.32	7-9	261	4.88	1.50-2.99	111	6.33
Middle Sudras	260	5.86	200-399	230	8.41	10,000-17,999	244	6.54	10-12	270	6.41	3-5.99	283	8.22
Upper Sudras	641	8.32	400-799	105	9.51	18,000-29,999	176	7.72	13-15	184	7.08	6-8.99	122	9.40
Pancha Brahma, Vaisya, Kshatriya	46	5.15	800-1,599	49	10.53	30,000-49,999	152	8.38	16-18	118	7.67	9-11.99	80	11.34
Brahmins	39	12.03	1,600-3,199	31	12.77	50,000-99,999	125	9.64	19-23	153	9.99	12-20.5	23	13.91
Total	1,314	6.83	3,200-20,705	13	19.08	100,000-669,555	101	12.75	24-31	98	13.26			
	$F=56.5$ $n^2=.206$	$r^2=.141$ $F=21.5$	$F=94.86$ $n^2=.337$	$r^2=.323$ $F=4.60$	$F=102.96$ $n^2=.355$	$r^2=.342$ $F=4.57$	$F=102.24$ $n^2=.354$	$r^2=.327$ $F=9.10$	$F=72.42$ $n^2=.249$	$r^2=.238$ $F=4.03$				

reducing inequality also would increase innovation.

The relationship between jati ritual rank and innovation was somewhat more complicated. There was little change in innovation over the first three levels of jati ritual rank. These levels represented relatively poor cultivators who were at the very bottom or below the four-rank Hindu varna scheme. They resort, at times, to hiring themselves out as agricultural laborers or they may pursue other low status "artisan" occupations (washerman, potter, weaver). Over the next two levels (middle and high Sudras), there were increases in innovation. These Kapu jatis traditionally have been cultivators and are more likely to pursue cultivation full-time. More important, as mentioned above, they are often numerically, monetarily and politically the dominant caste group in Andhra and Telangana villages (Eliott, 1970). For the same reasons, it is not surprising that the Pancha Brahma castes¹⁰ and the Vaisya jatis (traditionally merchants) showed lower levels of innovation. Their economic fortunes often have been much less than those of the Sudras and many may still pursue their "traditional" occupations in addition to growing crops. They formed only a small proportion (3.5%) of the cultivators in the sample, most of whom were either Sudras or Harijans. Finally, the Kshatriyas and Brahmins (3% of the sample) had the highest innovation scores. Brahmins who cultivated land did not seem to be particularly constrained by religious norms proscribing active participation in cultivation.

If the Pancha Brahma and Vaisya jatis were considered to be the upper-middle rank, then innovation for this rank was lower than that for the next lowest group, the upper level Sudras. While this would appear to be exactly the result predicted by Cancian's theory, ritual caste status is

¹⁰ The ritual rank assigned to these jatis (traditionally carpenters, blacksmiths, sculptors and goldsmiths) is questionable. Dube (1967) placed them below the upper Sudra castes in his study of Sharmirpet (a village in Telangana close to Hyderabad), but noted that even the untouchable Madiga viewed the taking of food or drink from these castes as "bad luck." On the other hand, Pancha Brahma castes will not accept food or drink from even the Brahmins.

practically immutable, since one is born into a jati. Therefore, it cannot be risked, and Cancian's fifth condition would be violated. As a consequence, there should be only positive effects of ritual rank on innovation (Morrison et al., 1976). In fact, while the relationship was positive, it was hardly a simple linear function. Even the nonlinear eta-squared for jati ritual rank and innovation was lower than the zero-order linear correlation for the other status dimensions (Table 4). To summarize, the relationship between jati ritual status and innovation was both complex and relatively weak. Although it was difficult to measure accurately, ritual status per se appeared to have had far less influence on innovation than many sociological and anthropological writings on India would have led us to believe.

Increases in innovation appeared to be a linear function of both education and level of living. Equally large gains in innovation could be made by distributing new resources of these kinds to the poor rather than to the rich. Since independence, emphasis on providing free universal elementary education to villagers may well pay off in both higher innovation and greater equality. This would occur as cohorts which have had the advantages of this basic education become cultivator heads-of-households.¹¹

Finally, innovation was a linear function of the logarithm of jajmani services received and total Rs. agricultural capital (wealth). The higher a head-of-household's status on these resource di-

mensions, the lower the increase in innovation per unit increase in status. The effects of caste on innovation appeared to be manifested more through services rendered in the village jajmani system than through ritual status per se. However, increased participation in this system of exchange had increasingly lower payoffs in innovation the higher the level of services received.

Besides the obvious effects on inequality, redistribution of land (the principal component of farm capital) might result in greater innovation. However, government programs appear to have been far less successful in redistributing wealth than they have been in increasing educational equality. While there has been an obvious commitment to greater equality in government rhetoric, land reform has not radically altered the distribution of land (Singh and Misra, 1964:148; Sharma, 1967: 231; Indian Institute of Public Opinion, 1968:58). Settlements with Zamindars were perhaps too generous, as were land ceilings which were applied to individuals instead of families or even households. Nonretroactive legislation was conveniently delayed to allow circumvention of the law. Tenancy legislation often resulted in many tenants losing their rights when owners resumed cultivation of their lands using managers and hired labor (Varghese, 1970; Sonachalam, 1970; Government of India, 1966). Taxation based on land remained regressive, while inflation drastically reduced its impact (Rao, 1966; Joshi et al., 1968). In short, agricultural wealth was not effectively redistributed by reform legislation. In addition, the effects of community development and agricultural extension programs were directed primarily at wealthier villages in wealthier regions and largely at relatively well-to-do cultivators (Frankel, 1972; Parthasarathy, 1974; Tripathy, 1974; Gotsch, 1972; Myrdal, 1968). If the present results for cultivators in 84 villages in Andhra Pradesh are any indication, agrarian development does not appear to have proceeded in a manner most conducive to the innovation expected to yield higher food production.

¹¹ Each of the 84 randomly selected villages had an elementary school. Inequality in the distribution of education for adult males was still high ($\bar{X}=73.41$) and levels of education were still low ($\bar{X}=2.27$ years). However, there was some evidence that new educational inputs have been going to the disadvantaged, since village educational levels and inequality in the distribution of education were negatively correlated ($r=-.63$). Differences between villages in education levels may have been partially a function of the timing of these new inputs. Communities which received schools first would be more likely to show the effects in adult cohorts. However, education beyond the primary grades remains a considerable expense (travel, lodging, books, etc.), since higher level schools are usually located in towns.

Status Dimensions as Predictors of Innovation

Each of the five dimensions of status had fairly substantial effects on innovation (zero-order correlations ranging from .328 to .589). However, when innovation was regressed on all five status variables simultaneously, the influence of jati ritual rank was not significant. Controlling for the other four status variables (Table 5), its fifth-order partial correlation with innovation was virtually zero ($r = -.006$) and it was therefore omitted from the equation reported in Table 5. Its effects appeared to be indirect through other aspects of status and only in this sense was it a barrier to innovation and change. These results paralleled those of Roy et al. (1968) and provided further support for the contention that caste status is not the overwhelming determinant of behavior in village India.

The multiple regression results also showed log farm capital to have only modest effects on innovation net of other status dimensions ($\beta = .145$). While differences between the betas were based on only small differences between the zero-order correlations (Appendix B), one possible explanation for the relatively weak impact of wealth on innovation may lie in the partially commercialized nature of agricultural production in Andhra Pradesh.¹² Also, the relative importance

of capital investment may have been lower, since labor-intensive technologies were dominant particularly for the Green Revolution technology organized around high-yield varieties of rice. However, a number of conditions have prevented the greater utilization of labor: climate, low nutritional and health standards, poor wages and institutionalized conditions which insure the poverty of laborers' economic and social position, little training or development of laborers' skills, and the relative lack of labor mobility along with the imperfection of markets that results. As Myrdal (1968) argued, investment in new technology cannot be utilized effectively until such conditions are altered to allow the improvements in labor efficiency necessary to increased production.

The effects of level of living may have reflected underlying dimensions of modernism—the consumption of modern goods and the trial of modern agricultural technology. In fact, Rogers (1969) has treated similar indices of level of living precisely in this manner. However, increases in both traditional and modern forms of consumption appeared to have resulted in increased innovation net of other dimensions of status. Maintenance of traditional jajmani service relationships within the village appeared to be important in this respect, and it was this aspect of the caste system that was important

¹² For example, an average of 47.58% of the crops in a village were sold ($S=16.41$), while the average percentage of jajmani services paid for in cash was 40.76 ($S=24.31$). While many cultivators still do not think in monetary terms and keep no records, there

appears to have been a rather slow shift from exchange in kind towards exchange in cash (compare with Iyengar, 1931). Because of the substantial inflation since World War II, this change has been resisted by those providing the service or labor.

Table 5. Status and Innovation: Multiple Regression Results

Variable	B	Std. Error B	F	Standardized Beta	Std. Error Beta
1. Level of Living	.160	.022	54.01	.233	.032
2. Log. Rs. Jajmani Services	.942	.096	96.02	.270	.027
3. Education Years	.208	.033	39.61	.177	.026
4. Log. Rs. Capital	.402	.083	23.25	.145	.030
Constant	-4.10	.611	45.04		
R=.673	ANOVA	df	SS	MS	F
R ² =.453	Regression	4	11,134.11	2,783.53	270.83
S _e =3.21	Residual	1,309	13,453.83	10.28	

rather than ritual status per se. Certainly, the positive sign of the coefficients for both consumption variables failed to indicate any support for arguments suggesting a trade-off between consumption and innovation due to scarce resources.

The positive effects of education ($\beta = .177$) suggested that increased innovation from recent increased inputs of primary education will operate independently of other status dimensions. On the other hand, there is disturbing evidence that the poor in rural India have not enjoyed gains in their level of living over the 1960s and can look forward to more of the same in the 1970s (Government of India, 1968; Dandekar and Rath, 1971). If gains in the level of living continue to go to those who already enjoy a relatively high standard of living, increased innovation may be gained only at the cost of greater inequality.

In summary, the effects on innovation of traditional and modern consumption, education and wealth were all positive. Together these four dimensions of status accounted for 45.3% of the variance in trial of innovations. The effects of caste ritual status on innovation appeared to be indirect through these dimensions of status. While the trial of new technology was linked more closely to consumption than to capital resources, one should not emphasize these differences in partial regression coefficients too much, since they were based on rather small differences in zero-order correlations (Gordon, 1968). More importantly, wealth is obviously necessary to the purchase of jajmani services, housing improvements, modern consumer goods, education beyond the primary level, and the new Green Revolution technologies themselves. In this sense, the effects of wealth on trial of innovation may be partially indirect through consumption and education.

The same three dimensions of community stratification systems introduced above (Cancian's conditions) were considered as predictors of innovation: (1) the degree of inequality in the distribution of farm capital across household in the community (wealth inequality); (2) the degree of inequality in the distribution of knowledge about modern agricultural

technology (knowledge inequality); (3) the average household farm capital (level of wealth). While there was little well-formulated theory to "fall back on" in attempting to link these community characteristics to innovation, I speculated that inequality would impede the diffusion of innovation and that higher levels of wealth would enhance it. The reasoning behind this prediction of negative effects for inequality rested on the notion that people of similar social status were more likely to share information and to provide the social support necessary to the reduction of risks. Indeed, the latter appears to have been particularly relevant for a sample of Pennsylvania farmers (Fliegel et al., 1968). Stanfield and Whiting (1972:414) noted that Brazilian farmers in communities with less land-ownership concentration were somewhat more innovative, although they concluded that "the degree of land-ownership concentration produced disappointingly few direct effects."

On the average, cultivators faced stratification systems with relatively few resources present where knowledge was relatively widely diffused, but wealth was highly concentrated. Both wealth and knowledge inequality had significant independent effects on trial of innovation (Table 6), but the level of community wealth did not.¹³ While it was positively correlated with innovation ($r = .236$, Appendix B), it had little effect independent of individual status. The small negative effects of knowledge inequality on innovation lent only weak support to arguments that knowledge inequality inhibited the diffusion of innovation. On the other hand, as opposed to results reported by Stanfield and Whiting (1972), inequality in the distribution of wealth had substantial positive direct effects on innovation. Beyond the notion that high inequality reduced risks to status, high inequality also positively covaried with the presence of the labor resources necessary to the implementation of Green Revolution innovations ($r = .66$). In addition, the positive

¹³ The partial correlation of level of wealth and innovation controlling for the variables in Table 6 was $-.003$; that for jati ritual status with the same controls was $-.001$.

Table 6. Effects of Community Characteristics and Individual Status on Trial of Innovation

Variable	B	Std. Error B	F	Standardized Beta	Std. Error Beta
1. Level of Living	.161	.021	56.22	.235	.031
2. Log. Rs. Jajmani Services	.921	.095	93.79	.263	.027
3. Adult Male Education Years	.187	.033	32.28	.150	.026
4. Log. Rs. Wealth	.433	.083	26.92	.156	.030
5. Inequality in Wealth	.045	.008	34.34	.131	.022
6. Inequality in Agricultural Knowledge	-.021	.007	9.20	-.068	.022
Constant	-6.621	.834	63.09		
R=.683	ANOVA	df	SS	MS	F
R ² =.467	Reg.	6	11,484.43	1,914.07	190.92
S _e =3.166	Res.	1,307	13,103.51	10.03	

effects of wealth inequality on innovation may indicate that, where resources were scarce and many peasants operated at the margins of subsistence, concentration of resources facilitated risk-taking. However, one should not overestimate the importance of these effects. While the addition of community-level wealth and knowledge inequality to individual status dimensions as predictors of innovation resulted in a significant improvement in the accuracy of prediction, the increment in R^2 was small ($R^2 = .467$ versus $.453$).

The lack of significant effects of community wealth levels on innovation suggest that innovation may not have been enhanced through disproportionate investment in wealthier communities. This would question the efficacy of government policy directed at the introduction of new technology where complementary factors of production already existed. While knowledge inequality had only very small direct effects on innovation, they were negative. The wider diffusion of information within communities might encourage innovation slightly. Since wealth inequality had positive indirect effects on innovation, in one sense the failure of redistribution programs and the concentration of new inputs actually may have resulted in greater innovation. Note, too, that these effects, while relatively weak, were independent of individual status. In villages with high inequality, all cultivators innovated more, whatever their status.

The Interaction of Community Characteristics and Individual Social Status

Given the paucity of writings directed at the specification of the interactions in question, analysis was necessarily exploratory and the reasoning speculative. There appeared to be no evidence that the relationship between properties of community stratification systems and innovation substantially suppressed the relationship between status and innovation. When wealth and knowledge inequality were controlled, the standardized partial coefficients representing the direct effects of status on innovation remained roughly the same (compare Tables 6 and 5). Similarly, cross-tabular results (Tables 2 and 3) did not suggest interactions between status and community-level measures. While I found little evidence confirming Cancian's theories for this sample, his appears to have been one of the few analyses which explicitly considered the system conditions under which the effects of status on innovation might vary. Extrapolating somewhat from his theory, we might speculate that lower inequality and higher levels of wealth at the community level would result in increasingly negative effects of status on innovation at the individual level. The simplest way to handle such interactions within an additive regression model was to add new terms to the prediction equation composed as the

multiples of individual status and community characteristics.¹⁴

After controlling for individual statuses, knowledge, inequality and wealth inequality (Table 6), none of the 15 first-order interaction terms could be added to the model. Correlations between the interaction terms and the variables which composed them ranged from .66 to .82, with most of the coefficients around .70, and serious problems of multicollinearity resulted.

Instead of judging whether the entire set of cross-product terms added significantly to explained variance (Blalock, 1972:464), forward stepwise regression was used to see if any of the interaction terms could be included. The results (Table 7) included only one of the 15 terms—the product of wealth inequality and adult male education years. It was more highly correlated with innovation net of the two consumption status indicators than were the main

effects of wealth and education status. This interaction term also was highly correlated with each of the main effects (wealth inequality $r = .742$, education $r = .744$), and it replaced these main effects in the model (compare Tables 6 and 7). The positive standardized partial regression coefficient for the interaction term ($\beta = .207$) indicated that having high education status in a village with high wealth inequality was conducive to innovation. However, accuracy of prediction (Table 7) was not greater than that obtained with only main effects (Table 6), and the coefficients for the main effects included in Table 7 differed only slightly from those in Table 6. While one might ordinarily prefer the more parsimonious equation with five predictors (Table 7) over that with six (Table 6), the inclusion of an interaction term in the former largely obviates this reasoning. Problems of multicollinearity also render the difference between the two equations difficult to interpret.

Finally, in order to explore further the possibility of interaction effects, innovation was regressed on individual status variables for the subsample of cultivators in low-inequality, high-resource communities. These conditions attempt to represent the situation in which Cancian reasoned that the inhibiting effects of status on innovation (negative slopes) would be most likely to be manifest. While average innovation was slightly higher in these communities (compare Table 8 with Appendix B), regression results again showed little support for Can-

¹⁴ First-order interactions were computed for all five dimensions of individual status with the three community characteristics after the means of each variable was set to 100. This procedure, as well as the focus on standardized coefficients, avoids some of the problems in interpreting the effects of the interaction terms (Althausen, 1971). However, problems with multicollinearity and the kinds of issues discussed by Gordon (1968) remain. The correlation matrix containing all these first-order interaction terms and the main effects could not be inverted because of problems of singularity. However, stepwise regression effectively eliminated several interaction terms which were virtually linear combinations of other variables. Rockwell (1975) provides a discussion of problems of multicollinearity and suggests several helpful "coping" strategies.

Table 7. Multiple Regression Including First Order Interactions between Individual-Status and Community-Level Variables

Variable	B	Std. Error B	F	Standardized Beta	Std. Error Beta
1. Level of Living	.164	.020	65.3	.240	.030
2. Log. Rs. Jajmani Services	.922	.095	94.2	.264	.027
3. Wealth Inequality X Adult Male Education Years	.0060	.0007	75.7	.207	.024
4. Log. Rs. Wealth	.437	.083	27.8	.158	.030
5. Knowledge Inequality	-.022	.007	10.5	-.070	.022
Constant	-63.43	6.78	87.6		
R=.683	ANOVA	df	SS	MS	F
R ² =.467	Reg.	5	11,486.5	2,297.30	229.35
S _e =3.16	Res.	1,308	13,101.4	10.02	

cian's theory. All zero-order correlations between status and innovation were positive, as were the standardized partial slopes for the variables included in the prediction equation (Table 8). The latter coefficients also were similar to those for the total sample (Table 5). However, increases in wealth appeared to produce somewhat larger increases in innovation and level of living had nonsignificant effects net of jajmani services, education and wealth.¹⁵ This reflects the magnitude of the zero-order correlations, since the only substantial difference of interest for the subsample was in the correlation of level of living with trial of innovations ($r = .370$ for $n = 292$, $r = .589$ for $n = 1,314$). In low-inequality high-resource villages, consumption mattered much less and wealth influenced innovation somewhat more.¹⁶ Even though the correlations be-

tween these different dimensions of status were not high enough to inflate standard errors seriously, this only serves to reinforce caution in interpreting differences between the coefficients for the various status variables.

As was emphasized above, this analysis of interactions has been exploratory. While there are many additional interactions between individual status and community characteristics which could be included to continue this exploration, the results discussed above and the difficulty in interpreting higher order interactions make this line of inquiry appear to hold little promise in the absence of well-reasoned theory. There were, however, some indications of possibly significant interactions between different community characteristics. I have already noted that average inequality in the distribution of knowledge about modern agricultural practices was relatively low. More importantly, for the 84 communities there was a large negative correlation between the average level of knowledge (x_1) and in-

¹⁵ The third-order partial correlation of level of living with innovation was $-.077$, while that for jati ritual rank was $-.044$. The latter result was consistent with that found for the total sample (Table 5).

¹⁶ These differences would be magnified in comparisons between cultivators in low-inequality, high-resource villages and all other cultivators. Presumably, in the latter sub-sample, the partial slope for level of living would be even higher and that for

log wealth even lower than was the case for the total sample.

Table 8. Resources and the Trial of Innovations: Regression Results for Cultivators in High-Resource Low-Inequality Communities (N=292)

Variable	B	Std. Error of B	F	Beta		
1. Log. Rs. Jajmani Services	.992	.200	24.66*	.311		
2. Adult Male Education Years	.192	.051	14.15*	.205		
3. Log. Rs. Farm Capital	.624	.183	11.66*	.221		
Constant	-4.539	1.46	9.63			
* p<.001	R=.616	R ² =.379	S _e =2.72			
ANOVA	df	SS	MS	F		
Regression	3	1,302.67	434.22	58.63		
Residual	288	2,132.96	7.41			
Zero Correlations						
	1	2	3	4	5	6
1. Jati Ritual Rank	—					
2. Log. Rs. Jajmani Services	.441	—				
3. Log. Rs. Wealth	.478	.657	—			
4. Level of Living	.373	.470	.614	—		
5. Male Adult Education Years	.322	.444	.498	.637	—	
6. Trial of Innovation	.279	.547	.527	.370	.453	—
\bar{X}	68.65	5.06	10.03	13.76	2.77	7.27
S	24.07	1.08	1.22	5.97	3.67	3.44

equality in its distribution (x_2 ; $r_{12} = -.74$). Once information entered the village, it was widely diffused. This relationship suppressed the correlation between wealth inequality (x_3) and knowledge inequality ($r_{23} = .41$), yielding a higher partial correlation between wealth and knowledge inequality controlling for the level of knowledge ($r_{23.1} = .55$). Widespread diffusion of information in densely settled nucleated villages appeared to reduce the otherwise high impact of wealth inequality on knowledge inequality. This diffusion effect also helped to specify the relationship between inequality in the distribution of knowledge and trial of innovations (x_4). While the zero-order correlation between knowledge inequality and innovation was small and negative ($r_{33} = -.06$), the first-order partial correlation controlling on community knowledge level was positive ($r_{33.1} = .26$). The small inhibiting effect of knowledge inequality on innovation (Table 6) appeared to have resulted, in part, from the relatively wide diffusion of knowledge within these villages. These and other results suggest that future analysis should attempt to expand the community characteristics considered.

Conclusions

The idea that social structure affects individual behavior has pervaded social science for some time. There appears to be only weak empirical confirmation for such effects in these exploratory results, since the independent effects of the structure of community stratification systems on innovation were modest, and there appeared to be few simple interactions of much consequence between system characteristics and individual status. Community effects may have been weak because government intervention, urban influence, greater mobility and the penetration of regional markets have become increasingly important determinants of the behavior of Indian villagers.¹⁷ In fact, it is

possible that systematic and random errors in measuring individual level variables may have understated their effects, a situation complicated by the likelihood that such errors are probably nonlinear. Knowledge and wealth inequality at the community level may have picked up some of the variance in innovation left unexplained by these measurement errors. On the other hand, the relationship between the distribution of statuses and inequality is certainly complex, and may itself be nonadditive, particularly since inequality systems are not easily distinguishable from their manifestations in the distribution of inequality and Gini indices are only aggregations of individual inequalities. The system of inequality most certainly has effects not captured by the few simple measures employed here, and those effects may have been concealed in individual-level measures of status.

Innovations are not necessarily risky just because they are new. Indeed, since innovations in agriculture and elsewhere are directed primarily at improving production, they often reduce risk. Contrary to the situation in the U.S.A. and Canada, it does not appear that economic production risks associated with Green Revolution innovations in South India are highly indivisible or call for complicated organization. Neither are such technologies created specifically for those with greater resources (Hightower, 1972). In this Indian sample, individual utilities apparently lead cultivators to make decisions regarding innovation that are consistently a positive function of status, even in communities which vary widely in the structure of their stratification systems.

Since these relationships were linear or linear for the log of status, innovative behavior at lower status levels might be af-

relationship between status and innovation. The standard deviation for land inequality (the major component of wealth) was 13.45 and the mean was 72.19 for the 84 communities. The average Gini coefficient for the distribution of land for 50 nations was reported as 71.7 with $S=13.9$ by Russett et al. (1967:239). The update on these figures (Taylor and Hudson, 1972:267-8) found a mean of 67.0 and a standard deviation of 15.2 ($n=54$). There appears to be almost as much variability across village communities in a single Indian state as there is across the nations of the world.

¹⁷ While the range of development levels and inequality encompassed by these 84 communities was surprisingly large, this too might have contributed to the lack of identifiable systematic differences in the

affected by the subsistence needs of the family. Furthermore, the magnitude of proportionate differences in the investment necessary to innovation may protect the rich, as may other institutions. Even at the relatively low levels of development represented in this sample, there was little systematic evidence to suggest that scarcity of resources dictated a trade-off between spending for consumption and the investment necessary for the trial of modern farm technology. Higher levels of wealth appear to have allowed both.

Future research also must consider the degree of uncertainty involved in innovation, since there is usually wide variation by social status in the degree to which risks are clearly calculable. While aware-

ness of agricultural innovations may be widely diffused, everything from experiential grounding through prior innovation to the distribution of credit renders these risks far less certain for poorer cultivators. As a result, awareness of innovation is probably translated into trial at different rates for different levels of resources. If future research is to represent proportionate risks more directly, the utilities associated with innovations need to be considered more closely and different types of innovations need to be sampled. Furthermore, the magnitude of feedback over time between innovation, resource accumulation and status must be explored if agrarian development policy is to find a firm base in sociological research.

APPENDIX A

Cutting Points Used in the Categorization of Resources

Variable	Low	Low Middle	High Middle	High
1. Jati Rank	10-59	60-79	80	81-99
2. Rs. Jajmani Services	0-49	50-199	200-799	800-20,705
3. Rs. Capital	0-999	1,000-9,999	10,000-29,999	30,000-669,555
4. Level of Living	1-6	7-12	13-18	19-31
5. Adult Male Education Years	0-1.49	1.5-3.99	4-8.99	9-20.5

APPENDIX B

Status, Inequality and Innovation: Correlations
(N=1,314)

	1	2	3	4	5	6	7	8	9	10	11
1. Jati Ritual											
2. Rs. Jajmani Rank Services	.122										
3. Log. Rs. Jajmani Services	.443	.545									
4. Rs. Farm Capital	.254	.365	.450								
5. Log. Rs. Farm Capital	.448	.277	.633	.649							
6. Level of Living	.409	.302	.578	.575	.665						
7. Adult Male Education Years	.313	.240	.430	.483	.475	.626					
8. Knowledge Inequality	-.041	-.019	-.067	-.046	-.143	-.052	.014				
9. Inequality in Wealth	-.026	.040	.005	.110	-.079	.009	.104	.411			

APPENDIX B

Status, Inequality and Innovation: Correlations
(N=1,314)

	1	2	3	4	5	6	7	8	9	10	11
10. Level of Village Wealth	.139	.019	.135	.361	.363	.360	.277	-.289	.013		
11. Trial of Innovation	.328	.374	.550	.501	.568	.589	.497	.064	.110	.236	—
\bar{X}	65.68	316.8	4.88	33,026	9.45	12.54	2.69	36.35	69.07	22,107	6.85
S	26.22	923.8	1.24	57,756	1.56	6.32	3.46	14.12	12.54	14,213	4.33

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ARMS RACES: A TEST OF TWO MODELS*

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This is an investigation of two models of arms races: Lewis F. Richardson's linear, additive, symmetric model and a curvilinear, asymmetric alternative based on the same interaction paradigm but with empirical equations from psychophysical experiments on perception as premises. Two hypotheses derived from each model are tested using data from seven arms races. In general, the data give stronger support to the curvilinear, asymmetric model which predicts the follower in the arms race will increase its armaments through time as an exponential function of the leader's armaments, and the leader will increase its armaments as a power function of the follower's efforts at the previous time period. The conclusions are based on the relative fit of the models to the seven sets of data, parsimony, reasonableness of the parameter estimates as adjudged by their signs and magnitudes, and predictions about the outcomes of the races—war or armed truce.

Lewis F. Richardson's (1939; 1960) analysis of arms races has become a classic in mathematical conflict theory and research. Although more or less ignored at first, his contributions have been popularized by Rapoport (1957; 1960), Boulding (1962) and Rashevsky (1959). Others have continued his tradition, suggesting interesting modifications (cf. Smoker, 1965; Abelson, 1963; Saaty, 1968). His work stands out, in our opinion, for its mathematical sophistication and for the concern shown for empirical evidence.

The purposes here are (1) to review Richardson's model, (2) to derive implications which may be used in its empirical evaluation, (3) to develop a modified interaction model using empirical generalizations from modern psychophysical research about perception as premises, (4) to derive the implications and (5) to evaluate the relative adequacy of the two models using data from seven arms races.

* The first author was responsible for working out the second model of arms races presented here. All authors wish to thank Paul Fife and Robert O'Malley, Professors of Mathematics, and Ronald Schoenberg, Assistant Professor of Sociology, for their suggestions for solving mathematical and computational problems encountered in this research. This research was supported by an NSF grant, SOC 75-08448.

RICHARDSON'S MODEL

Richardson (1960:14) started his exposition with a "quote" from a speech of the Defense Minister of "Jedesland":

The intentions of our country are entirely pacific. We have given ample evidence of this by the treaties which we have recently concluded with our neighbors. Yet, when we consider the state of unrest in the world at large and the menaces by which we are surrounded, we should be failing in our duty as a government if we did not take adequate steps to increase the defense of our beloved land.

Richardson suggested that a simple mathematical representation of the above rationale for increasing armaments is:

$$dy/dt=kx, \quad (1)$$

where dy is the increment in armaments by nation Y, dt is the increment in time, k is a positive constant named a defense or *insecurity* coefficient, and x represents the cumulative armaments of nation X, the menaces of which nation Y is "surrounded." A similar equation may be written for nation X:

$$dx/dt=ly, \quad (2)$$

where l is a positive insecurity coefficient and y , the accumulative armaments of nation Y.

However, this formulation may be criticized because it leaves out terms for the fatigue or expense of keeping up defenses. So Richardson (1960:15) "improved" the equations:

$$dy/dt = kx - ay, \quad (3)$$

$$dx/dt = ly - \beta x, \quad (4)$$

where α and β are fatigue coefficients and the other variables and constants are defined as before.

Richardson's (1960:16) final differential equations include g and h as constants representing Y and X 's respective ambitions or grievances toward the other:

$$dy/dt = kx - ay + g \quad (5)$$

$$dx/dt = ly - \beta x + h. \quad (6)$$

This system of equations can be conveniently expressed in matrix notation:

$$\frac{d}{dt} \begin{bmatrix} y \\ x \end{bmatrix} = \begin{bmatrix} -\alpha & k \\ l & -\beta \end{bmatrix} \begin{bmatrix} y \\ x \end{bmatrix} + \begin{bmatrix} g \\ h \end{bmatrix}, \quad (7)$$

$$\frac{d}{dt} Z = BZ + A. \quad (8)$$

In this form, it is possible to solve for cumulative armaments at time t , $Z(t)' = [y \ x]'$, as a function of cumulative armaments at time $t-1$, $Z(t-1)$, using the theory of linear nonhomogeneous differential equations (Boyce and DiPrima, 1969:317-21; Kaufman, 1976) if $\alpha\beta - kl \neq 0$:

$$Z(t) = e^B Z(t-1) + (e^B - I) B^{-1} A, \quad (9)$$

$$= B^* Z(t-1) + A^*, \quad (10)$$

where $e^B = I + B + B^2/2! + B^3/3! + \dots$.¹ The component equations are:

¹ According to Kaufman (1976), Coleman (1968:449-51) and Land (1970:271-2) erroneously state that $A^* = B^{-1}A$. Kaufman (1976) presents a solution for a more general system that includes exogenous variables. Richardson (1960:37-41) presents solutions derived from several simplifying assumptions. The solution in (9)-(10) requires none of Richardson's simplifying assumptions and is, therefore, more appropriate for the empirical analysis here. Although e^B is defined by the infinite series $I + B + B^2/2! + B^3/3! + \dots$, a formula that is computationally simpler for the 2×2 case is:

$$e^B = \frac{e^{\lambda_1}}{\lambda_1 - \lambda_2} (B - \lambda_2 I) + \frac{e^{\lambda_2}}{\lambda_2 - \lambda_1} (B - \lambda_1 I),$$

if $\lambda_1 \neq \lambda_2$ where λ_1 and λ_2 are the eigenvalues of B .

$$y(t) = b^*_{11}y(t-1) + b^*_{12}x(t-1) + a^*_1, \quad (11)$$

$$x(t) = b^*_{21}y(t-1) + b^*_{22}x(t-1) + a^*_2. \quad (12)$$

Equation (10)—or equivalently, equations (11)-(12)—can be used to estimate empirically the parameters of Richardson's model. The fit of the estimated model to the data and the pattern of the parameter estimates can then be used to compare Richardson's model with alternatives. (If $\alpha\beta - kl = 0$, then B^{-1} does not exist and A^* is ill-defined.)

AN ALTERNATIVE MODEL

One reason Richardson's model has appealed to so many is that the parameters in the differential equations are interpretable as measures of insecurity, fatigue and grievance. Also, the estimates of the parameters may be used to predict whether an arms race is stable and thus will result in armed truce or is unstable and thus will result in war (cf. Richardson, 1960:22-3; Blalock, 1969:106-9; Coleman, 1968:448-9). These may be very desirable features, but they are irrelevant if Richardson's model is mis-specified. That is, Richardson's linear, additive differential equations—particularly in their representations of the "insecurity" and "fatigue" parameters—may not correctly describe the actual interaction process. Richardson's initial verbal description seems to suggest that the insecurities are intervening variables, with the causal sequence diagrammed as follows: X 's insecurity \longrightarrow X 's armaments \longrightarrow Y 's insecurity \longrightarrow Y 's armaments \longrightarrow X 's insecurity, etc. This sequence implies that the form of the equations ought to be congruent with equations for perception, specifically those which have emerged from psychophysical experiments. In fact, Richardson (1960:14, 95-7, 105-6) considered modifying his model by incorporating Fechner's psychophysical equation. However, Fechner's logarithmic equation was then under attack, in part by Richardson himself who did several psychophysical experiments (cf. Richardson, 1928; 1929; Richardson and Ross, 1930) which anticipated the more definitive work

of S. S. Stevens (cf. Stevens, 1957; 1961; 1962; 1971; 1972; 1975).

Stevens' equation is one of the generalizations used in the modification proposed here. His experimental results imply an underlying curvilinear model where the relative change in a psychological sensation ($d\psi/\psi$) is a constant proportion (n) of the relative change in the related stimulus ($d\phi/\phi$) or:

$$d\psi/\psi = n d\phi/\phi. \quad (13)$$

Stevens' power function, the solution of this differential equation, has been shown to describe quite accurately the behavior of a wide range of perceptual attitudinal interactions (cf. Stevens, 1966; 1972; Hamblin, 1966; 1971; 1974) including the negative sensations, not unlike insecurity, that accumulate through time in "low stakes" conflict (Hamblin, Bridger, Day and Yancey, 1963).

Like Richardson's original, the model of arms races proposed below assumes a vicious cycle of interaction wherein the armament increments by each party increase the insecurity of the other, leading both to further increments in armaments and, in turn, to further increases in insecurity. However, it differs from the original model in at least four ways. (1) As mentioned, the forms of the differential equations are congruent with appropriate generalizations for psychophysical experiments. (2) The process is assumed to be asymmetric. All of the arms races indicate a leader who is always in front, evidently taking the initiative, and a follower who is always behind, evidently under duress, trying desperately to catch up (see the data in Appendix A). The duress results in a form of interaction for the follower which is different from that of the leader. (3) The leader's equation is lagged because the asymmetric interaction is assumed to occur in sequence, not simultaneously. (4) The fatigue or expenditure variable is eliminated from the equations primarily because it may not be relevant to the shape of an arms race, only to the outbreak of hostilities (when the leader's advantage is optimal) or its termination (when a sufficient deterrence or overkill capacity is achieved).

The Follower's Response Pattern

Assume that Y suddenly finds itself substantially behind X in armaments and, because of the competitive, hostile relations between the two, its decision makers are highly threatened by their relative weakness. Suppose further that under such conditions of high threat or duress, sensory adaptation occurs. Consequently, the accumulated insecurity not only mediates Y's armament effort but modifies it from round to round, not unlike the adaptation of the eye or the ear to the cumulative effects of light or noise in the recent past. Psychophysical experiments on adaptation by Ekman (1970) have yielded data described not by Stevens' power function but by an exponential equation. Assuming adaptation because of duress and, hence, the applicability of Ekman's findings, the increment in the insecurity of Y's decision makers (dI) should increase as a constant proportion (c) of their cumulative insecurity (I), times the increment (dx) in armament effort of X, as in the following differential equation:

$$dI = c I dx \text{ or } dI/I = c dx. \quad (14)$$

If the response to the cumulative insecurity follows Stevens' psychophysical equation as implied by experimental results on other sensory phenomena (see Hamblin, Clairmont and Chadwick, 1975), then the relative increase in Y's armaments (dy/y) at time t should be a constant proportion (p) of the relative increase of Y's decision maker's insecurity (dI/I) at time t , as in the following differential equation:

$$dy/y = p dI/I = p c dx = m dx \text{ or } dy/dx = m y \quad (15)$$

where $m = p c$. If (15) is integrated to solve for $y(t)$ as a function of $x(t)$ one obtains:

$$y(t) = be^{mx(t)}, \quad (16)$$

where b is partly a scaling constant and partly a grievance parameter. This is the first of two alternative hypotheses which can be empirically compared to Richardson's linear model using arms race data.

The Leader's Response Pattern

Because X started the arms race, has a superior arms stockpile and always takes

the *initiative*, the insecurity of its decision makers has to be determined by what has happened previously. Also, because X is ahead in the race, its decision makers are assumed not to act under duress and, therefore, not to experience adaptation. Therefore, they behave like subjects in Stevens' experiments, where adaptation is absent or controlled, and like actors in "low stakes" conflict (Hamblin et al., 1963). Thus, the relative increases in insecurity (dI/I) should be a constant proportion (k) of the relative increase in Y's armaments (dy/y) with some time lag:

$$dI/I = k L(dy/y) \quad (17)$$

where L is a lag operator. If, as before (equation [15]), it is assumed that the relative increase in X's armaments (dx/x) will be a constant proportion (h) of the relative increase in X's insecurity (dI/I) then

$$\begin{aligned} dx/x &= h dI/I = h k L(dy/y) \\ &= n L(dy/y). \end{aligned} \quad (18)$$

If (18) is integrated to solve for x , one obtains:

$$x = s[L(y)]^n, \quad (19)$$

where s is partly a scaling constant and partly an aggression parameter. Because the data are in discrete years, we take a lag of one year, i.e., $L(y) = y(t-1)$, so that equation (19) now can be stated as:

$$x(t) = s y(t-1)^n. \quad (20)$$

Equation (20) is the second substantive hypothesis.²

² It might be noted that if equation (16) is true, it implies $x(t) = k + 1/[m \log y(t)]$. This log function is not proposed as a causal hypothesis for the leader's interaction because it ignores the asymmetry in leader-follower interaction which is crucial to this alternative model and it does not follow from an appropriate psychophysical generalization. One might be tempted to use Fechner's log function as a premise to derive this hypothesis, but Fechner's equation has been shown to describe the relationship between the magnitude of perceptual error and stimulus magnitude for *some*, not all modalities (cf. Eisler, 1963; Shinn, 1974; Stevens, 1962). It does not describe the relationship between perceptual magnitude and stimulus magnitude, the kind of generalization required by this model of arms races.

Lagging the leader's interaction might suggest a difference equation model of the process. How-

THE DATA

The seven data sets used here involve different arms races—pre-World War I, pre-World War II, the cold war between the U.S.S.R. and the United States—and also different through-time measures of armaments—expenditures, warlike work time and weapons counts.

Expenditures by the Entente and Alliance

The data set which Richardson (1960: 87-97) used as a preliminary test of his theory involved armament expenditures by the Entente (France, Russia and Great Britain) and the Alliance (Germany, Austria-Hungary and Italy) from 1907 through 1914. Richardson collected the data himself and converted the expenditures in various national currencies into pounds sterling. He also made adjustments so the budget years all corresponded to one another.

Warlike Work Time: The Axis versus the Allies

Richardson (1960:114-20) also collected expenditures data on the arms race prior to World War II. He ran into trouble, however, because this period spanned the Depression, and the currencies of some of the nations fluctuated wildly in value. Also France, Germany, and perhaps some other nations hid their armament expenditures in budgets of several ministries, made false statements about armament expenditures or simply refused to give information.

In an effort to circumvent these difficulties, Richardson (1960:136-44) developed a measure of warlike work time for four Axis nations (Germany, U.S.S.R., Japan and Italy) and six Allied nations (France, China, Britain, United States, Poland and Czechoslovakia) for the 1927-1939 period. The measure is not without defects, but economist Kenneth Boulding (Richardson, 1960: vii) characterized

ever, our model is based on perceptual equations and perception is a continuous, not a discrete process. Also, intelligence information is accumulating continuously and decision makers, (like the National Security Council and the Joint Chiefs of Staff) meet weekly and make decisions, not in discrete yearly intervals but aperiodically, if not continuously.

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Richardson's efforts as bringing "order into a field where disorder is usually regarded as supreme." These data indicate that the arms race began in 1934 (before that the data fluctuate as in an equilibrium). We excluded the U.S.S.R., because of its difficulty in deciding whether it was Axis or Ally, and China, because of its internal wars during that period. Work time on actual wars by Italy (in Abyssinia) and Japan (in China) also was excluded.

Nuclear Explosions and Weapons Counts: U.S. and U.S.S.R.

Data are available on several arms races between the United States and the U.S.S.R. since World War II (Stockholm Peace Research Institute, 1973; 1974; Institute of Strategic Studies, 1970; 1974). During that period, several important weapons were developed and the two nations raced to gain superiority in each. Such weapons were nuclear warheads, ballistic missiles (intercontinental and submarine-launched) and photographic and electronic satellites. However, their selection here was not arbitrary; each is a key component of the essential cold war armament effort and together they are the most deadly weapons system ever devised.

In the case of nuclear explosions, the gauge of armaments is not a weapons count but a measure of accumulated warlike work on the development of the most destructive weapon ever, the nuclear bomb. The data on nuclear explosions involve two races, one from 1949 through 1958, before the 1959-1960 nuclear test moratorium, and one afterward, from 1961-1973. These data seem to be accurate because they are based on seismographic readings or, in the case of some of the early explosions, radioactive fallout. In spite of that, the exact years of some 53 explosions were not given. In those instances, we added them to the last of the possible years.

Ballistic missiles were developed as the prime vehicle for delivering nuclear warheads. They are launched either from silos (ICBMs) or from submarines (SLBMs). The count is a total of both types during the period of mutual increase in inventories.

To some, photographic and electronic reconnaissance satellites may not seem like much of a weapon, but they are the eyes which accurately pinpoint potential nuclear targets in the other country, as well as monitor the other's warlike and industrial activity. Thus, they are extremely important weapons when used in conjunction with ballistic missiles and nuclear warheads.

Richardson's model was designed to describe arms races. Therefore, it follows that the best or most direct measures to use in evaluating his model are counts indicating the quantity and quality of the weapons. His use of expenditures and work time were, at best, indirect indexes of numbers and quality. The total number of ballistic missiles is a measure of the quantity of one very crucial cold war weapon and as a measure it can increase as it has for the U.S.S.R. (at least until 1973) or remain constant (as it has for the U.S. for at least seven years), or it can decrease (missiles can be destroyed by their owners). The cumulative numbers of photographic and electronic reconnaissance satellites were used, like nuclear explosions, not as weapons counts but as indexes of quality—i.e., level of weapons development.

ANALYSIS

Equation (10), the solution to Richardson's model, takes the form of a linear regression equation. The coefficient matrices in (10), A^* and B^* , were estimated using ordinary least squares (OLS).³ Because A^*

³ Applying OLS entails the usual assumptions (Blalock, 1972:429-33; Johnston, 1972:121-2). The cross-lagged model employed here may have a more complex error structure than is assumed by OLS. While the deterministic equations specified by Richardson do not allow for any error, we have assumed an additive error term in the estimating equations to allow for random (non-accumulating) measurement errors. This assumption is particularly vulnerable to problems of autocorrelation. Durbin-Watson tests (Johnston, 1972:251-3) failed to reject the null hypothesis of no autocorrelation in all but the electronic satellites race. Generalized least squares estimates that adjust for autocorrelation (Johnston, 1972:259-65) were not computed because they would consume an additional degree of freedom, too

and B^* are functions of the structural parameters of the differential equations (A and B), the method proposed by Singer and Spilerman (1976) was used to obtain estimates.⁴ From equations (9) and (10):

$$B^* = e^B \text{ or } B = \ln(B^*) \quad (21)$$

However, equation (21) yields an estimate of B from an empirically estimated B^* only under specific necessary conditions (Singer and Spilerman, 1976:10-4) which were satisfied except for the ballistic missiles race. For the six races that satisfied the necessary conditions, B was estimated using Sylvester's formula (Singer and Spilerman, 1976:18):

$$\hat{B} = \ln(\hat{B}^*) = \frac{\ln(\hat{\lambda}_1)}{\hat{\lambda}_1 - \hat{\lambda}_2} (\hat{B}^* - \hat{\lambda}_2 I) + \frac{\ln(\hat{\lambda}_2)}{\hat{\lambda}_2 - \hat{\lambda}_1} (\hat{B}^* - \hat{\lambda}_1 I), \quad (22)$$

if $\hat{\lambda}_1 \neq \hat{\lambda}_2$, where the $\hat{\lambda}$'s are eigenvalues of \hat{B}^* . Given that estimate of \hat{B} , \hat{A} was obtained from the definition of A^* :

$$A^* = (B^* - I)B^{-1}A \quad (23)$$

$$\text{thus, } \hat{A} = \hat{B}(\hat{B}^* - I)^{-1}\hat{A}^*. \quad (24)$$

This method for obtaining an estimate of A assumes that \hat{B}^{-1} and $(\hat{B}^* - I)^{-1}$ exist—i.e., $a\beta - kl \neq 0$. An estimate of A was found for every race in which an estimate of B was found—i.e., for all but the ballistic missiles race. Since the ballistic missiles race lacked empirical solutions to the differential equations, a difference equation approach, which implies

$$\hat{B} = \hat{B}^* - I, \quad (25)$$

$$\hat{A} = \hat{A}^*, \quad (26)$$

where \hat{B}^* and \hat{A}^* are estimated using equation (10), was used to obtain the estimates presented.⁵

As mentioned, part of the appeal of Richardson's theory has been its implications for the stability of arms races (e.g., Richardson, 1960:22-3; Blalock, 1969:106-9; Coleman, 1968:448-9). According to the model, stable arms races approach an equilibrium point defined as the point at which neither country's arms stores are changing:

$$\frac{dy}{dt} = \frac{dx}{dt} = 0. \quad (27)$$

Furthermore, once the equilibrium is reached, the system adapts to random shocks that move one country or the other off its equilibrium point and returns the arms race to equilibrium levels. Unstable arms races, on the other hand, escalate indefinitely according to Richardson's equations; the result being war or bankruptcy (Richardson, 1960:61).

Stability is a function of the relative sizes of the "insecurity" and "fatigue" parameters. In general, a system of linear differential equations will be stable if the eigenvalues of B have negative real parts (Boyce and DiPrima, 1969:390). In the 2 x 2 case being considered here, a race is stable if (Blalock, 1969:106-9; Coleman, 1968:448-9):

$$a\beta - kl > 0, \quad (28)$$

$$a + \beta > 0. \quad (29)$$

high a price to pay to correct a problem affecting less than two percent of the total variance in a relatively short series (seven years). Another potential problem is simultaneity bias which leads to under-identification, but Blalock (1969:83-4) shows that simultaneity bias in cross-lagged models is zero when autocorrelation of the errors is insignificant.

⁴ Although the discussion by Singer and Spilerman (1976) is limited to continuous-time Markov processes, their derivations apply to any matrix equation of the form:

$$B^* = e^B.$$

⁵ Derivation of the difference equation solution and parameter estimates obtained from that solution for all arms races are available from the authors upon request. In general, parameter estimates obtained using the difference equation approach were not equal to those obtained using the differential equation approach, but the signs of the parameter estimates were the same in all but one case. Furthermore, since both solutions are based on equation (10), R^2 values are identical. Thus, the conclusions reached in this paper would not change if a difference equation approach were substituted for the differential equation approach developed by Richardson and employed here.

The values of x and y at equilibrium can be obtained by setting equations (5) and (6) to zero and substituting terms if the conditions in equations (2*) and (29) are satisfied:

$$y = (g/a) + (k/a)x \quad (30)$$

$$x = (h/\beta) + (l/\beta)y \quad (31)$$

$$y^* = (\beta g + kh) / (a\beta - kl) \quad (32)$$

$$x^* = (lg + ah) / (a\beta - kl) \quad (33)$$

if $a \neq 0$; $\beta \neq 0$; $a\beta - kl \neq 0$, where y^* and x^* are the predicted equilibrium values of y and x , respectively. Equations (28) through (33) allow a further test of Richardson's model since we can determine whether or not unstable races resulted in wars, and we can compare the predicted equilibrium values with the observed stopping points for stable races.

Analyses for the exponential and power functions predicted by the second theory were done using a nonlinear regression program based on the Fletcher-Powell (1963) optimization technique. Both equations are just-identified because they can be log transformed so they are linear in the parameters (Christ, 1966:331-4).

RESULTS AND DISCUSSION

The relative adequacy of the two models will be gauged using four criteria: (1) the overall fit of the derived equations to the arms race data, (2) parsimony, (3) the reasonableness of the parameter estimates and (4) the predictions about war and armed truce. Other things being equal, relative superiority on any one of these cri-

teria is sufficient grounds for choosing between the models.

Comparison of Fit

The R^2 values gauging the fit of the alternative models to the seven sets of arms race data are given in Table 1. Since the linear equations have one more independent variable than the power and exponential equations, the R^2 values were adjusted for degrees of freedom (Johnston, 1972: 130). In general, the fit of all of these models to the data is quite good: only three R^2 values are less than .90. However, Richardson's linear, additive equation models the leader's interaction slightly better on the average than the power function; the *mean* R^2 is .977 versus .974. In contrast, the exponential equation models the follower's interaction somewhat better on the average than does Richardson's linear equation: the *mean* R^2 is .986 versus .957. Thus, neither Richardson's linear, symmetric model nor the alternative curvilinear, asymmetric model is preferred on the basis of fit alone.

Comparison of Parsimony

Richardson's linear, additive model has six parameters (three in each equation), and the asymmetric, curvilinear model has only four (two in each equation). Since the fit of the two models is approximately the same, relative parsimony could be important in making a choice. However, we must consider two more criteria before making a decision—the reasonableness of

Table 1. R^2 Values for Linear and Curvilinear Equations for Seven Arms Race Data Sets *

Measure	Period	Side		Linear		Power	Exponential
		Leader	Follower	$dx/dt =$ $ly - \beta x + h$	$dy/dt =$ $kx - \alpha y + g$	$x(t) =$ $sy(t-1)^*$	$y(t) =$ $be^{mx(t)}$
Expenditures	1907-14	Entente	Alliance	.993	.950	.991	.994
Man-Years	1934-38	Axis	Allies	.998	.964	.952	.997
Nuclear Explosions	1949-58	U.S.A.	U.S.S.R.	.984	.958	.982	.988
Nuclear Explosions	1961-73	U.S.A.	U.S.S.R.	.985	.947	.950	.952
Ballistic Missiles	1960-67	U.S.A.	U.S.S.R.	.886	.904	.953	.977
Photographic Satellites	1962-73	U.S.A.	U.S.S.R.	.997	.999	.992	.999
Electronic Satellites	1967-73	U.S.A.	U.S.S.R.	.997	.979	.996	.997

* R^2 values for the linear equations were adjusted for degrees of freedom; parameter estimates are those for the differential equations, but the data were fit using their respective solutions, equations (11) and (12). See analysis section for method of calculating parameter estimates.

the parameters and the predictions about war and armed truce.

Comparison of Parameter Estimates

The linear model. The parameter estimates for the linear model are given in Table 2. Although Richardson discussed conditions under which the insecurity parameters k and l (1960:12) or the grievance parameters g and h (1960:16) may be negative, he clearly expected positive values for all parameters under the antagonistic circumstances of the pre-World War I, pre-World War II and, presumably, the cold war arms races analyzed in Table 2 (Richardson, 1960:13-37, 61-9). Thus, the negative parameters in Table 2 are problematic for Richardson's model. There is at least one problematic coefficient for six of the seven arms races.

Many of the negative coefficients are estimates of g and h , the grievance parameters. Richardson (1960:16) observed that a negative grievance parameter can arise for one side in an arms race when at the onset "the prevailing mood of that side is contentment." Taken at face value, the findings indicate that the Entente and the Alliance both began their pre-World War I race with contentment coefficients (-6.9 versus -41.9) and that the Axis and the Allies began their pre-World War II arms race with grievance coefficients (19.8 versus 4.9). Both the U.S. and the U.S.S.R. started the first nuclear explosions race

with large coefficients of contentment toward each other (-13.0 versus -15.4), but started the second nuclear explosions race (*a year or so before the atmospheric test ban treaty was negotiated*) with large grievance coefficients (106.8 versus 18.7). The U.S. and U.S.S.R. photographic satellites race began with a grievance coefficient (1135.0) for the U.S. and a contentment coefficient (-315.5) for the U.S.S.R. Similarly the electronic satellite race began with a 20.0 grievance coefficient for the U.S. and a -41.3 contentment coefficient for the U.S.S.R. The difference-equation solutions for the ballistic missiles race give both the U.S. and U.S.S.R. grievance coefficients (273.6 versus 46.4). This is an unrealistic pattern of mixed results that makes little if any sense, given our historical knowledge of conditions surrounding the beginnings of these races.

Just as surprising are the non-positive estimates of k and l , the insecurity parameters, which appear in all but the expenditures (1909-1914) and ballistic missiles (1961-1967) races. Negative insecurity coefficients indicate that the party in question is responding to increases in the other's armaments with security, not insecurity, and thus it decreases rather than increases its armaments each round as a result of the other's armament increases. The results indicate that the Axis (-8.31) in the pre-World War II arms race, the U.S.S.R. (-6.81) in the first nuclear explosions race,

Table 2. Parameter Estimates for Linear Differential Equations of Richardson's Model for Seven Arms Race Data Sets

Measure	Side		Follower (y)			Leader (x)		
	Follower	Leader	a	k	g	β	l	h
Differential Equations								
Expenditures	Alliance	Entente	.440	.534	-41.898	.558	1.142	-6.864
Man-Years	Allies	Axis	2.230	.416	4.921	-1.556	-8.311	19.819
Nuclear Explosions ^a	U.S.S.R.	U.S.A.	-8.966	-6.813	-15.393	6.351	9.177	-13.044
Nuclear Explosions	U.S.S.R.	U.S.A.	-.074	.045	18.686	.170	-.004	106.846
Ballistic Missiles ^b	U.S.S.R.	U.S.A.	.598	.208	46.427	.594	1.970	273.590
Photographic Satellites ^a	U.S.S.R.	U.S.A.	2.149	13.153	315.543	-2.138	-5.378	1134.965
Electronic Satellites	U.S.S.R.	U.S.A.	.358	1.138	-41.343	.285	.000	20.034

^a The eigenvalues of the estimating matrices for the arms races are complex conjugates.

^b The determinant of the estimating matrix is less than zero, therefore no solution to the differential equation exists. Solution to difference equation formulation is substituted (see footnote 5).

both the U.S. (-0.004) and the U.S.S.R. (-0.45) in the second nuclear explosions race, and the U.S. (-5.38) in the photo satellite race had "security" rather than "insecurity" coefficients—i.e., were decreasing their armaments as a function of their racing partner's increase in armaments. The insecurity coefficient of the U.S. in the electronic satellites race was zero, which implies the U.S. simply was not responding during that race to accumulative increases in the U.S.S.R.'s electronic satellites.

Estimates of α and β , the "fatigue" parameters, are also negative in some instances. Richardson (1960) did not mention this possibility, but negative values of α and β imply that spending money for arms does not result in a side's becoming fatigued, with its expenditures slowing down the further accumulation of armaments—but that in each round the expenditures create economic energy which increases the rate of arms accumulation. The data imply this mysterious process occurred with the Axis (-1.56) in the pre-World War II race, with the U.S.S.R. in the first (-8.97) and second (-0.07) nuclear explosion races, and with the U.S. (-2.14) in the photographic satellites race. Keynesian economic theory might predict such "pump priming" results for market economies if not planned economies like the U.S.S.R. However, this is not an explanation, because the postulated effects occurred in some races but not others. Why did the expenditures for armaments result not in energy, but fatigue—a slowing down of expenditures—for the Entente (0.56) and the Alliance (0.44) in the pre-World War I race, the Allies (2.23) in the pre-World War II race, the U.S. in the first (6.35) and second (0.17) nuclear explosion races, the ballistic missiles race (0.59) and the electronic satellites race (0.29) and the U.S.S.R. in the ballistic missiles race (0.60) and the satellite races (2.15 and 0.36)?

Historically and substantively, this pattern of mixed—some positive and some non-positive—grievance, insecurity and fatigue coefficients make little if any sense. Rather the unreasonable mixture is an indication that Richardson's model may be misspecified, that he inadvertently imposed

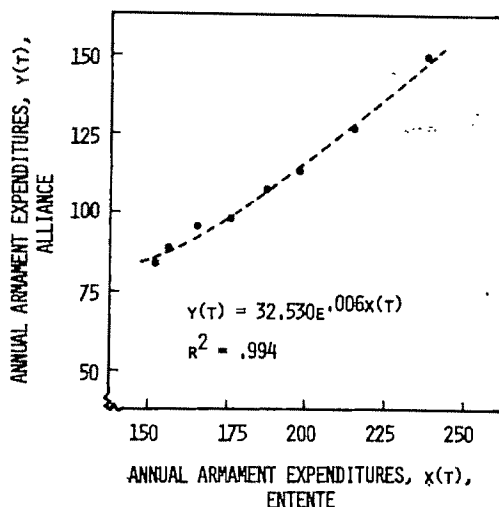


Figure 1. Annual armament expenditures for Entente and Alliance nations 1907-1914 in millions of pounds sterling. The dashed line represents the exponential function given in the figure. (Data from Richardson, 1960.)

a linear model on data generated by a curvilinear process (see Figures 1-5). The question is, does the asymmetric, curvilinear model behave more consistently?

The curvilinear model. In evaluating this model, several criteria are used. First, to

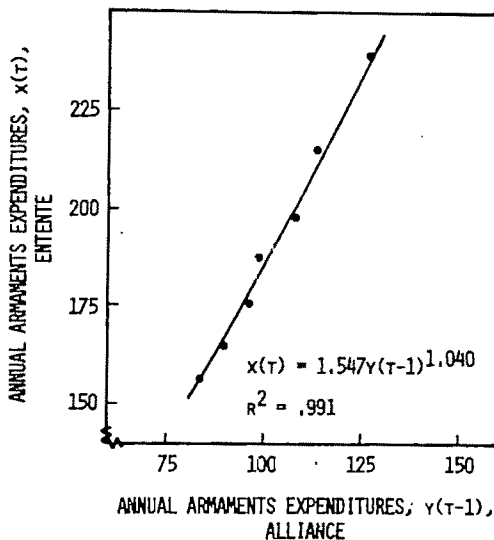


Figure 2. Annual armaments expenditures for Alliance and Entente nations 1908-1914 in millions of pounds sterling. The line represents the non-linear least squares power function given in the figure. (Data from Richardson, 1960.)

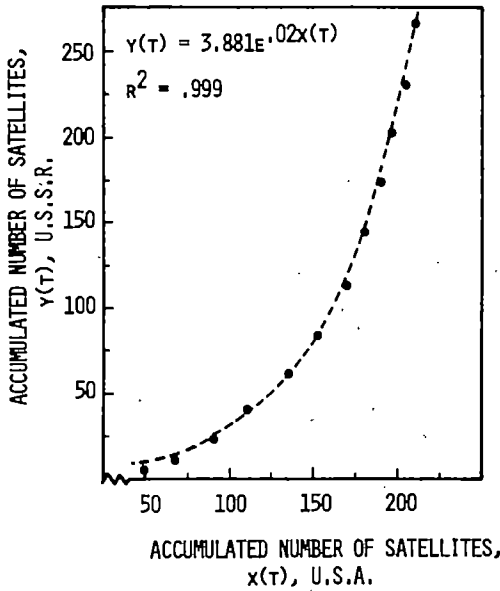


Figure 3. Accumulated photographic satellites launched by the U.S. and the U.S.S.R. 1962-1973. The dashed line represents the above exponential equation fitted using non-linear least squares regression. (Data from the Stockholm International Peace Research Institute, 1973; 1974.)

be reasonable, the estimated parameters should be positive. Second, the scaling constants for both predicted equations should not approach zero, since that indicates no

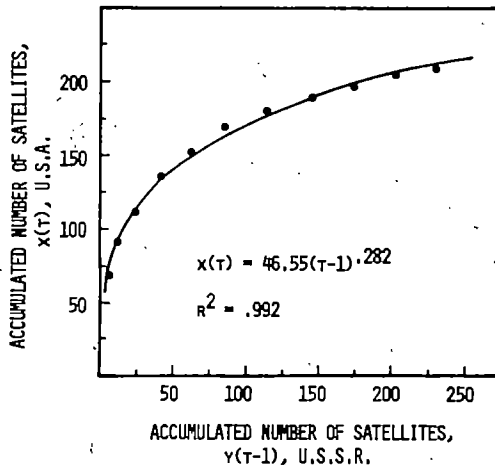


Figure 4. Accumulated number of photographic satellites launched by the U.S. and U.S.S.R. 1963-1973. The line represents the non-linear least squares function given in the figure. (Data from the Stockholm International Peace Research Institute, 1973; 1974.)

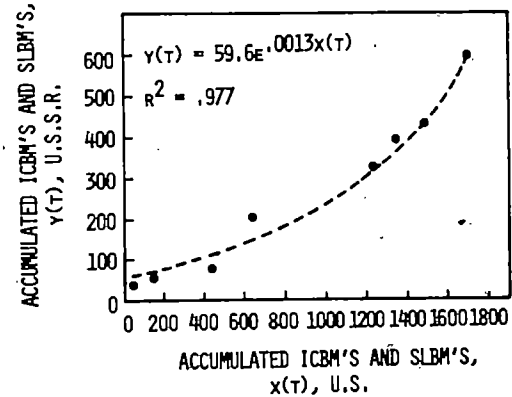


Figure 5. Accumulated Intercontinental Ballistic Missiles and Submarine Launched Ballistic Missiles inventoried by the United States and the Soviet Union from 1960 through 1967. The dashed line represents the above exponential equation, fitted using non-linear least squares regression. (Data from the Institute for Strategic Studies 1970; 1974.)

relationship. Also, the asymmetrical predictions are based on the assumption that different experimental generalizations—i.e., psychophysical equations about perception and, hence, reactions to stimuli—describe the leaders' and followers' respective interactions in arms races. Consequently, for the test of the model's asymmetry to be positive, the predicted exponential equation should describe the followers' interactions better than the power function, and the predicted power function should describe the leaders' lagged interaction better than the exponential.

First, all of the parameter estimates in the equations predicted for both the followers and the leaders in the top and bottom of Table 3 are positive for all seven races. The curvilinear model passes this test.

Second, all of the compound rates in the top of Table 3 for the exponential equations predicted for the followers are somewhat larger than zero. Some might question the 0.00134 compound rate estimated for the ballistic missiles race (see Figure 5). While there is some noise, it is typically exponential with as much curvature as is found in the plot of the photographic satellite race, Figure 3. This is because the process is compounded 1,710

Table 3. Non-linear Least Squares Parameter Estimates and r^2 Values of the Power and Exponential Equations for the Followers' and Leaders' Data Sets

Measures: For Followers' Data Sets	Period	Side		Power, $y(t) = sx(t)^n$			Exponential, $y(t) = be^{mx(t)}$		
		Follower	Leader	s	n	r^2	b	$m \cdot 100$	r^2
Expenditures	1907-14	Alliance	Entente	.165	1.240	.987	32.53	.639	.994
Man-Years	1934-38	Allies	Axis	1.396	.452	.955	2.604	3.743	.997
Nuclear Explosions	1949-58	U.S.S.R.	U.S.A.	.139E-2	2.138	.999	3.260	1.870	.988
Nuclear Explosions	1961-73	U.S.S.R.	U.S.A.	1.197	.854	.907	80.11	.219	.952
Ballistic Missiles	1960-67	U.S.S.R.	U.S.A.	.031	1.315	.969	59.58	.134	.977
Photographic Satellites	1962-73	U.S.S.R.	U.S.A.	.273E-5	3.431	.993	3.881	2.004	.999
Electronic Satellites	1967-73	U.S.S.R.	U.S.A.	.223E-8	5.779	.998	.106	9.945	.997
Measures: For Leaders' Data Sets									
Measures: For Leaders' Data Sets	Period	Side		Power, $x(t) = sy(t-l)^n$			Exponential, $x(t) = be^{my(t-l)}$		
		Follower	Leader	s	n	r^2	b	$m \cdot 100$	r^2
Expenditures	1908-14	Alliance	Entente	1.547	1.040	.991	69.07	.983	.989
Man-Years	1935-38	Allies	Axis	.561	2.376	.952	1.392	59.05	.934
Nuclear Explosions	1950-58	U.S.S.R.	U.S.A.	17.61	.664	.982	37.62	5.140	.907
Nuclear Explosions	1961-73	U.S.S.R.	U.S.A.	3.073	.937	.950	169.3	.463	.887
Ballistic Missiles	1961-67	U.S.S.R.	U.S.A.	40.45	.615	.953	463.1	.315	.854
Photographic Satellites	1963-73	U.S.S.R.	U.S.A.	46.55	.282	.992	109.7	.323	.792
Electronic Satellites	1968-73	U.S.S.R.	U.S.A.	39.58	.122	.996	49.83	.513	.899

times in the ballistic missiles race. The compound rates reflect the insecurity produced by one armament unit. In this instance, one ballistic missile does not produce much insecurity, hence the compound rate is small. The exponents of the power functions predicted for the leaders in the bottom of Table 3 are all quite substantial, ranging from 0.122 for the U.S. in the electronic satellites race to 2.38 for the Axis in the pre-World War II race. So everything considered, the curvilinear model also passes this test.

Third, in the top of Table 3, each exponential equation exhibits a better fit than the corresponding power equation, as predicted for the followers' interactions in arms races. In the first nuclear explosions race and the electronic satellite race, the r^2 for the power function is greater than that for the exponential function. However, in those instances, near zero scaling constants combine with large exponents for unreasonable results. In every instance for the leaders' interactions in the bottom of Table 3, the power equation does better than the exponential. Hence, the results indicate the curvilinear model passes the test for its asymmetry.

The r^2 values for the curvilinear model are somewhat less than .99 in four of the seven arms races: those involving years of warlike work, nuclear explosions and ballistic missiles. It should be remembered, however, that the model is based on the way the decision makers of the racing nations perceive and then react to the other

side's armaments. In these four races, the relevant information was secret and, hence, had to be obtained or estimated by intelligence agents. There is little reason to believe that these year-by-year intelligence reports are reliably represented by the more accurate (but still fallible) data analyzed here. Thus, we attribute the attenuated r^2 values obtained for these races to the unreliability of both perceptions and measures. For three of the races, the measurement is unquestionable: in the pre-World War I race, accurate expenditure data were published each year by the various nations and the quantity of launched photographic and electronic satellites are reliably detected by radar. In these three races, it is very significant that the r^2 values are virtually perfect (.994, .999 and .997, respectively, for the followers' interactions and .991, .992 and .996 for the leaders'). Notice also the plots for two of these races in Figures 1-4.

War and Armed Truce

We will now evaluate the alternative predictions of war or armed truce by the two models.

The linear model. Table 4 presents the stability-instability calculations based on the parameter estimates in Table 2. The stability conditions predicting armed truce, as given in equations (28) and (29), are that both $a+\beta$ and $a\beta-kl$ must be positive. The formulae for the equilibrium points (y^* , x^*) are given in equations (32) and

Table 4. Stability and Location of Equilibrium as Implied by Parameter Estimates for Richardson's Model

Measure	Period	Side		Stability		Equilibrium Point		Stopping Point	
		Leader	Follower	$a+\beta$	$a\beta-kl$	y^*	x^*	y_T	x_T
		Differential Equation Solution							
Expenditures	1907-14	Entente	Alliance	.998	-.365	—	—	—	—
Man-Years	1934-38	Axis	Allies	.674	-.009	—	—	—	—
Nuclear Explosions	1949-58	U.S.	U.S.S.R.	-2.615	5.576	—	—	—	—
Nuclear Explosions	1961-73	U.S.	U.S.S.R.	.096	-.013	—	—	—	—
Ballistic Missiles *	1960-67	U.S.	U.S.S.R.	1.192	-.055	—	—	—	—
Photographic Satellites	1962-73	U.S.	U.S.S.R.	.011	66.144	63	226	266	210
Electronic Satellites	1967-73	U.S.	U.S.S.R.	.073	.102	70	108	67	65

* Difference equation solution substituted (see footnote 5).

(33). The observed stopping points (y_T and x_T) are also presented.

According to these results, the first five arms races were unstable and therefore should have resulted in war. The first two did: World War I and World War II. The first nuclear and ballistic races ended with moratoriums or with one party just stopping. Thus, Richardson's model predicted war when it did not occur. The two satellite races appear to be stable as predicted by Richardson's model. However, they did not stop at the expected points. For example, in the photographic satellite race (1962–1973), the model predicts the equilibrium at a point when the U.S.S.R. had roughly 200 fewer satellites than they had put in orbit by 1973. Thus, these stability-instability predictions are partly correct and partly incorrect. Since they are a function of parameter estimates already shown to be unreasonable, their inaccuracy is not surprising.

The curvilinear model. This model implies that the origin ($y^*=0$, $x^*=0$) is the only equilibrium and that it is unstable (any movement will touch off an arms race among rivals). The exponent in the power function describing the leaders' interaction appears to gauge the leaders' aggressive intentions and, thus, predicts whether the outcome will be war or armed truce. To the extent the exponent is greater than 1.0, the leader proportionately increases its relative advantage (see equations [17] and [14]). On the other hand, to the extent the exponent is less than 1.0, the leader allows the follower to catch up. Maintaining or increasing the advantage by the leader (as when $n \geq 1$) appears to predict war and allowing the follower to catch up (as when $n < 1$) appears to predict a negotiated end to the race—an armed truce.

In the pre-World War I race, the exponent for the leader (the Entente) was 1.04, which apparently indicates only a moderate level of aggressive intent—more than enough to maintain the initial armament advantage. The beginning of that war seems to have been somewhat accidental, not the obvious responsibility of the Entente. However, their behavior signalled that their intentions were not necessarily defensive. They were aggressive enough to

more than maintain their relative advantages throughout the race, which may have communicated an unwillingness to live with an equitable balance of power.

The Axis, the leader in the pre-World War II race, increased their relative advantage each round by 2.38. This seems to signal an extreme level of aggressive intent, but no more than was contained in the speeches of their decision makers, nor more than was later evidenced in their *blitzkriegs* in Europe, Africa and the Pacific. Think of it—each year they increased their relative advantage by 2.38!

The U.S., the leader of the cold war arms races, in most instances signalled less and less aggressive intent in subsequent races. The 0.94 exponent for the second nuclear explosions race was higher than the 0.66 exponent for the first. However, the others progressively decreased to 0.62 for the ballistic missiles race, 0.28 for the photographic satellites race, and 0.12 for the electronic satellites race. In every instance, the U.S. allowed the U.S.S.R. to catch up, thus signaling that their intent was not aggression but a balanced armed truce.

In such arms races, there is another possibility. The follower can overtake the leader and thus turn the asymmetry around by refusing to stop: the former follower becomes the new leader and the former leader, the new follower. From that juncture, the race should reflect changed asymmetric patterns of interaction. The prospect is unlikely, however, when the balance involves a nuclear deterrent with a several-fold overkill capacity. Yet the nature of the balance probably has to be negotiated, as the U.S. and U.S.S.R. are finding out.

CONCLUSIONS

Using data from seven arms races we have completed four tests of Richardson's linear, symmetric model and the curvilinear, asymmetric model and are in a position to compare the two. (1) Both pairs of equations fit the data almost equally well. (2) The curvilinear model is more parsimonious. (3) The parameters of the linear model were mixed in sign in a non-

sensical pattern, the apparent result of attempting to describe what is clearly a curvilinear process with linear equations. The parameters of the curvilinear model were well behaved: the compound rate in the exponential equation for followers' reactions evidently gauges the insecurity produced by the arms unit in question, and the exponents in the leaders' power functions gauge the aggressive intent of the leader. (4) Concerning the seven predictions about war or balanced armed truce, Richardson's model made at least three which were in error. None of the predictions by the curvilinear, asymmetric model were incorrect. The inferences drawn from this fourth comparison are based partly on an empirical generalization and are, therefore, more guarded than the others. However, on three of four criteria, the results indicate the alternative model is preferable.

Richardson (1960:14) was quite frank about the tentativeness of his model: he followed Ockham's rule by trying out "the easiest formulae first." In his favor, some of the relationships in the pre-World War I race did approach linearity as may be noted in Figures 1 and 2. One might well question whether simultaneous linear, additive differential equations are the easiest, however. Nevertheless, they have been given a thorough try here and found wanting.

While incorporating certain features of Richardson's verbal paradigm, the alternative model was constructed using a different strategy.⁶ Instead of using ad hoc differential equations, the alternative model was built using experimental generalizations from psychophysics about the behavior of people. These generalizations probably would have delighted Richardson. He was the first social scientist to sense the

potential importance of psychophysics for understanding social behavior. A number of classical theorists such as Mead and Weber have argued the importance of perceptions, but they never had the insight about using psychophysical equations as perceptual premises in social theory. This was an attempt to use these perceptual generalizations to construct a middle-range theory or model of the way a particular conflict, an arms race, is escalated through time. We are not disappointed.

The model does have some very important policy implications since it apparently identifies the characteristics of arms races which predict war or the possibility of a balanced, armed truce. It also suggests there is no stable arms race which will eventuate automatically in an equilibrium level of armaments. It does identify a class of arms races that can eventuate in a balance, but the armed truce may not occur automatically. Very likely, it must be negotiated.

In any event, this is a subject for further research. It would be helpful if the model and all of its implications were evaluated further on data sets generated in additional races. That is one path to a better understanding of these very important phenomena.

APPENDIX A Arms Race Data Sets

Armament Expenditures Race 1907-1914

Expenditures in Millions of Pounds Sterling

Year	Entente	Alliance
1907 ^a	151.8	83.9
1908	156.2	89.4
1909	165.0	96.2
1910	175.8	98.7
1911	187.4	108.1
1912	197.8	113.9
1913	215.1	127.2
1914 ^b	239.2	150.9

(Source: Richardson, 1960:87)

⁶ We tested an additional model which was not built on Richardson's interaction assumptions; the racing nations simply increased their armaments as decaying exponential functions of time— $dy/dt=k(n-y)$ and $dx/dt=l(m-x)$. We attempted to fit the solutions of these equations to the relevant data for the seven arms races, using SPSS nonlinear regression, the Fletcher-Powell nonlinear regression and an iterative program based on a log transformation. None of these programs converged to unique solutions, so the model was rejected.

Photographic Reconnaissance

Satellite Race 1959-1973

Accum. # of Satellites Launched			1965	24	0			
Year	U.S.A.	U.S.S.R.	1966	33	0			
			1967 ^a	41	5			
			1968	48	12			
1959	6	0	1969	54	23			
1960	12	0	1970	58	33			
1961	25	0	1971	61	48			
1962 ^a	50	5	1972	63	55			
1963	67	12	1973 ^b	65	67			
1964	91	24	(Source: Stockholm International Peace Research Institute: 1973; 1974)					
1965	112	41						
1966	135	62	Nuclear Explosions Race 1945-1973					
1967	153	84						
1968	169	113						
1969	181	145						
1970	190	174						
1971	197	202						
1972	205	231						
1973 ^b	210	266						
						Accumulated Number of Explosions		

Nuclear Explosions Race
1945-1973

Accumulated Number of Explosions

Year			U.S.A.	U.S.S.R.
1945			3	0
1946			6	0
1947			6	0
1948			9	0
1949 ^a			9	1
1950			9	1
1951			25	3
1952			35	3
1953			46	5
1954			52	7
1955			68	11
1956			82	18
1957			110	31
1958 ^b			178	90
1959			178	90
1960			178	90
1961 ^a			187	123
1962			276	165
1963			324	165
1964			352	171
1965			380	180
1966			420	194
1967			448	208
1968			485	220
1969			513	235
1970			543	248
1971			554	266
1972			561	287
1973 ^b			570	301

(Source: Richardson, 1960:137)

Electronic Reconnaissance

Satellite Race 1960-1973

Accum. # of Satellites Launched			1970	543	246
			1971	554	266
Year	U.S.A.	U.S.S.R.	1972	561	287
			1973 ^b	570	301
1962	4	0	(Source: Stockholm International Peace Research Institute, 1974)		
1963	11	0			
1964	19	0			

Ballistic Missiles Race
1960-1973

Number of Both
ICBM and SLBM

Year	U.S.A.	U.S.S.R.
1960*	50	35
1961	159	50
1962	438	75
1963	648	200
1964	1250	320
1965	1350	390
1966	1496	425
1967 ^b	1710	590
1968	1710	930
1969	1710	1210
1970	1710	1580
1971	1710	1950
1972	1710	2087
1973	1710	2155

(Source: Institute for Strategic Studies,
1970; 1974)

* Start of race/first year for which data are available.

^b End of race/last year for which data are available.

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COMMENTS

POPULISM AND LYNCHING IN LOUISIANA*

(COMMENT ON INVERARITY, ASR
APRIL, 1976)

From its beginnings in the work of Durkheim (1893), social solidarity has been a central concept in sociology. Indeed, the functionalism of Parsons and the functional structuralism of Merton can be seen to follow in this tradition (cf. Turner, 1974). More recently, Blau (1974) in his "parameters of social structure" and Ekeh (1974) in his analysis of social exchange theory have attempted to synthesize processes of differentiation and integration in society.

This comment addresses one recent attempt (Inverarity, 1976) to examine the causes and consequences of solidarity. Although this comment points out a number of methodological and conceptual problems, it is intended in no way to detract from what can be described as a highly original operationalization and test of mechanical solidarity. In fact, a portion of the reanalysis of Inverarity's data indicates that the data support the theory to a greater extent than reported in certain instances.

Inverarity's First Causal Model

The principal model tested by Inverarity consists of three causes of mechanical solidarity and its indicators (see Figure 1). Consider first the findings reported by the author. Using the procedures developed by Jöreskog et al. (1970), Inverarity (1976: 274) claims that the model produced a χ^2 goodness-of-fit of 11.61 with six degrees of freedom and $p > .05$. The author then concludes:

Since the chi-square value in this case is not significant, there is no major difference between observed and expected values. . . .

Failure to reject the model on statistical grounds means that we can adequately account for the configuration of observed relationships between lynching and five characteristics of the parishes by a model specifying three characteristics of a cause of the unmeasured variable mechanical solidarity

and two characteristics as consequences of solidarity. (Inverarity, 1976: 275)

The interpretation of the χ^2 test by Inverarity is erroneous in this case, and it leads the author to propose a conclusion opposite to that indicated by the data. Given the results reported by the author, one should reject the model and conclude that the structure of causal relationships does not support the theory. The χ^2 test in question is a test of one's model against the most general alternative that the variance-covariance matrix is any positive definite matrix (cf. Jöreskog and van Thillo, 1972: 8). The probability, p , for the χ^2 test is the probability of obtaining a χ^2 value larger than the value actually obtained, given that the hypothesized model is true. Consequently, the larger the value of p , the better the fit. As a rule of thumb, Lawley and Maxwell (1971: 42) suggest that one fails to reject models where the value of χ^2 is below the ten percent significance level, i.e., when $p > .10$.

Actually, the causal analysis performed by Inverarity contains a number of errors, and the data can be shown to support the theory to a degree better than that reported. Although the model proposed by Inverarity will be modified below in order to better represent Durkheim's (1964) theory of solidarity, it will prove informative to reanalyze the data. Inverarity incorrectly applied Jöreskog's procedures by failing to treat the model of Figure 1 as a true multiple indicator-multiple cause (MIMIC) model. In order to use Jöreskog's procedures for the MIMIC model, certain assumptions and alterations must be made to the structural equations representing the causal model. First, the correct equations for the model of Figure 1 must be written as

$$(1) \eta = (\gamma_1 \ \gamma_2 \ \gamma_3) \begin{pmatrix} \xi_1 \\ \xi_2 \\ \xi_3 \end{pmatrix} + \zeta$$

$$\begin{pmatrix} y_1 \\ y_2 \\ y_3 \end{pmatrix} = \begin{pmatrix} \lambda_1 \\ \lambda_2 \\ \lambda_3 \end{pmatrix} \eta + \begin{pmatrix} \epsilon_1 \\ \epsilon_2 \\ \epsilon_3 \end{pmatrix}$$

$$\begin{pmatrix} x_1 \\ x_2 \\ x_3 \end{pmatrix} = \begin{pmatrix} 1 & 0 & 0 \\ 0 & 1 & 0 \\ 0 & 0 & 1 \end{pmatrix} \begin{pmatrix} \xi_1 \\ \xi_2 \\ \xi_3 \end{pmatrix} + \begin{pmatrix} 0 \\ 0 \\ 0 \end{pmatrix}$$

* The author would like to thank Professors Robert F. Boruch, Northwestern University, and Ronald S. Burt, University of California, Berkeley, for pointing out errors made in an earlier version of this comment.

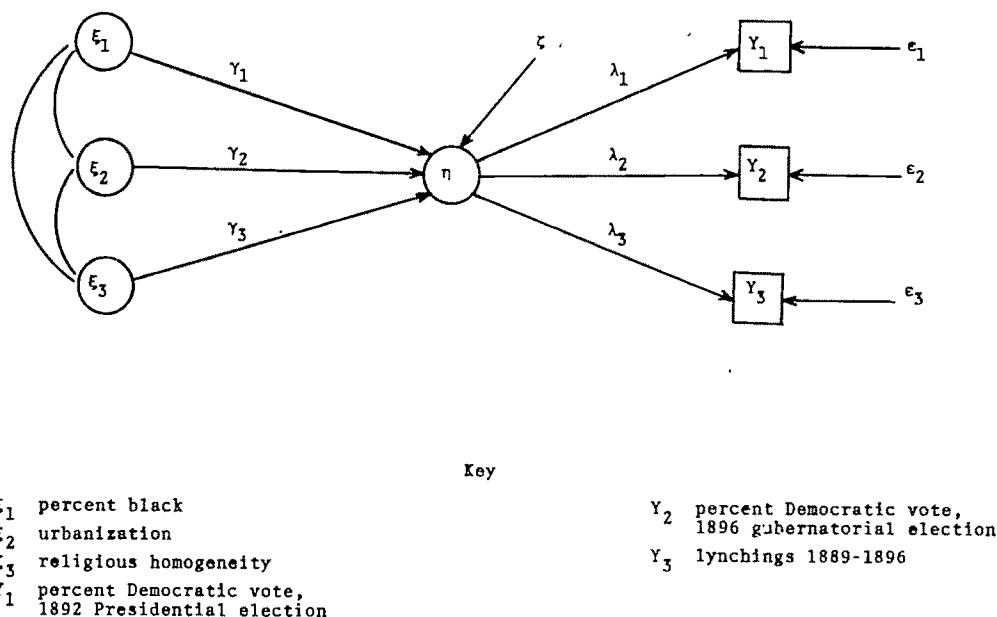


Figure 1. Causal Model for Mechanical Solidarity after Inverarity (1976: 271)

Second, in order to remove an indeterminacy in the structural parameters, the variance of the theoretical construct, mechanical solidarity, must be assumed normalized at unity (cf. Jöreskog and Goldberger, 1975: 632). Third, the degrees of freedom for the MIMIC model must be calculated as $q(p-1)+p(p-3)/2$ where q is the number of observable exogenous variables and p is the number of observable endogenous variables. Finally, it is necessary to treat the correlation matrix as a dispersion matrix when using Jöreskog's procedures to test a MIMIC model.

Using the computer program devised by Jöreskog and van Thillo (1972), LISREL II, Inverarity's first model was reanalyzed as shown in Table 1. Notice that the χ^2 , degrees of freedom, p value, and parameter estimates differ from those reported by Inverarity. Although the model fits the data somewhat better than the one reported by Inverarity, one must still reject the proposed structure. An examination of the parameter estimates indicates that the effects of both urbanization and religious homogeneity upon mechanical solidarity (i.e., γ_2 and γ_3) are not twice their standard errors and hence might be dropped from the model. Alternatively, better measures of these variables could be found. These last points illustrate why it is important to calculate standard errors of parameter estimates in sociological research and how they may be used in a diagnostic and model-building sense.

A final point to note about the analysis of Inverarity is that the low sample size ($n=59$)

may cause a greater degree of variability than desirable in one's parameter estimates. This is the result of the well-known phenomenon that maximum likelihood procedures are notoriously unstable for low sample sizes. In these cases, one might consider correcting the χ^2 estimate using a correction factor such as Box's (cf. Lawley and Maxwell, 1971: 36). For an excellent discussion of external and internal validity and related problems with the χ^2 test, see Burt (1973: 146-50).

Inverarity's Second Causal Model

The second model tested by Inverarity is identical to the first except that a fourth exoge-

Table 1. Parameter Estimates, χ^2 Goodness-of-Fit and Standard Errors for the Model of Figure 1 and the Data of Inverarity (1976)

Parameter	Maximum Likelihood Estimate	Standard Errors	Standardized Estimates
γ_1	1.085	.299	.723
γ_2	.166	.191	.111
γ_3	.165	.195	.110
λ_1	.371	.097	.556
λ_2	.575	.140	.863
λ_3	.208	.097	.312
θ_{ϵ_1}	.831	.087	.831
θ_{ϵ_2}	.505	.153	.505
θ_{ϵ_3}	.950	.090	.950

$\chi^2=11.20$, $df=6$, $p=.08$.

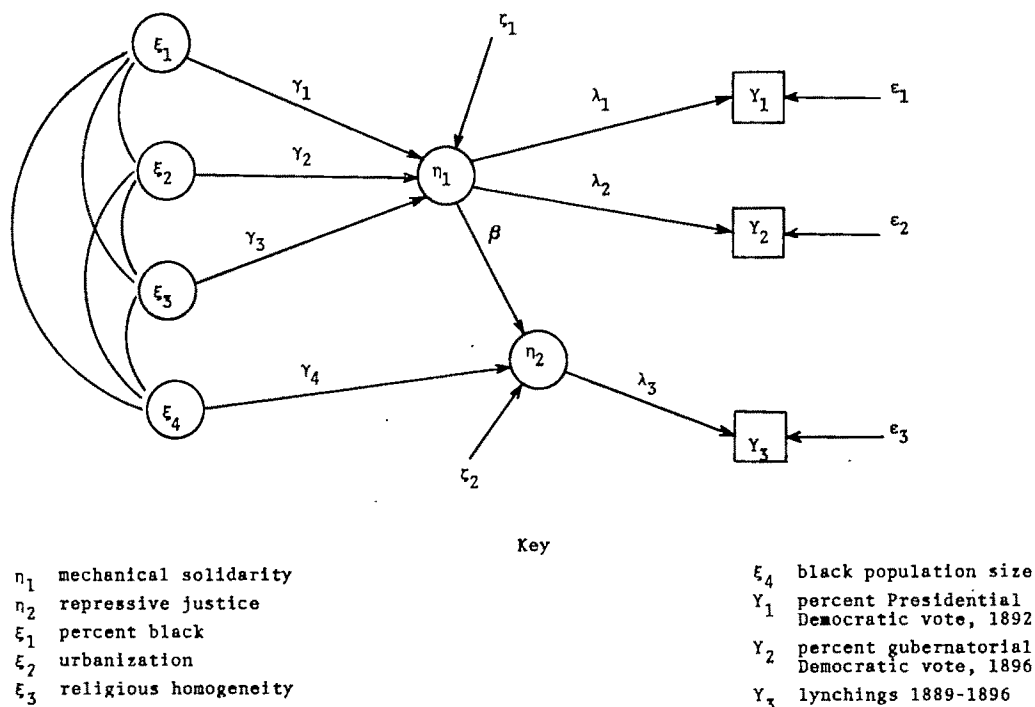


Figure 2. A Causal Model Respecifying the Relationships among the Causes and Consequences of Mechanical Solidarity

nous variable, black population size, is introduced as a cause of repressive justice. Strictly speaking, this is not a MIMIC model and should not be treated as such. Before presenting the reanalysis of Inverarity's second model, it will prove helpful to discuss the specification of the model. In both of his models, Inverarity represents repressive justice as an indicator of mechanical solidarity. This practice is somewhat misleading, and there are conceptual reasons why one might not want to do this. It is important to make a distinction between a causal relation between variables on the one hand and the relationship between a variable and its indicator(s) on the other. The former is a relationship between theoretical constructs, while the latter is a relationship between a theoretical construct and its operationalization(s). Since Inverarity desired to test the effect of mechanical solidarity upon repressive justice, this relationship should have been represented as one between theoretical variables rather than between a theoretical variable and its indicator, as was done in the original analysis. A second point about the specification of the model deals with the two causes of mechanical solidarity, percent black and black population size. Since a common element exists in both of the "causes," they are not separate variables, by definition, and should not be modeled as such. One or the other should have been omitted.

With these caveats in mind, Figure 2 represents a respecification of Inverarity's second model showing repressive justice as a theoretical variable. Unfortunately, the parameter relating mechanical solidarity to repressive justice, β , cannot be estimated in its present form because the model is not identified.¹ The parameter β cannot be separated from λ_3 as the model stands. In order to remove this indeterminacy, one can model lynchings as an infallible indicator of repressive justice. The following represents the structural equations for this modification of Figure 2:

$$\begin{pmatrix} 1 & 0 \\ -\beta & 1 \end{pmatrix} \begin{pmatrix} \eta_1 \\ \eta_2 \end{pmatrix} = \begin{pmatrix} \gamma_1 & \gamma_2 & \gamma_3 & 0 \\ 0 & 0 & 0 & \gamma_4 \end{pmatrix} \begin{pmatrix} \epsilon_1 \\ \epsilon_2 \\ \epsilon_3 \\ \epsilon_4 \end{pmatrix} + \begin{pmatrix} \zeta_1 \\ \zeta_2 \end{pmatrix}$$

$$\begin{pmatrix} y_1 \\ y_2 \\ y_3 \end{pmatrix} = \begin{pmatrix} \lambda_1 & 0 \\ \lambda_2 & 0 \\ 0 & 1 \end{pmatrix} \begin{pmatrix} \eta_1 \\ \eta_2 \end{pmatrix} + \begin{pmatrix} \epsilon_3 \\ \epsilon_2 \\ 0 \end{pmatrix}$$

$$\begin{pmatrix} x_1 \\ x_2 \\ x_3 \\ x_4 \end{pmatrix} = \begin{pmatrix} 1 & 0 & 0 & 0 \\ 0 & 1 & 0 & 0 \\ 0 & 0 & 1 & 0 \\ 0 & 0 & 0 & 1 \end{pmatrix} \begin{pmatrix} \epsilon_1 \\ \epsilon_2 \\ \epsilon_3 \\ \epsilon_4 \end{pmatrix} + \begin{pmatrix} 0 \\ 0 \\ 0 \\ 0 \end{pmatrix}$$

¹ This was pointed out to me by Professor Ronald Burt.

Table 2. Parameter Estimates, χ^2 Goodness-of-Fit and Standard Errors for the Model of Figure 2 and the Data of Inverarity (1976)

Parameter	Maximum Likelihood Estimate	Standard Errors	Standardized Estimates	Standard Errors
γ_1	1.030	.288	.707	.197
γ_2	.122	.181	.084	.124
γ_3	.148	.186	.101	.128
γ_4	.374	.119	.375	.120
β	.134	.091	.197	.133
λ_1	.377	.096	.550	.140
λ_2	.608	.149	.887	.217
θ_{e_1}	.835	.087	.835	.087
θ_{e_2}	.462	.186	.462	.186
$\psi^2_{\eta_2}$.789	.148	.795	.149

$\chi^2=10.31$, $df=8$, $p=.24$.

Table 2 presents the parameter estimates, χ^2 , degrees of freedom, and p value for this model where it can be seen that the model fits the data reasonably well. However, notice that the estimates for the effects of both urbanization and religious homogeneity upon mechanical solidarity and the effect of mechanical solidarity upon repressive justice are not at least twice their respective standard errors, providing evidence for rejection of the model and Inverarity's hypothesis.

One final comment is in order regarding the relative impact of various causal paths in the models. When comparing the relative magnitudes of causal paths for the same population of individuals, one should use standardized parameter estimates (cf. Tukey, 1954; Wright, 1960; Blalock, 1967a; 1967b). Comparing the relative impact of mechanical solidarity on repressive justice with the impact of black population size in Figure 2, one sees that the former is about 53 percent as large as the latter. Similarly, comparing the impact of mechanical solidarity upon repressive justice across Figures 1 and 2, one sees that the former is about 1.58 times the latter. These findings tend to invalidate Inverarity's (1976:276) assertion, "When black population size is explicitly considered, the estimated impact of mechanical solidarity on lynching is cut by more than half."

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SOUTHERN VIOLENCE AND THE
POLITICAL PROCESS

(COMMENT ON INVERARITY, ASR
APRIL, 1976)

A recent work by Inverarity (1976) brilliantly employed sophisticated theory and methodology to examine the manner in which disturbances in a particular society, i.e., Louisiana between 1889 and 1896, influenced the level of repressive justice in the society, as measured by lynching incidents in the society over this time period. The article illustrated how social investigators might employ limited historical data to test the validity of social theories.

However, there are a number of problems with Inverarity's findings for the case of Louisiana and his exposition of the Durkheimian theory. First of all, this author questions the basic substantive premise of the article.

Southern whites in the 1890s experienced a boundary crisis analogous to the three that Puritan Massachusetts experienced during the seventeenth century. Reconstruction consolidated the two classes of Southern whites into a one-party system, but this unity was broken by a wave of agrarian radicalism (Inverarity, 1976:268).

In order to establish a causal linkage between Populism and lynchings, a plot of the frequency of lynching incidents in Louisiana over the period 1889 to 1900 was presented (Inverarity, 1976:267), and it was found that lynching incidents peaked during the election years when the Populists were most politically active in the state.

It seems reasonable to suppose that the crises would be most severe in election years and that, consequently, this will be revealed in the lynching rate. Thus, in the three election years (1892, 1894 and 1896) the average was 17 incidents per year. Moreover, the sharpest increase in lynchings occurs in 1896, the year of the most bitter contest in the gubernatorial election (Inverarity, 1976:268).

The previous argument assumes a zero time lag between Populist political activity and lynching activity, an assumption that will be accepted in this piece. In order to examine further the cross-sectional and longitudinal patterns of lynchings in this time period, let us differentiate the South into two subregions and then examine lynching patterns in these two subregions, as well as the remainder of the nation, between 1889 and 1917. Recent studies (Cosman, 1966; Matthews and Prothro, 1966:169-72; Strong, 1971) have shown that the eleven states of the old Confederacy may be differentiated into states in the upper South (i.e., Arkansas, Florida, North Carolina, Ten-

nessee, Texas and Virginia) and the deep South (i.e., Alabama, Georgia, Louisiana, Mississippi and South Carolina). The pre-Civil War plantation system was concentrated within the states of the deep South, and historically these states tended to have a low degree of urbanization and a large nonwhite population concentration in their rural areas. By contrast, the plantation system was less developed in the upper South, and these states tended to have a relatively low nonwhite population in their rural areas. An examination of the number of lynchings in the three areas between 1889 and 1917 (N.A.A.C.P., 1969) indicates that the highest number of lynchings occurred in the deep South, while the lowest number of lynchings occurred in the remainder of the nation. Also, in all three areas, lynching incidents reached some maximum value in the time period 1890 to 1900 and then fell off. This national and regional pattern of lynchings raises questions about Inverarity's (1976) assumption that the falloff in lynching incidents in Louisiana after 1896 indicated that a reunification of the white class groups in the state was occurring.

In order to relate further the Louisiana pattern to that of other similar areas, let us examine changes in lynching incidents in all states of the deep South between 1889 and 1917. As can be seen from Table 1, there were sharp variations as to when lynching incidents peaked in the five states of the deep South in this time period. For example, in Mississippi they peaked in 1891 and then proceeded to decline, whereas in Georgia they remained at a relatively high level until 1911. In Alabama, a state with a strong Populist movement, they were high in both 1892 and 1896, but they reached their peak in 1897. The data in Table 1 for the five Southern states do not support the position that Populism had a similar effect on lynching patterns within these five states, an effect one would expect if the Inverarity (1976) model were correct, given the similar cultural history of these five states and the relative strength of Populism in them.

Another difficulty with the article involves the conceptualization and testing of the theoretical model. The work is concerned essentially with determining whether changes in the level of mechanical solidarity in a society leads to a growth of repressive justice within the society. In the article, Louisiana society is differentiated in terms of county units, the level of mechanical solidarity is measured indirectly from three indicators (percent black, the degree of urbanization and the extent of religious homogeneity in the county) and the level of mechanical solidarity then is related to the

Table 1. Frequency of Lynching Incidents in the Deep Southern State between 1889 and 1917

Year	Lynching Incidents					
	Total Incidents	Alabama	Georgia	Louisiana	Mississippi	South Carolina
1889	50	6	8	9	22	5
1890	36	6	12	6	9	3
1891	57	13	8	13	22	1
1892	61	14	13	17	9	5
1893	42	12	7	8	9	6
1894	54	10	14	13	13	4
1895	47	11	14	4	12	6
1896	48	14	7	19	4	4
1897	60	16	13	10	15	6
1898	41	7	11	8	10	5
1899	44	5	15	10	13	1
1900	44	4	13	7	18	2
1901	48	10	12	10	11	5
1902	31	5	6	9	9	2
1903	40	3	10	9	14	4
1904	31	6	11	2	7	5
1905	24	2	3	4	13	2
1906	32	5	5	8	10	4
1907	35	10	5	9	9	2
1908	34	3	12	6	13	0
1909	37	7	11	9	7	3
1910	25	6	9	4	5	1
1911	26	4	15	4	2	1
1912	33	8	10	6	5	4
1913	20	2	6	3	8	1
1914	20	2	1	7	6	4
1915	28	4	11	3	9	1
1916	16	2	9	2	2	1
1917	18	4	6	5	2	1

level of repressive justice in the society. It seems to this author that Erikson's theory does not lead to the conclusion that during a "boundary crisis" the level of mechanical solidarity within a society will have a positive impact on the level of repressive justice within the society. Instead, it implies that *changes* in the level of mechanical solidarity within a society during a "boundary crisis" will have an impact on the level of repressive justice within the society. The Erikson model is concerned with the influence of dynamic changes in the society upon the level of repressive justice in the society. The article assumes that within a society undergoing a "boundary crisis" the level of repressive justice will be greater in areas of the society with a higher level of mechanical solidarity. However, using Erikson's theory one also might argue that under these circumstances the portion of the society that possesses a higher level of mechanical solidarity should be less influenced by these disruptions and should thus experience smaller changes in its level of repressive justice, a result in contra-

diction with the findings of Inverarity's (1976) work.

This author would maintain that an alternate model developed by Snyder and Tilly (1972) to understand collective violence can be employed to explain lynching behavior in Louisiana and the remainder of the South. Snyder and Tilly (1972) were concerned with developing models to explain changes in the level of collective violence in France between 1830 and 1960. Rather than maintaining that lynching represents a form of repressive justice, let us define it as a form of collective violence.¹ Given this definition of lynching,

¹ In their work, collective violence is characterized in the following manner: "Our measure of collective violence is the estimated number of participants in continental France as a whole. 'Disturbances' are continuous interactions involving at least one group of fifty or more persons in the course of which someone seized or damaged persons or objects over resistance" (Snyder and Tilly, 1972:522). Assuming that most lynch mobs were composed of

which is consistent with the definition of the previous authors, one then can employ their model, assuming that lynching rates in the South in the time period 1889 to 1917 were related to the overall rate of collective violence in the region. The previous authors maintained that the immediate causes of collective violence are *political*, in the sense that collective violence results from changes in the power relationships between groups of men.

We suppose that the principal, immediate causes of collective violence are political. Collective violence results from changes in the relationship between groups of men and the major concentrations of coercive power in their environments (Snyder and Tilly, 1972:520).

Lynching in the South tended to be aggression against lower class whites and blacks by other white class groups in the South. One political activity that might have been related to changes in the lynching rate could be the disfranchisement of the blacks and the lower class whites in the South in the period between 1890 and 1910. A precise test of the political model would require a specification of other relevant independent variables and a statistical analysis which relates changes in the lynching rate to these specified variables.

There is some fragmentary evidence available at the present time which lends support to the position that the disfranchisement process was related to changes in the lynching rate. First of all, with regard to the state of Mississippi, Kousser (1974:139-45) has shown that it was between 1889 and 1892 that a series of political maneuvers occurred in the state which essentially disfranchised all blacks and many lower class whites ("ignorant and vicious whites") from the electorate. As can be observed from Table 1, it was also in this time period that the peak number of lynching incidents occurred in the state.

The results from the state of Louisiana with regard to the disfranchisement of blacks and lower class whites were somewhat distorted by Inverarity (1976).

"The agrarian revolt, however, died quietly. . . . The Democrats adopted the policy of disfranchising the blacks. In effect, this policy shifted emphasis from economic interests back to racial status as the basis of political organization" (Inverarity, 1976:266).

more than fifty individuals, lynching by this definition would have to be considered a form of collective violence. In the previous study, most of the measured forms of collective violence were directed against the state, whereas most of the lynching incidents involved interactions between class and racial groups.

The above historical assertion by Inverarity is empirically inconsistent with a recent study of the disfranchisement process in Louisiana in this time period (Kousser, 1974:152-65). In fact, it was in the period from 1892 to 1896 that political disfranchisement actually occurred in the state, with the political process related to this process peaking in the years 1892, 1894 and 1896, the same years when lynching incidents peaked in the state. Thus, in 1892, the state legislature set up a constitutional commission to limit the electorate to those who paid poll taxes and could read or write; in 1894, the New Orleans Ballot Reform League proposed to eliminate fraud by disfranchising illiterates (blacks and lower class whites); in 1896, there was an attempt to pass a constitutional amendment limiting the vote, but this amendment was defeated in the state election in April of 1896. However, in the latter part of 1896, the Louisiana State Legislature passed a bill which essentially disfranchised most blacks and many lower class whites.

The registration act, which went into effect after January 1, 1897, cut the white registration by more than half and the Negro by 90%. The disfranchisement of almost all Negroes and many whites was, therefore, a *fait accompli* by the time of the constitutional convention (Kousser, 1974:162-3).

The previous statement raises serious questions about Inverarity's (1976) substantive conclusions. One can perform a crude test on the lynching data (N.A.A.C.P., 1969) for Louisiana to determine whether it was the Populist activity or some other factors, such as the disfranchisement process, which was related to the high level of lynching incidents in the state during the year 1896. In 1896, there were two aspects to the political struggle engaged in by the Populists. On April 27, 1896, there was a gubernatorial election in which the Republicans and the Populists formed a coalition to oppose the Democrats (Kousser, 1974:157), while on May 28, 1896, the Republicans and the Populists formed a coalition in an attempt to elect their senatorial candidate in the state legislature (Kousser, 1974:158-9). Assuming a zero time lag between political activity and lynching incidents and noting that the disfranchisement activity was occurring throughout 1896, one would predict either more lynching incidents between January 1st and April 28th, or more incidents between January 1st and May 28th, assuming that Populist activity was indirectly related to lynching behavior in the state. If there were not significantly more lynching incidents in either of the above periods, one could conclude that it might be something else beside the

Table 2. Goodness-of-Fit Tests for the 1896 Louisiana election Results in Relation to Lynching Incidents

Time Period	Actual Number of Lynching Incidents	Expected Number of Lynching Incidents
April 28, 1896 Gubernatorial Election *		
January 1, 1896–April 28, 1896	6	6.178
April 29, 1896–December 31, 1896	13	12.822
May 28, 1896 Senatorial Legislative Struggle **		
January 1, 1896–May 28, 1896	8	7.735
May 29, 1896–December 31, 1896	11	11.265

* Chi-square=.00756, df=1, $p>.900$.** Chi-square=.01531, df=1, $p>.900$.

Populist activity which explained the high number of lynching incidents in the state in 1896. In order to make a rough test of these two assumptions, we performed a chi-square goodness-of-fit test on the available data, assuming that it was generated by a random process.² The results from Table 2 show that significantly more lynching incidents did not occur in either of the earlier time periods and thus indicate that lynching incidents occurred randomly throughout 1896. This finding raises questions concerning Inverarity's model, but it does not validate alternate models. In order to explore the problem further, it would be necessary to present a number of alternate models and test them with time-series data.

This comment has raised questions about Inverarity's (1976) assumption that Populism was related to increases in the lynching incident rate in Louisiana in the 1890s and has questioned his use of the "boundary crisis" model. In its place, a model which defines lynching as a form of collective violence and

relates changes in this variable to political changes has been proposed. More extensive data fitted to different models will be required in order to test the various models tentatively proposed in this piece.

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² In this case, the chi-square goodness-of-fit test is not designed to determine differences between sample and population results but, rather, to determine whether the observed and estimated number of lynching incidents might have been generated by a random process (Leabo, 1972:520–8). Thus, our null hypothesis, which supports the disfranchisement model or some other model, assumes that lynching incidents occur randomly throughout the year, while the alternate hypothesis, which supports the Inverarity (1976) model, assumes that they are concentrated in certain time periods. Clearly the total number of lynching incidents (19) for the year 1896 is small; however, the expected values in the individual cells are greater than five.

MECHANICAL SOLIDARITY, REPRESSIVE JUSTICE, AND LYNCHINGS IN LOUISIANA*

(COMMENT ON INVERARITY,
ASR APRIL, 1976)

Noting Erikson's indebtedness to "Durkheim's classic formulation of the relationship between repressive justice and mechanical solidarity," Inverarity (1976:262) asserts his intent to use Erikson's theory—"the disruption of solidarity (in his terms a 'boundary crisis') produces a sudden and dramatic increase in repressive justice (a 'crime wave')"—to explain lynchings in Louisiana. We applaud Inverarity's use of quantitative historical data to develop sociological theory. However, his effort is marred by both theoretical and methodological defects.

THEORETICAL DEFECTS

Mechanical Solidarity and Repressive Justice in Louisiana

We reject Inverarity's contention that lynchings in Louisiana during the Populist period constitute enactments of repressive justice in response to disruptions of mechanical solidarity. In *The Division of Labor in Society* (hereafter referred to as *Division*), Durkheim (1960:70-229, esp. 129-32, 226-9) carefully distinguished between two types of solidarity, mechanical and organic. Mechanical solidarity is based on commonalities or likenesses. Individuality is undeveloped, and individuals are interchangeable with one another. Structurally, mechanically solidary societies are undifferentiated. In contrast, organic solidarity presupposes individuality and is based on mutually complementary differences between and among individuals and on the functional interdependence of differentiated structures. There is ample reason to reject Inverarity's claim that Southern white society was mechanically solidary.

First, Inverarity (p. 268) refers to the existence during Reconstruction of the Solid South whose unity "was broken by a wave of agrarian radicalism." He suggests that for some period prior to the Civil War there was no Solid South. The Civil War helped solidify the South, and this solidarity among whites was perpetuated through Reconstruction. This

white unity was threatened, if not broken by agrarian radicalism and was restored (pp. 266, 268) as Populism was crushed. In Durkheim's theory, however, during the course of evolutionary development, mechanical solidarity gradually recedes with the growth of organic solidarity. Accompanying this change in the nature of solidarity is a change in the morphology of society from an undifferentiated "mechanical" mass to an "organic" system of differentiated and (in the "normal" instance) mutually complementary structures and functions. Durkheim did not portray societies as rapidly fluctuating in and out of mechanical status; among other things, this would require a corresponding fluctuation in the morphology of society foreign to Durkheim's theoretical perspective.

Second, Inverarity (p. 262n) asserts that "mechanical solidarity is based on similarity of individual characteristics . . . and is characterized by consensus on values, harmony . . . of interests and unity of purpose." Elsewhere, developing the idea of the Solid South, he (p. 265) refers to the "consensus on values, harmony of interests and unity of purpose among Southern whites."¹ By Inverarity's own account, the South can be seen as mechanically solidary to the extent that the features which he identifies characterize Southern white society. But do they? Inverarity's description of the South provides the answer, a description which includes "a dichotomous class structure" (p. 265), the enagement of lower class whites (at wealthier whites) that sometimes led to open rebellion against the Confederacy (p. 265n); "costs . . . borne by the poor whites" (p. 266); conflicts in interests over "railroad rate regulation, cheaper credit and higher wages" (p. 266); "the conflict between the two classes of whites" (p. 266); and Populist discussions of "open, armed rebellion" due to frustration with "legitimate means of political opposition" (p. 266). Inverarity's own description of the South provides ample basis for rejecting the claim that Southern white society was mechanically solidary. More than Durkheim's explication of mechanical solidarity, this description brings to mind the Marxian image of class conflict or the Weberian image

¹ Inverarity's portrayal of the South is problematic. He (p. 265) argued that Durkheim's concept of mechanical solidarity could be applied to the South because, "the familiar idea of the Solid South may be viewed as an instance of mechanical solidarity among Southern whites"; furthermore, "from the end of Reconstruction to the last decade of the century, the Solid South remained intact." However, in that same paragraph he also argued that "the Solid South was a social myth."

* We thank Jere Cohen for his comments on an earlier version of this paper and Carolyn J. Mullins for her editorial assistance. We also thank James Inverarity for his reactions to an earlier version of this comment. Inverarity's reactions have proved useful in our attempt to clarify the issues raised here.

of the struggle for competitive advantage between and among classes, status groups, and parties.

Durkheim, of course, asserts that mechanical solidarity persists in the organic setting to a limited degree. Given that both types are thought to be present, it is reasonable to assess the amounts and, perhaps, the relative balance between the two. In so doing, it may be tempting to view any group identified and thereby defined by a shared characteristic as mechanically solidary. The very definition itself—e.g., workers, Catholics, Southern whites—focuses attention on similarities. Clearly, however, the implicit argument that the group of individuals in question are similar on some dimension and therefore mechanically solidary is unconvincing. Obviously, further demonstration is required—a demonstration which Inverarity does not provide. For instance, he nowhere claims that Southern whites were all alike and exhibited the lack of individuality which Durkheim saw as the source of the mutual attractiveness which constitutes or leads to mechanical solidarity. Nor does Inverarity show that Southerners exhibited less individuality than that typical of individuals in organically solidary groups. Further, Inverarity nowhere asserts that Southern white society was less structurally differentiated than the modern social units which Durkheim describes as organic. Inverarity (p. 265) points simply to one commonality—the individuals in question were all white—and to their common interests *vis-à-vis* external threats (blacks, the North). However, against this must be placed his account of the severe conflicts and cleavages permeating white society, conflicts and cleavages in no way consistent with Durkheim's description of mechanically solidary groups.²

What Inverarity's description of Southern white society does bring to mind is Book Three of *Division* (Durkheim, 1960:353–95), which

describes three abnormal (pathological) forms of the division of labor. In discussing two of these forms, the anomic and the forced division of labor, Durkheim (1960:354, 355, 374) refers to "the conflict between capital and labor," the "sharp line . . . between masters and workers," and "class-wars" (compare the title, *Origins of the Class Struggle in Louisiana*, Shugg, 1939). Ironically, Inverarity's account of the South points less to the presence of mechanical than to the absence of organic solidarity.

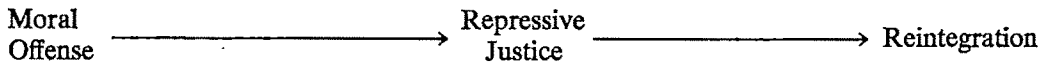
We suggest that what Inverarity had—or, at least, should have had—in mind was something other than Durkheim's mechanical solidarity—perhaps changing levels of integration in white society.³ At least such a formulation reduces or eliminates important defects and discrepancies noted above. Specifically, this formulation (1) allows for the rapid fluctuations in level of integration described by Inverarity; (2) permits analysis of changes in the relative importance of factors that promote cleavage (e.g., class conflict) versus factors that favor a commonality of interests (e.g., external threat); (3) eliminates any implication that mechanical solidarity was predominant in Louisiana; (4) avoids any implication that Louisiana was relatively integrated as opposed to being relatively unintegrated; (5) eliminates the disparity between Inverarity's explication of mechanical solidarity and his description of the South.

Furthermore, this reformulation is consistent with both Durkheim's and Erikson's theories. Erikson's theory is not limited to instances of mechanical solidarity (see discussion below). In *Suicide* (which Inverarity does not cite), Durkheim discusses the relationship between external threats and integration. Durkheim (1951:208) argues that "great popular wars rouse collective sentiments" and thereby "cause a stronger integration of society" as people concentrate on the need to unite in the face of common danger. Durkheim (1951:160) also notes that Jews' minority status and the general hostility they confront increase the integration of Jewish society. Similarly, Inverarity (p. 265) identified blacks and the North as external threats that fostered the integration of Southern white society.

² In a personal communication, Inverarity asks: given that Durkheim treats the modern military as mechanically solidary, why cannot Southern white society in Louisiana also be treated as an instance of mechanical solidarity? We believe the reasons are clear. Durkheim (1951:234) observed that "the first quality of a soldier is" a degree "of impersonality not to be found anywhere in civilian life." Durkheim's (1951:233–9) explanation of military suicide is consistent with his observation that "of all elements constituting our modern societies, the army . . . most recalls the structure of lower societies" (p. 234). In contrast to lower societies and the military, Louisiana is a modern society, certainly as modern and organic as social structures in France and elsewhere characterized by Durkheim (1960) as (primarily) organically solidary.

³ We wish to distinguish here between Durkheim's general concept of integration or social solidarity and his types of solidarity. Integration refers to the strength of the bond between the individual and the group, while the terms "mechanical" and "organic" refer to the different forms this bond may take. In *Division*, Durkheim (1960:37–8) argues that, on the whole, integration increases as organic solidarity supersedes mechanical solidarity.

Durkheim



Erikson via Inverarity

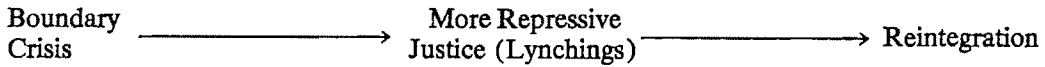


Figure 1.

In a similar fashion, we reject Inverarity's contention that lynchings, in particular the lynching of blacks by whites, are manifestations of repressive justice. While Durkheim argues that the collective conscience and the repressive justice it engenders persist in the organic setting, lynchings do not conform to Durkheim's model. Because crime violates society's moral rules, it "shocks sentiments . . . found in all healthy consciences" (Durkheim, 1960:73). Further, violation of these sentiments invokes punishment, which Durkheim (1960:96) *summarily defines* as a "passionate reaction . . . that society exercises . . . upon *those of its members who have violated*" moral rules (*italics added*). Here, as at many other points, Durkheim emphasizes his concern for the nature and changing nature of the relationship between the individual and the group (see, e.g., Durkheim, 1960:37-8).

In the instance of lynchings, the victim is a member of an *outside* group.⁴ Blacks are *not* part of white society, nor are they lynched because of offenses committed against other blacks. Indeed, whites often remain relatively unconcerned about the offenses that blacks commit against other blacks. Rather, blacks are lynched because they have become targets of white ire. They may or may not have committed some criminal or other offense. Clearly, then, lynchings are *not* examples of repressive justice and Durkheim's argument in *Division* does not address the phenomenon Inverarity seeks to explain.⁵

⁴ Table 1 in Inverarity (p. 263) shows that 15% of the lynchings were of whites. Inverarity would have been better off assessing the applicability of Durkheim's theory to instances in which whites lynched whites, particularly for such offenses as homicide, rape, and blatant violations by whites of racial etiquette.

⁵ Simmel, of course, had much more to say about the relationship between external threats and the integration of the group than Durkheim. In *Conflict and the Web of Group Affiliation*, for example, Simmel (1964:96-8) suggests that groups sometimes may seek to maintain external threats in order to

Durkheim addresses the question of inter-group sanctions in *Moral Education*. He argues, "Whenever two populations, two groups of people having unequal cultures, come into continuous contact with one another, certain feelings develop that prompt the more cultivated group—or that which deems itself such—to do violence to the other" (Durkheim, 1961:192-3). Further, he asserts that this feeling of superiority "produces a veritable intoxication, an excessive exaltation of self, a sort of megalomania, which goes to the worst extremes," and that the violence this feeling engenders is only restrained by "moral forces which he [the one who feels superior] respects and on which he dare not encroach" (Durkheim, 1961:193). Clearly, Durkheim would apply this argument to race relations in the South rather than his model of repressive justice, in which violence is engendered by moral sentiments, not simply restrained by sentiments. Indeed, he remarks that violence engendered by feelings of superiority is common in "colonies and countries of all kinds where representatives of European civilization find themselves involved with underdeveloped peoples" (Durkheim, 1961:193).

Erikson and Durkheim as Theoretical Foundation

We may compare Erikson's theory as interpreted by Inverarity with Durkheim as follows (see Figure 1). The two models are similar in viewing reintegration as the effect of repressive justice. Beyond that, however, differences abound. Lynchings are not examples of repressive justice, so the two models address different phenomena. Similarly, the factor that elicits repressive justice differs; for Durkheim it is a moral offense, while for Inverarity it is a boundary crisis.

The two models also differ on the nature of

maintain the integration of the group. This argument seems more relevant to the relationship between Populism and lynchings than Durkheim's model of mechanical solidarity and repressive justice.

W. J. Wilson

the causal links between variables. Inverarity attempts to explain variation in the amount of repressive justice. Though Durkheim often explained variation and tested for cause-and-effect relationships by determining whether two phenomena varied systematically together (e.g., Durkheim, 1951), his model of repressive justice is not conceived in this fashion. For Durkheim, crime led to punishment, the latent function of which is to reintegrate the group. Rather than using his theory to explain variation in the rate at which repressive justice occurred, Durkheim analyzed punishment as one mechanism that explained the integration and reintegration of the group.

The attempt to use Durkheim's model to explain variation in the rate of repressive justice as a function of mechanical solidarity immediately encounters difficulties. According to Durkheim, the more powerful the collective conscience, the more it controls individual behavior. Thus, it might be argued that the more powerful the collective conscience, the less likely are moral offenses and, consequently, the lower the crime rate. However, it is also the case that the more powerful the collective conscience, the greater the number of acts that, potentially at least, might be defined as moral offenses. Thus, it might also be argued that the stronger the collective conscience, the more likely are moral offenses and the higher the rate of crime. There is a further complication. As the crime rate changes, the definition of crime also is likely to change, and that change itself affects the crime rate (Durkheim, 1964:68-9). It may be that Durkheim's recognition of these problems partly explains his failure to use his theory of the relationship between mechanical solidarity and repressive justice to explain variation in crime rates.

Inverarity (e.g., pp. 262, 272, 278) repeatedly attributes to Erikson the concept of repressive justice and sometimes (e.g., p. 272) that of mechanical solidarity. In one passage, he (p. 272) refers to "Erikson's thesis on repressive justice and *mechanical solidarity*" (italics added). However, Erikson (1966:9) stated that the argument developed in *Wayward Puritans* "should fit all kinds of human collectivity"; he did not limit his theory to instances of mechanical solidarity. Indeed, the term does not appear in his work. Thus, Inverarity's attribution of this concept to Erikson is misleading. Furthermore, this attribution is inconsistent with Inverarity's own conclusion that his test "leads to an important qualification of [Erikson's] formulation, viz., that boundary crises cause crime waves *only to the*

extent that the community is mechanically solidary" (p. 275).

Of Inverarity's three central explanatory theoretical concepts, two (mechanical solidarity and repressive justice) are found in Durkheim's work but not Erikson's, while the third (boundary crisis) appears in the latter but not the former. Clearly, Inverarity should have taken care to state the extent to which he was testing Erikson's theory, Durkheim's theory, neither, or both. Unfortunately, he often presented Erikson's theory as a direct extension of Durkheim's (see, e.g., Inverarity's characterization of *Wayward Puritans* as "the major work on Durkheim's . . . thesis concerning the relationship between repressive justice and mechanical solidarity" (p. 276). He failed to specify the relationship of, differences in, and boundaries between the work of the two men. He attributed to Erikson explicit use of two concepts found only in Durkheim. These failings, together with his erroneous claim to have tested Durkheim's theory of the relationship between mechanical solidarity and repressive justice, have produced a theoretical muddle.

METHODOLOGICAL DEFECTS

Serious methodological defects also plague Inverarity's discussion. These include the variables he uses in his MIMIC model, his failure to report the standard errors of his estimates, and the low explanatory power of his model.

Inverarity constructed his independent variables—percentage of blacks, number of blacks, urbanization, and religious homogeneity—from data found in the census of 1900 and that of 1916. The latter census occurred twenty years after the high point of Populism. Inverarity (p. 269n) implicitly acknowledges that it may be inappropriate to explain dependent variables measured at one point in time with independent variables measured at a later point in time; he argues, however, that this procedure does not undermine his empirical analysis because the intercensus correlations between the relevant variables are high. Nevertheless, this argument does not explain why data from the later period are used, given that similar data are present in the 1890 census. Furthermore, the correlations between his independent variables and the number of lynchings fluctuate considerably. For example, our data show that the correlation between urbanization *dichotomized as a dummy vari-*

able and the number of lynchings is .293 for 1890 (N.A.A.C.P., 1969; U. S. Census Office, 1892), while Inverarity (p. 270) reports a correlation of .170 with 1900 urbanization data.

A more serious problem lies in Inverarity's finding that urbanization is positively associated with mechanical solidarity. In *Division*, Durkheim (e.g., 1960:256) treated urbanization as both a cause and an indicator of organic solidarity. Thus, Inverarity finds that the greater the structural characteristics (population density and differentiation) associated with organic solidarity, the greater the mechanical solidarity. Inverarity (p. 275) suggests that this positive relationship exists *because* he coded urbanization as a dichotomous variable, and very few parishes had any urban population. As a result, he argues, the contrast is weak—between parishes that are primarily rural and parishes that contain small towns. In fact, dichotomization *obscures* a strong relationship between urbanization and lynchings. The correlation between number of lynchings and percentage living in urban areas in 1890 is .425. This figure contrasts with Inverarity's (p. 270) report for 1900 of a .170 correlation between number of lynchings and urbanization *dichotomized as a dummy variable*. Furthermore, the percentage urban in 1890 is more strongly correlated with number of lynchings than any variable Inverarity (p. 274) reports. While it is true that this correlation is inflated by an outlier (Orleans parish, which was 100% urban in 1890, experienced a number of lynchings), the relationship is undeniably positive given that nine of the twelve parishes with any urbanization in 1890 experienced lynchings between 1889 and 1898.

There are also difficulties with Inverarity's choice of dependent variables. He (p. 264) argues that voting statistics indicate the severity of the boundary crisis: the greater the Populist vote, the greater the boundary crisis. Since he also argues that boundary crises produce waves of repressive justice, the reader expects that the Populist vote as a positive indicator of boundary crisis or the Democratic vote as a negative indicator should be used to predict the prevalence of lynchings. Inverarity (p. 276), however, asserts that the parishes of Louisiana experienced only "a single boundary crisis" and, therefore, excludes political indicators as causal factors. This is untenable, given that the variation in the political crisis as indicated by voting statistics was extreme. The Fusion vote (Populists plus Republicans) in the gubernatorial election of 1896 varied from 0% in the parishes of East Carroll, Madison, Tensas and West Feliciana to over 60% in the parishes of Acadia, Lincoln, Winn, Jackson, East Baton

Rouge, St. Mary's and Assumption (New York Tribune, 1898:271). Certainly, those parishes in which there was no Populist support did not experience a boundary crisis; Inverarity, however, excludes this consideration. In fact, the correlation between degree of boundary crisis as indicated by Populist support and the number of lynchings is negative; this implies that lynchings were most numerous where the boundary crisis was the least, which contradicts Inverarity's thesis.

Inverarity uses political variables in his model as indicators of *white* "mechanical solidarity." This, too, is untenable, given that approximately half of the voters were black and, as Inverarity (p. 274n) notes, there was much fraud and coercion, particularly in counties heavily populated by blacks. Illegal tactics greatly inflated the Democratic percentage in these parishes, sometimes to nearly 100% (Webb, 1962:176–226). The result is an empirical muddle. According to the structure of Inverarity's model, the Democratic vote indicates the solidarity of whites. According to Inverarity's theoretical discussion, the Democratic vote is a negative indicator of the boundary crisis. According to historical accounts, the Democratic vote indicates the coercion of black voters. In fact, the Democratic vote may indicate all three. If this is true, however, the variable is of doubtful utility and certainly should not be used for hypothesis testing purposes.

Inverarity's measurement of "repressive justice" is also problematic. Conceived as a variable, this concept is typically operationalized as a rate. If lynchings are acts of repressive justice (we argue above that they are not), then calculation of a lynching rate should precede analysis of variations. Reed (1972:356) argues, "To relate the number of lynchings in a population to the proportion of Negroes in that population, it is necessary to calculate some sort of rate, since the absolute frequency is obviously related somehow to the population size." Inverarity, however, analyzes only the raw number of lynchings; he does not conform to the usual practice of examining the rate of lynchings, nor does he attempt to justify his focus on the raw number of lynchings.

Another difficulty is that Inverarity does not report the standard errors of the coefficients in his models. This omission makes it impossible for readers to evaluate the significance of the paths from his independent variables to mechanical solidarity. Percentage black appears to be the only important causal factor, but is it the only significant one? If it is, this fact would challenge Inverarity's argument

that he is estimating "mechanical solidarity" and, thus, alter his interpretation of his findings.

This same problem plagues Inverarity's test (p. 277) of the alternative model, which includes number of blacks as an independent variable that is hypothesized to have a direct effect on number of lynchings. The path from mechanical solidarity to number of lynchings decreases from .283 to .159 (pp. 273, 277) when number of blacks is included in the model. However, it is impossible to determine whether the path is still significant in the second model, given that it only *may* have been significant in the first. The fact that the effect is small in both models may obscure the change in level of significance.

Still another defect is the small explanatory power of Inverarity's model. As we noted, the path from mechanical solidarity to number of lynchings is weak; the amount of variation in lynchings explained by mechanical solidarity is slight. This explained variance must be assessed in the context of questionable assumptions upon which the model is based. One assumption is that the independent variables influence the number of lynchings only through mechanical solidarity. This clearly is not true for the relationship between urbanization and number of lynchings. The primary source of contemporary data on lynchings is newspaper accounts. Since newspapers are an urban medium, lynchings probably were more likely to be *reported* if they occurred in urban areas. This effect, which is unrelated to mechanical solidarity, might occur regardless of the true distribution of lynchings between urban and rural areas. The second questionable assumption—that the independent variables are measured without error—is unrealistic in most sociological models and particularly unrealistic for Inverarity's model, since his independent variables are based on data from censuses that were taken years after the Populist revolt. Finally, given the low explained variance, there is little reason to expect that these findings could be duplicated in similar analyses of other Southern states that also experienced high rates of lynchings during this same period.

CONCLUSION

Inverarity erroneously treats white society in Louisiana as an instance of mechanical solidarity and lynchings as an example of repressive justice. He produces a theoretical muddle by using concepts and causal relationships from both Durkheim and Erikson without always indicating accurately which ideas and causal relationships are from which theorist.

Often treating Erikson as an extension of Durkheim, Inverarity implies that he has tested the latter when clearly he has not. Inverarity's models suffer from various methodological defects that, together with the small amount of variance explained, make it questionable whether he has provided significant empirical support for the ideas he claims to have tested. Finally, though Inverarity claims to have tested Erikson, the latter's basic causal concept, boundary crisis, does not appear as an independent variable in Inverarity's own models. In sum, Inverarity does not show how the relationship between Populism and lynching in Louisiana can be explained in terms of boundary crisis, mechanical solidarity, and repressive justice.

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TYPOLOGICAL INVALIDITY IN "GENERATION AND FAMILY EFFECTS IN VALUE SOCIALIZATION" *

(COMMENT ON BENGTON,
ASR JUNE, 1975)

Bengtson's (1975) paper was an assessment of the relative effect of generation and family upon the individual's value orientation. Since Bengtson was particularly concerned over adequate operationalization of the concept "value," he first constructed a two-dimensional value-orientation typology (adapted from that of Fallding, 1965) and performed validation procedures upon it using factor analysis. He then constructed factor scales for the purpose of operationalizing the value dimensions and used those scales as dependent variables in his analysis of the effects of generation and family. Unfortunately, Bengtson erred at the early stage of typology validation. He interpreted the factor analysis as having reproduced his typology but, in fact, only one of the two dimensions obtainable from the factor analysis corresponds to those in Bengtson's postulated typology.

The two dimensions of Bengtson's postulated typology were Expansion-Restriction and Egoistic-Collectivity; the four value types were humanism, materialism, collectivism and individualism. This typology is portrayed graphically in a two-dimensional attribute space in Figure 1. Each solid-line axis in the figure represents one of the typological dimensions (dichotomized at the origin). The circles, representing the four different value orientations as measured by clusters of items, have positions along the axes corresponding to the positions of the value orientations relative to typological dimensions (see Lazarsfeld, 1937: 126-7 regarding the characterization of types by position on coordinate axes). Note that since the value types here are inherently two-dimensional, the items measuring them are also inherently two-dimensional.

* I would like to thank Kenneth D. Bailey and anonymous ASR referees for helpful comments on earlier drafts of this paper.

Bengtson attempted to validate his typology through principal components factor analysis. A validating factor analysis would have been one which reproduced the Egoistic-Collectivity and Expansion-Restriction dimensions of the typology as factors, with the value types in the predicted positions relative to those factors. However, instead of the expected Egoistic-Collectivity and Expansion-Restriction factors, factors named "Humanism/Materialism" and "Collectivism/Individualism" appeared in Bengtson's (1975: 361) Table 1. Nor was this a mere question of labeling; Bengtson's obtained factors simply were not his postulated typology dimensions (although he termed one of them a "postulated" axis).

Part of Bengtson's problem was the use of a simple-structure factor rotation method (varimax). A simple-structure rotation (Harman, 1967) is one which minimizes the number of variables loading highly on more than one factor. It produces a set of factors such that as many variables as possible load only on single factors. The variables are then unidimensional with respect to the factors produced. The contrasting type of structure shown in Figure 1 (solid axes), where the number of variables loading highly on every factor is maximized, may be termed a complex structure. Complex-structure rotation is the appropriate final solution when the goal is to reproduce a typology of non-unidimensional constructs, as in the present case where each value orientation (and the items operationalizing it) is thought to have a non-zero position along both of two dimensions. In the case of two orthogonal axes, a complex structure and a simple structure are merely forty-five degree angle rotations of each other. If a forty-five degree angle rotation were performed on the hypothetical items in Figure 1, the dotted lines would become the axes. We would, through such a rotation, lose the typology, since the axes would no longer correspond to the Expansion-Restriction and Egoistic-Collectivity dimensions. When Bengtson performed simple-structure rotation, he perforce obtained axes passing through item clusters (whose names he therefore gave to the obtained dimensions). In addition, his factor scales operationalized dimensions corresponding to the simple-structure factors rather than typology dimensions corresponding to complex-structure factors.

If the choice of rotation method were all that was involved in the failure to reproduce the typology, Bengtson's factor solution would correspond to the forty-five degree angle rotation of his typology indicated in Figure 1 (dot-

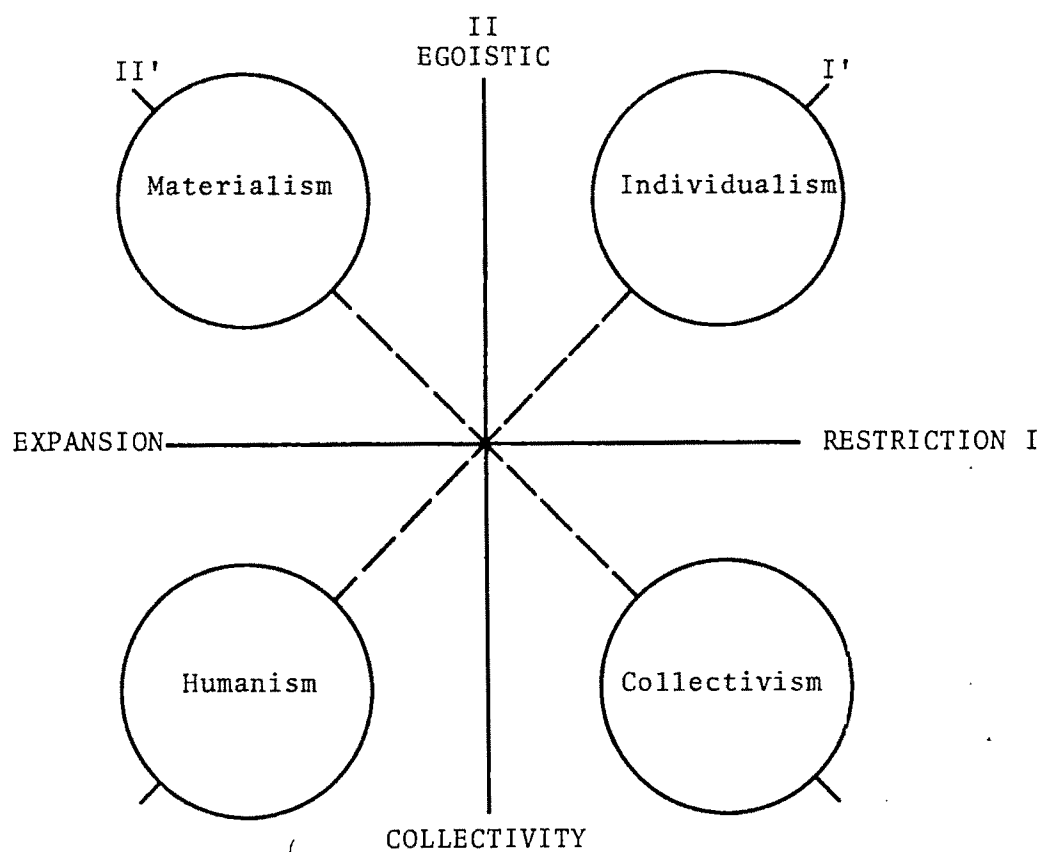


Figure 1. Bengtson's Typology in Two-Dimensional Space (Axes I and II) and Simple-Structure Rotation (Axes I' and II'). Derived from Bengtson's Figure 1.

Table 1. Simple-Structure and Complex-Structure Loadings of the Value Items on Two Factors

Value	Variable	Bengtson's Simple-Structure Loadings		Complex-Structure Loadings	
		Factor I	Factor II	Factor I	Factor II
Humanism	(a) Service	-.59	-.20	-.28	-.56
	(b) Equality	-.52	-.07	-.32	-.42
	(c) A world at peace	-.45	-.22	-.16	-.47
	(d) Ethical life	-.43	-.16	-.19	-.42
Materialism	(a) Financial comfort	.76	-.06	.58	.49
	(b) Possessions	.58	.09	.35	.47
	(c) Attractive appearance	.38	.20	.13	.41
	(d) Respect or recognition	.27	-.02	.20	.18
Collectivism	(a) Religious participation	-.15	-.54	.28	-.49
	(b) Loyalty	.09	-.51	.42	-.30
	(c) Patriotism	-.17	-.46	.20	-.44
	(d) Friendship	-.12	-.15	.02	-.19
Individualism	(a) Skill	.18	.54	-.25	.51
	(b) An exciting life	.19	.54	-.25	.52
	(c) Personal freedom	-.05	.52	-.40	.33
	(d) Sense of accomplishment	.15	.26	-.08	.29

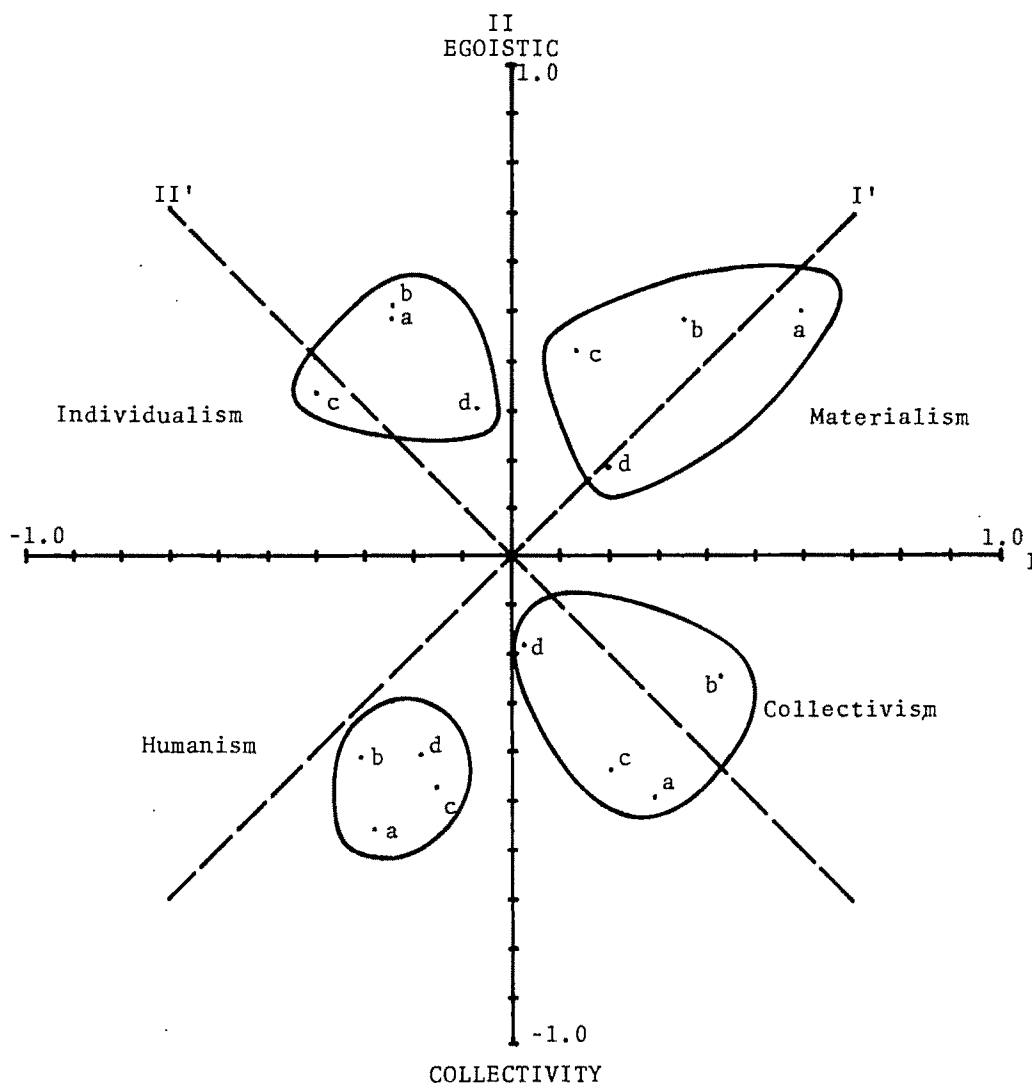


Figure 2. Bengtson's Simple-Structure Rotation (Axes I' II') and the Complex-Structure Rotation (Axes I and II). (The letters correspond to item letters in Table 1.)

ted lines). But this is not the case. The position of individualism and the position of materialism in Bengtson's typology are exchanged in his factor solution.

Figure 2 (dotted-line axes) gives a graphic representation of Bengtson's factor solution. The coordinates of an item with respect to the dotted-line axes are its simple structure loadings¹ (see Table 1). The reversal of indi-

vidualism and materialism in Bengtson's solution can be seen by comparing Figure 2 (dotted-line axes) to Figure 1 (dotted-line axes). The conflict between the postulated typology and the obtained solution is even clearer in the complex-structure rotation (Figure 2, solid axes). Item coordinates are now the complex-structure loadings, which were computed from Bengtson's two-decimal simple-structure loadings² (see Table 1). A

¹ In an orthogonal two-dimensional principal components solution, each item can be expressed as a linear combination of the two factors:

$$x_i = a_{i1}F_1 + a_{i2}F_2.$$

Each item therefore can be represented in two-dimensional space as a point with coordinates (a_{i1} , a_{i2}) (see Harman, 1967).

² The matrix formula for a forty-five degree angle clockwise rotation of two factors is as follows:

$$F' = F \times \begin{pmatrix} \sqrt{2}/2 & \sqrt{2}/2 \\ -\sqrt{2}/2 & \sqrt{2}/2 \end{pmatrix}$$

comparison of Figure 2 (solid axes) with Figure 1 (solid axes) shows that rather than confirming *in toto* Bengtson's postulated typology, the factor analysis contradicts it. The items cluster approximately as intended, and Factor II does seem to have reproduced the hypothesized Egoistic-Collectivity dimension (individualism and materialism versus humanism and collectivism). However, on Factor I both materialism and collectivism are positive, while both individualism and humanism are negative. If Factor I were the postulated Expansion-Restriction dimension (see Figure 1), it would contrast humanism and materialism against collectivism and individualism, rather than humanism and individualism against collectivism and materialism as it does now. Therefore, the complex-structure Factor I is some dimension other than the postulated Expansion-Restriction dimension. However, instead of attempting to identify this new factor, which explains only about 8.4% of the variance as compared with Factor II's 17.7%, one should probably reject the two-factor solution for a single-factor solution.

Does the failure to produce the postulated Expansion-Restriction dimension mean that Fallding's (1965) typology from which Bengtson's was derived was incorrect? A reading of Fallding's article suggests, rather, that Bengtson's value items do not accurately reflect Fallding's types. Fallding's Expansion/Collectivity cell was called "membership" rather than "humanism" (see Figure 1) and entailed belonging to as much as possible. But Bengtson's items here tap only the humanistic life orientation (see Table 1). Fallding's Expansion/Egoistic cell was called "ownership" rather than "materialism" and explicitly included achievement, not only material goods and attributes as suggested by Bengtson's items. In fact, Bengtson's achievement-oriented "individualism" items belong in this cell of Fallding's typology, and not in the Restriction-Egoistic cell. The latter cell in Fallding's typology was called "interest" and related only to narrow absorbing interests such as a hobby or sport. Bengtson has no items clearly appropriate for this cell. Finally, Fallding's Restriction/Collectivity cell was called "partisanship" rather than "collectivism," and related to valuing one's own group, religious sect, etc., in opposition to other groups. Only Bengtson's "loyalty to your own" item clearly fits here, since religious participation, patriotism and friendship do not necessarily imply exclusivism but can

also be membership values. Of the items in Bengtson's two Restriction cells (individualism and collectivism), only this single item ("loyalty to your own") has clear restrictive value by Fallding's definition. It is thus not surprising that Fallding's Expansion-Restriction dimension was not obtained from the factor analysis. It has been shown that Bengtson failed to validate his own typology, but we cannot say that Fallding's typology has been invalidated; it has not been tested.

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REPLY TO McCONAGHY

It is apparent that many problems in social science research can be traced to insufficient concern for construct definition prior to measurement. In particular, this can be seen in studies employing the concept "value" used so frequently in sociology with such infrequent attention given to nominal definition and empirical dimensionality. McConaghy's intent is laudable because she takes seriously what so many investigators have passed over lightly: the difficulties (perhaps the mystique) of typology construction and the use of factor analysis as a tool in construct validation.

Unfortunately, McConaghy has misinterpreted the primary goal of my analysis; worse, the criteria she employs in her critique are neither as clear-cut nor as conventionally accepted as her comment assumes. There is no "one way" to construct a useful typology, nor is there any single "best solution" in defining dimensions through factor-analytic models. Earlier methodologists (she cites Lazarsfeld, 1937, and the first edition of Harman, 1967) sought to bring some order out of chaos by asserting that particular models were *the* appropriate solution to a given problem. In contrast, more recent writings (see Heiss, 1974;

where F' is the p by 2 matrix of new factor loadings, F is the p by 2 matrix of original factor loadings, and p =number of items.

Harman, 1976:4-5) point to the several strategies and options available. They emphasize the primary criterion of "meaningful interpretation" in choosing among available logical or statistical models to best address substantively important research questions.

In my study of value socialization, the primary goal was not to validate a typology but, rather, to define dimensions which would prove useful in examining cross-generational contrast or congruence. I began with a nominal definition of values ("conceptions of desirable ends which serve as guides to action") and proceeded to operationalize the construct. I wanted the measurement design to reflect my premise that, while there are a variety of discrete values, these are patterned and organized according to superordinate cognitive categories. It is important to point out the advantages of a higher-order conceptual analysis of values, in preference to the ad hoc assessment currently in vogue (e.g., Rokeach, 1968). In defining such categories, I was drawn to an earlier suggestion (untested by survey methods) of Fallding (1965) that values might be categorized along two theoretical axes: expansion-restriction and egoistic-collectivity. Their intersection led to four cells which Fallding termed ownership, membership, interest and partisanship (labels which I felt were not the best descriptors and to which I did not subsequently refer). Questionnaire items were selected on the basis of several pretests reflecting (with varying degrees of face validity) this typology.

Data from the larger study then were subjected to principal-components factor analysis with the intent of describing the most characteristic dimensions, which I labeled humanism-materialism and collectivism-individualism. These terms were applied because they more accurately reflected the item-to-factor loading configurations, and because they had greater intuitive meaning, than Fallding's original terms. In my view, the Fallding typology is suggestive rather than definitive; my concern was to use cell types as a framework for distinguishing among sets of values in testing cross-generational hypotheses. (It should be noted that the factor analysis then was repeated for the three age-groups to be compared in the substantive analysis, with similar underlying dimensional structures found to characterize each group.)

I confess I am puzzled by McConaghy's interpretation of what a correct "typology" is and her implicit criteria for judging whether a typology is good or bad. A two-dimensional bipolar "typology" strikes me as no less

"valid" than a four-cell, or four-factor characterization. The crucial issue is utility: whether a particular set of types accurately and economically reflects data in a manner that adds meaning to cross-group comparison. Similarly, I am puzzled by her preferred-factor solution. A complex-structure rotation may be an appropriate way to validate a typology, but is certainly not useful in describing underlying dimensions with a view toward the most meaningful interpretation of data. This was my major intent in performing the analysis—to define dimensions amenable to testing hypotheses concerning within-lineage and between-generation contrasts in value orientations. Whether the dimensions employed exactly reflect what Fallding had in mind may be, as she suggests, open to question; but, in any case, this issue seems quite incidental to the analysis reported in my paper concerning cross-generational transmission of values.

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ANOTHER LOOK AT MODERNITY SCALES: REANALYSIS OF THE CONVERGENT AND DISCRIMINANT VALIDITIES OF THE ARMER, KAHL, SMITH AND INKELES, AND SCHNAIBERG SCALES *

(COMMENT ON ARMER AND SCHNAIBERG, ASR JUNE, 1972)

Armer and Schnaiberg (1972) have reported that the scales designed by Smith and Inkeles,

* We are indebted to Joan Cohen and Carolyn Mullins for their editorial help.

Kahl, Schnaiberg, and Armer for measuring individual modernity are only moderate in their degree of equivalence and inadequate in their discriminant validity. Although the moderate raw intercorrelations between the four scales became considerably higher when corrected for attenuation, the authors showed even the corrected intercorrelations to be low relative to the discriminant validity coefficients relating the four modernity scales to anomia, alienation and socioeconomic status. While Armer and Schnaiberg (1972:315) stopped short of advocating the abandonment of the psychological modernity concept, they suggested that it might be a "myth." The apparent failure of the discipline's efforts to measure modernity lends credence to the charge that the concept is unmeasurable in principle. Furthermore, the concept holds little present value for empirical science if it cannot be measured on a valid instrument, and the Armer-Schnaiberg (1972) analysis apparently eliminated the most carefully developed and widely-used modernity scales available.

Fortunately, however, the need to repudiate all of the scales (with pessimistic implications for the entire modernity concept) is more apparent than real. Since the inadequacy of the scales' discriminant validities was based on a comparison to their *convergent* validities, at least part of the burden of failure can be attributed to moderate convergence. Since the degree of convergent validity reflects in part the *group* of scales selected, it is possible that one scale is greatly reducing general convergence, while the others are much higher in this regard.

Convergent Validity

Examination of the four scales tested shows that one is different in content from the others. Armer and Schnaiberg (1972) did not use Schnaiberg's (1968; 1970) full 46-item summary scale of overall modernity, but only the 24 items in the Schnaiberg emancipation scale, a subportion of the original 46. Whereas Schnaiberg's (1968) original 46 items measured six different aspects of modernity, over two-thirds (18) of the "Emancipation Scale" items deal with attitudes toward women's equality, and almost all of the rest (5) are measures of mass media exposure. One cannot consider this two-component scale sufficiently broad to encompass a many-faceted concept like modernity, which is generally agreed to contain a half-dozen or so components, including orientations toward change and custom, rationality and planning, work, and independence from one's family. These dimensions, omitted from the emancipation scale, have been found empirically, both by Kahl (1968)

and by Armer and Ycutz (1971), to be central to individual modernity.

Because emancipation is only one aspect of individual modernity and not the same as overall modernity,¹ an emancipation scale can be substituted with confidence for a modernity scale only when the two have been shown equivalent for the purposes at hand. Since the Schnaiberg emancipation scale was not designed originally to measure overall modernity, Schnaiberg (1968), in analyzing the modernity of Turkish women, proceeded cautiously before he substituted his emancipation scale for his modernity scale. He showed that emancipation was a good indicator of modernity in his Turkish sample, and that since the effects of emancipation on demographic variables were roughly equal to the effects of modernity (Schnaiberg, 1968:276), emancipation could be substituted for modernity for purposes of demographic analysis. In contrast, Armer and Schnaiberg (1972) took no such precautions. They presented no evidence that emancipation was a good indicator of modernity in their all-male Chicago sample and no evidence that emancipation and modernity correlated similarly with the Kahl, Armer, and Smith and Inkeles scales. Even more important, whereas Schnaiberg (1968:298) found emancipation to be at the core of modernity among Turkish women, there is no evidence that emancipation is at the core of modernity for Chicago males.² It is clear that emancipation is not in all cases the core of modernism. Schnaiberg (1968:314) warned that emancipation might be central to modernism only in Muslim countries, where women's exposure to modern society hinged strongly on freedom from their husbands' traditional domination and regulation. And this warning, it turns out, was quite justified: Kahl (1968) has reported that his family modernism dimension, which included husband-wife egalitarianism, was *not* a part of the core of either Brazilian or Mexican modernity, and, likewise, Armer and Youtz (1971) found that their scale of three women's equality items was not

¹ The subsets of this scale are conceptualized as measures of different aspects of modernity, *not* as separate measures of overall modernity. For a contrast, see Campbell and Fiske's (1959:87) example of the George Washington Social Intelligence Test.

² The uptown respondents were all male: whereas Schnaiberg's (1968) female respondents answered six women's equality items as behavioral reports about how much freedom their husbands allowed them, the uptown respondents only gave opinions (on these items) on what women's rights should be. Therefore, the importance of the emancipation items for the uptown respondents was in all probability less than for the Turkish respondents.

Table 1. Intercorrelations^a among Modernity Scales Uncorrected (above the Slashes) and Corrected for Attenuation Using Cronbach's Alpha (below the Slashes)^b

Modernity Scales Uptown, Wave 1	Kahl	Schnaiberg	Armer
Smith and Inkeles	.54/.77	.45/.64	.57/.95
Kahl	—	.40/.57	.64/.97
Schnaiberg	—	—	.41/.68

^a Pearson correlation coefficients are employed throughout the entire analysis.

^b All values are Armer and Schnaiberg's (1972) computations.

closely correlated with the prime dimension of Nigerian modernity. Hence, the relationship between emancipation and modernity for Chicago uptown males was, and remains an open question, and Armer and Schnaiberg (1972) were skating on thin ice in using the emancipation scale as a modernity scale. Since the emancipation scale's face validity as a modernity scale is in doubt, it would be no surprise if its convergent validity were low, indeed much lower than for the other scales.

Secondary analysis of the first wave of Armer and Schnaiberg's (1972) data shows this to be the case, for the Schnaiberg scale's convergent validity is lowest by far among the four scales tested. Wave 1 correlations of the Schnaiberg scale with the other three averaged only .41, while intercorrelations among the other three scales averaged .58 (see Table 1). After correction of all coefficients for attenuation, Wave 1 correlations of the Schnaiberg scale with the other three scales averaged .63 and intercorrelations among the other scales averaged .90. Since even after correction for attenuation the "true score" of the Schnaiberg scale explains less than 40 percent of the variance in the other scales, it does measure something different from the other three scales.

The other three scales show a much higher degree of equivalence among themselves. After correction for attenuation, the "true" score of any one (on average) accounts for about 80 percent of the variance in any other, a figure double the average for the Schnaiberg scale, and fairly high. Dropping the Schnaiberg scale and its low intercorrelations with the others raises the "convergent validity coefficients" of the three remaining scales, since these latter coefficients are simply the average correlation of each scale with the others. Table 2 presents the "convergent validity coefficients" of the Smith and Inkeles, Kahl, and Armer scales, both as computed originally by

Armer and Schnaiberg using all four scales and as recomputed *without* taking the Schnaiberg scale into account. The coefficients based on only three scales are larger in every case than those based on all four scales. Uncorrected coefficients remain moderate in size. However, this moderation is primarily due to moderate reliabilities,³ and is not a validity problem. Tests of convergent validity most appropriately examine correlations between *what is being measured* by each instrument, and such "true score" correlations are given by correcting the raw coefficients for attenuation. After correction for attenuation, three-scale convergent validity coefficients based on correlations between "true scores" (Table 2, column 2) average .90, and the .96 figure for the Armer scale approaches 1.0. Despite "prima facie differences in content" (Armer and Schnaiberg, 1972:302), the degree of equivalence in what these scales measure is quite high.

Although Armer and Schnaiberg (1972:308) found "far from perfect equivalence" when they treated all four scales as a group, one must reject the implication that *all four* scales are only moderately equivalent: the Schnaiberg emancipation scale was inappropriately included as a modernity scale and reduced average convergence. Although Campbell and Fiske (1959) note that validity is greatest where different approaches converge, they state that each independent method used to measure a trait should be "appropriate to the trait as conceptualized" (Campbell and Fiske, 1959:103).⁴ We take this to mean that an emancipation scale should not be used in a multimethod analysis of modernity scale validity; and once the Schnaiberg scale is dropped, the three scales designed to measure overall modernity show a rather high degree of equivalence.

³ It should be noted that Armer and Schnaiberg's (1972) assessments of scale reliability are probably underestimates. Heise (1969) showed that test-retest correlations are lowered by the instability of "true scores" as well as by unreliability, and Lord and Novick (1968) have stated that Cronbach's alpha is a *lower bound* for reliability, not an unbiased estimate of it. Furthermore, measured reliability could be depressed by the lack of a probability sample. According to Campbell and Fiske (1959:102), "the curtailment of the sample . . . will depress the reliability coefficients. . . ."

⁴ "Although this view will reduce the range of desirable methods," it is rarely too restrictive, for "agreement between several methods is desirable" and "convergence between two is a satisfactory minimal requirement" (Campbell and Fiske, 1959:103).

Table 2. Convergent and Discriminant Validity Coefficients for Three Modernity Scales, Wave 1, Uncorrected (above the Slashes) and Corrected for Attenuation (below the Slashes)

	Convergence		Discriminance		
	Based on 4 Scales	Based on 3 Scales	Anomia	SES	Alienation
Smith and Inkeles	.52/.79	.56/.86	-.44/-.67	.48/.81	-.49/-.86
Kahl	.53/.77	.59/.87	-.60/-.85	.34/.53	-.62/-.90
Armer	.54/.87	.61/.96	-.52/-.84	.39/.71	-.39/-.67

Discriminant Validities

Although the absolute values of the convergent validity coefficients (analyzed above) are of considerable importance, the adequacy of the scales' convergent validities also must be judged relative to their discriminant validities (see Campbell and Fiske, 1959). Since Armer and Schnaiberg (1972) found the scales' convergence lower than their correlations with anomia, socioeconomic status and alienation, they concluded that none of the scales could discriminate successfully between modernity and the latter three variables. In order to modify that judgment, we have tested whether elimination of the Schnaiberg scale increases convergence to levels higher than the discriminant validity coefficients.

Armer and Schnaiberg's (1972) comparisons of convergent and discriminant validities were repeated with the Schnaiberg scale eliminated. Three-scale convergent validity coefficients were compared to discriminant validity coefficients which correlate each scale with anomia, socioeconomic status and alienation. Even with the Schnaiberg scale eliminated, the Smith and Inkeles scale did not discriminate adequately between modernity and alienation, and the Kahl scale did not acceptably discriminate modernity from either anomia or alienation (see Table 2). However, the Armer scale discriminated adequately between modernity and all three variables, since its .61 convergent validity coefficient was greater than all three uncorrected discriminant validity coefficients (for anomia $p < .10$, and for the other two $p < .05$ in one-tailed t-tests of the type described by Walker and Lev, 1953:257). Its discriminative ability also was adequate in each case when the coefficients were corrected for attenuation ($p < .05$ in all three comparisons).⁵ Contrary to

Armer and Schnaiberg's conclusions, the Armer scale does meet Campbell and Fiske's (1959) test for discriminant validity.

While the scales are still not impressive in discriminant validity when considered together as a group, it is equally appropriate in the present case to consider the results for each scale individually⁶ for several reasons. (1) The present set of scales was not chosen according to any systematic procedure (none was reported) and, thus, do not in any sense comprise a "sample" of scales. (2) Each scale employed generates its own unique discriminant validity coefficient, and in the present case the different scales fare differently in their discriminant validity comparisons. Indeed, there are such important differences in discriminant validity between the scales that an "average" would not tell the whole story. (3) Most important, only a single valid instrument is needed to refute Armer and Schnaiberg's (1972) suggestion that the modernity concept may be too imprecise

validities *vis-à-vis* alienation. This comparison has been run utilizing the raw and corrected Wave 2 scale intercorrelations to compute three-scale convergent validity coefficients for Wave 2. However, the Wave 2 scale intercorrelations are unpublished, and the present report is limited to the reanalysis of published data.

While Althaus and Heberlein (1970) have noted that convergent validity coefficients can be elevated by methods effects, in this case discriminant validity coefficients are based on scale intercorrelations, too, and should be elevated to a similar degree. Thus, the monomethod effect for scales should give neither convergent nor discriminant coefficients a substantial advantage in these comparisons. Armer and Schnaiberg's (1972) use of a non-acquiescent subsample does not resolve this issue. It seems suspect because of the possibility of unknown biases in a select subsample. Further, their selection of a subsample non-acquiescent on the Kahl scale does not guarantee a subsample non-acquiescent on the Armer scale.

⁶ Campbell and Fiske (1959) prescribed no single procedure for interpreting tests of discriminant validity, nor have recent discussions (e.g., Althaus and Heberlein, 1970; Althaus et al., 1971) dealt with this matter.

⁵ Since alienation was measured only in the second wave of Armer and Schnaiberg's panel data, discriminant validities *vis-à-vis* alienation are from Wave 2, while convergent validities are from Wave 1. More precise is the comparison of Wave 2 convergent validity coefficients with the discriminant va-

cise to be distinguished from other, related variables.⁷ If psychological modernity were empirically indistinguishable from alienation, anomia and socioeconomic status, the Armer scale could not have distinguished them as it has: the discriminant validity of the Armer scale shows that there is a psychological modernity phenomenon that is distinct from socioeconomic status, anomia and alienation.⁸

The failure of both the Kahl scale and the Smith and Inkeles OM-6 to discriminate modernity from related phenomena indicates serious defects in these two leading scales, but the defects are readily attributable to their questionnaire designs, not to the modernity concept. The Kahl scale's problems apparently stem from the way alienation and anomia creep into many of its modernity items.⁹ "Activism" and "planning" can be denied by respondents only by acknowledging "futility" and "despair"; modern stratification and urban life can be rejected only on grounds that a few wealthy persons control everything and that the city is an unfriendly place; family ties can be embraced (on one item) only through the assertion that strangers are untrustworthy; and mass-media participation is measured as low (on one item) if political problems are rarely discussed "with your friends." Likewise, five items of the sixteen in the OM-6, while asking about certain aspects of modernity, also happen to correspond to items in Middleton's (1963) alienation scale, the measure of alienation employed by Armer and Schnaiberg (1972). In each case, non-modern responses on the OM-6 correspond to specific dimensions of alienation: few organizational memberships indicate "social estrangement"; never wanting to do anything about public issues closely resembles Middleton's (1963:973) "powerlessness"; inability to understand the thinking of a man from a different country is worded like

Middleton's (1963:973) "meaninglessness"; and both the listing of few national problems and the nonreadership of newspapers show "cultural estrangement" (Middleton, 1963:974). Although both the Kahl and Smith-Inkeles scales require respondents who wish to place themselves at a distance from modern institutions to report that they are alienated from all institutions,¹⁰ this double-barreled approach is quite unnecessary. The Armer scale's simple "I prefer to plan ahead on (few/most) matters,"¹¹ and the Smith-Inkeles items "Would you give most weight to the advice of church or government"; "What news interests you most (world/nation/village/sports/religion)"; and "Would you prefer (rural/urban) life" (not on the OM-6) show by example that items measuring modernity without asking about alienation or anomie are easy to institute. With better questions, there would seem to be no reason for these scales to discriminate poorly, since our figures for the Armer scale show that discriminance has been and can be achieved.

Conclusion

Armer and Schnaiberg's (1972) conclusions called the very concept of psychological modernity into question: if sociologists agreed only moderately on what it was and if none could distinguish it from alienation and anomie, the concept might be so imprecise that no valid measuring device could, in principle, ever be devised. The present findings serve to remove these doubts. Since three different sociologists have operationalized modernity in highly convergent ways, there is indeed a shared content to the modernity concept. Even the more serious discriminance problem is readily surmountable: sociologists can measure modernity in accord with the Campbell-

⁷ Armer and Schnaiberg (1972) seem to concur on this point. They (1972:303) state that the meaningfulness of the individual modernity concept is called into doubt if evidence of validity "is lacking for *all* scales" (emphasis added).

⁸ This conclusion is *not* subject to the qualifications suggested by Althauser and Heberlein (1970) and Althauser et al. (1971). Since only one method (questionnaire) is used for all measures, the comparisons used here to determine discriminant validity are not like any of the three types of comparison they found inconclusive.

⁹ Final determination of an item's discriminative power depends on its correlations with other modernity items and with the variables of alienation and anomie, but it is at least possible here to show which items build anomia and alienation into their very design.

¹⁰ In Chicago, a modern city, where modern culture dominates, and in the uptown area especially, which features no strong traditional communities, the rejection of modernity will certainly be more associated with alienation than in a society where tradition predominates. However, even in Chicago, it is possible to measure modernity and not alienation. The majority of the items on the short OM-6 do not confound alienation and modernity so obviously; there are 119 modernity items to choose from on Smith and Inkeles' Long Form, many of which seem alienation-free; and, of course, there is the Armer scale.

¹¹ We do not claim that *all* items and/or subscales of the Armer scale discriminate well (or as well as the scale as a whole). Armer and Schnaiberg's (1972:313) analysis of the Armer scale's subscales indicates otherwise.

Fiske canons if they are careful to choose the Armer scale. Its convergent and discriminant validity have passed and may be considered adequate. Although the Armer scale has not been validated once and for all,¹² its validity has been supported,¹³ not disconfirmed as Armer and Schnaiberg reported originally.

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REPLY TO COHEN AND TILL *

Since publishing our analysis of the measurement validity of individual modernity scales, advances have been made in methods for investigating discriminant validity (Althauser 1974; Alwin 1974). While the type of data required for multitrait-multimethod analysis is unavailable in our study, it is possible nonetheless to phrase questions of convergent and discriminant validity using related techniques. Particularly important in

* We wish to thank Duane F. Alwin for helpful consultation on structural equation methods, LISREL computer program use and earlier drafts of this statement.

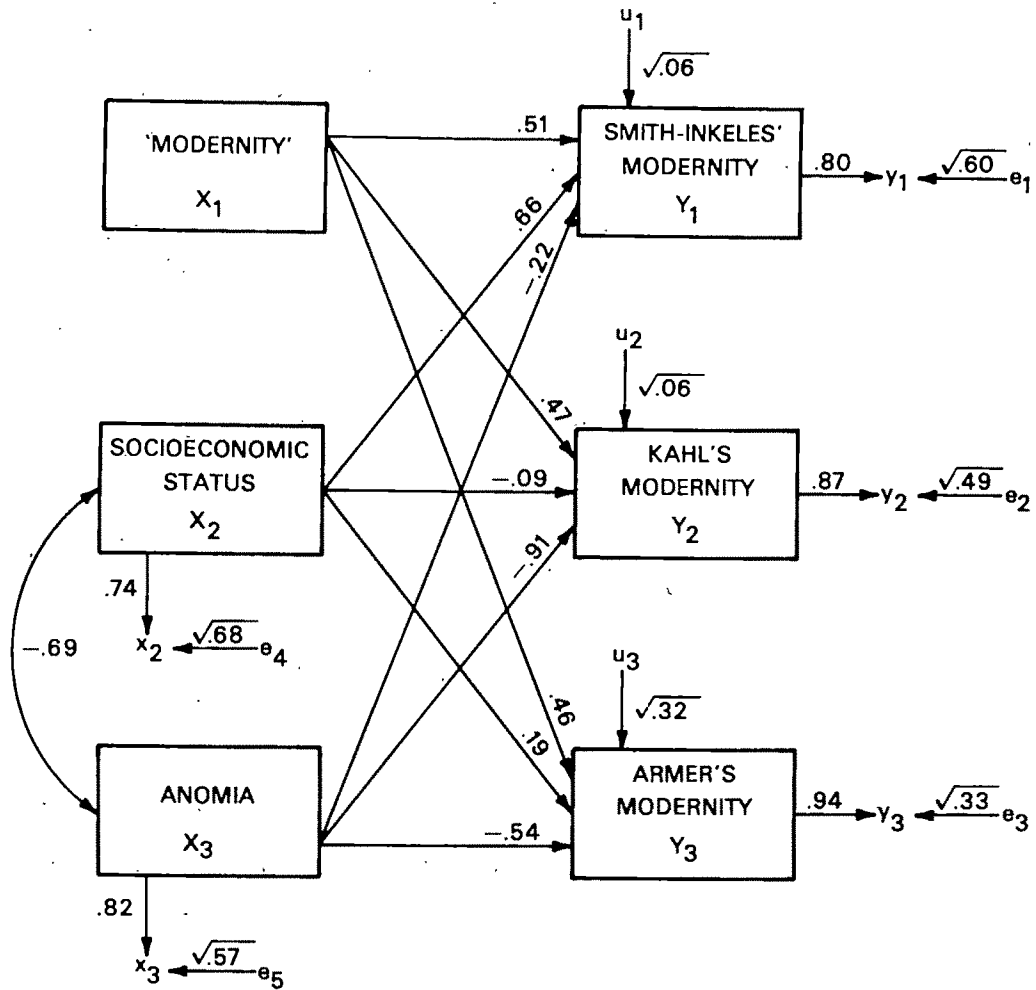


Figure 1. Path Diagram for Discriminant Validity Analysis of Modernity

this regard is the possibility of specifying and estimating a structural equation model for the correlations in our original study. Since the model implicit in our study is repeated in Cohen and Till's reanalysis, specification and testing of the model with the same data provides a more informative analysis of discriminant validity of the modernity scales, and a more constructive answer to Cohen and Till, than would a review of limitations in their evidence and inferences.

The path diagram underlying our original analysis is presented in Figure 1. The model employs three exogenous factors or traits (X_1). Two are measured constructs of socioeconomic status and anomia; the third is unobserved and is labeled "modernity." The model posits that the construct of individual modernity (X_1) is distinct and uncorrelated

at the conceptual level with the constructs of socioeconomic status (X_2) and anomia (X_3). The traits of socioeconomic status and anomia are allowed to be theoretically correlated without specifying causation between them. There are three endogenous variables (Y_1) corresponding to "true" or reliable variance in the endogenous measures (y_1) of Smith and Inkeles' "psychosocial modernity," Kahl's "modernism" and Armer's "individual modernity." Schnaiberg's modernity scale is not included in the model in order to conform to Cohen and Till's analysis even though we do not agree with their arguments for excluding it nor with their *post factum* approach to hypothesis testing.¹

¹ All of the modernity scales initially grew out of other theoretical concepts and endeavors; e.g.,

Table 1. Correlations, Means, Standard Deviations and Reliabilities for all Variables: Uptown, 1970, Wave 1 (N=156)

Variable	y_1	y_2	y_3	x_2	\bar{X}	s.d.	alpha
Inkeles-Smith's	y_1				9.308	2.617	.644
Kahl's	y_2	.540			18.217	3.636	.756
Armer's	y_3	.570	.637		33.751	3.510	.563
SES	x_2	.477	.340	.390	1.365	1.072	.540
Anomia	x_3	-.442	-.600	-.520	2.679	1.582	.672

The model states that nonrandom variation in each endogenous modernity variable (Y_i) is due to the three sources of exogenous variation (X_i) and to a unique random source of variation (u_i). The variation in observed measures of modernity (y_i) are due to "true" variation in the particular modernity variable (Y_i) and to unique unreliability or "error" variation (e_i). The structural equation framework allows one to take into account "corrections for attenuation" (in conformity with Cohen and Till's analysis) by separating the issues of random and nonrandom error and to include them as unique sources of variation.² Moreover, it allows the comparison of nonrandom variation in "modernity" scores including specific variation unique to a particular endogenous variable and variation due to the exogenous traits or factors.

The structural equations for the model in Figure 1 are as follows:

$$\begin{aligned} Y_1 &= \beta_1 X_1 + \beta_2 X_2 + \beta_3 X_3 + u_1 \\ Y_2 &= \beta_4 X_1 + \beta_5 X_2 + \beta_6 X_3 + u_2 \\ Y_3 &= \beta_7 X_1 + \beta_8 X_2 + \beta_9 X_3 + u_3 \end{aligned}$$

$$y_1 = Y_1 + e_1$$

$$y_2 = Y_2 + e_2$$

$$y_3 = Y_3 + e_3$$

$$x_2 = X_2 + e_4$$

$$x_3 = X_3 + e_5$$

Note that the equations for y_1 through x_3 conform to classical true score measurement models. Consistent with the diagram, the disturbance terms (u_i and e_i) are specified to be uncorrelated with the X_i , Y_i and y_i terms and with each other.³ A correlation matrix, standard deviation and reliability coefficients or the measured variables are provided in Table 1 for anyone wishing to check our results or estimate different models.

The estimated model in standard form is prescribed in Figure 1. A test for the goodness of fit of the model to the data is $\chi^2 = 0.000$ with 1 degree of freedom, $p = .998$. The largest single determinant of variation in Smith-Inkeles' modernity (Y_1) is socioeconomic status ($p = .66$) while the largest determinant of Kahl's and Armer's modernity is anomia ($p = -.91$ and $-.54$). The effect of the "modernity" factor is $p = .51$, $.47$ and $.46$, respectively.

The percentage of variance explained by valid and invalid factors is summarized in Table 2. Almost 94 percent of the total variance in Smith-Inkeles' modernity is accounted for by the three exogenous variables ($67.9 + 25.9 = 93.7$), and 72.4 percent of this is due to the invalid separate and joint effects of socioeconomic status and anomia. The same percent of total variance is explained by exogenous variables in Kahl's modernity ($71.3 + 22.4 = 93.7$), and 76.1 percent of this is due to socioeconomic status and anomia in-

Inkeles' (1960) industrial man, Kahl's (1965) achievement values, Armer's (1970) westernism, Doob's (1960) civilized man all initially grew out of research on other populations, and all have varying kinds and amounts of emphases on modernity themes. Women's rights, equalitarian marriage, family planning, etc., continue to be viewed by Inkeles and Smith (1974) and other modernity theorists as important components of individual modernity. Schnaiberg's scale cannot be selectively dismissed on any of these grounds.

² Internal consistency estimates of reliability, e.g., Kuder-Richardson KR-20 and Cronbach's alpha, are lower-bound estimates, and will be unbiased estimates of reliability only under certain conditions (see Lord and Novick, 1968:87-95). Therefore, if reliability is underestimated, any correction for attenuation will overestimate true correlations between variables (Bohrnstedt, 1970:89). In the present data, overestimation of error (e_1 — e_5) variances would produce bias in the underlying relationships. The extent of this possible bias in our analysis or in Cohen and Till's is essentially unknown.

³ In solving the set of equations we have taken random measurement error into account and have fixed the variances of e_1 through e_5 , given the knowledge of the reliabilities of the scales, and we have fixed the variances of the exogenous variables, X_1 and X_3 , as well. The model was estimated by maximum-likelihood methods under these constraints using the LISREL computer program (Jöreskog and van Thillo, 1973).

Table 2. Results of Path Analysis of Modernity Scales

Modernity Scale	% Contributions to True Variance of the Scale			% Contributions to Explained True Variance of the Scale	
	Invalidity Factors ^a	"Modernity"	Unexplained Variance	Invalidity Factors ^a	"Modernity"
Smith-Inkeles'	67.9	25.9	6.3	72.4	27.6
Kahl's	71.3	22.4	6.3	76.1	23.9
Armer's	47.0	21.5	31.5	68.6	31.4

^a Defined as socioeconomic status (X_2) and anomia (X_3) effects combined.

validity. For Armer's modernity variation on which Cohen and Till rest their defense of the concept and measurement of modernity, 68.5 percent of the variance is accounted for by the exogenous factors and more than two-thirds (68.6 percent) of this is due to socioeconomic status and anomia. Thus, while the "modernity" factor has a sizable effect on each of the three modernity measures, it accounts for only one-quarter of the total true variance in any of them; three-quarters ($67.9+6.3=74.2$) of Smith-Inkeles' true variance to 78.5 percent ($47.0+31.5$) of Armer's true variance are attributable to other nonrandom traits (X_1 and u_1).

Moreover, questions need to be raised about the conceptual identification of the X_1 exogenous factor. We have called it "modernity" simply because that is what the scale authors named the content of domain they thought they were measuring. After removing the observed socioeconomic status and anomia sources of variation in the measures, the remaining source of shared variation may be some other invalid trait or combination of traits not included as explicit exogenous factors in the model. Alienation has been identified as a probable source of variation in modernity scales in our original study and in Cohen and Till's reanalysis. Other factors with which modernity may be contaminated or confused include intelligence, social desirability or acquiescent response sets, authoritarianism, conservatism, etc. In other words, we would caution against rushing to the conclusion that factor X_2 is a new, distinct construct which can be confidently identified as "modernity" rather than one or more constructs already well-grounded in research. Further models incorporating measures of other constructs which may be producing variation in modernity scores need to be tested. The history of scale construction is strewn with measures of constructs that existed solely in the minds of their creators. The present illustration of structural equation analysis as an approach to test-

ing discriminant validity hopefully will encourage further validity analysis of "modernity" and other constructs.

In conclusion, we note that even granting Cohen and Till their principal grounds for challenging our original conclusions (i.e., dropping Schnaiberg's scale), the present analysis replicates and strengthens our initial findings. Data have been provided above for Cohen and Till and others to check our results and to re-estimate any other model which they feel better fits modernity assumptions in order to convince themselves of our original conclusion that (1972:315):

At best, the *concept* of individual modernity may be meaningful as a distinct variable, but the *measurement* of modernity has apparently been unsuccessful. At worst, the notion of individual modernity as a distinct set of orientations may be a myth, at least for some populations.⁴

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⁴ This conclusion has been ignored by many scholars who continue efforts to measure and explain modernity on the assumption that psychological modernization plays an important role in institutional and national development. However, there is evidence (Armer and Isaac, 1976) that modern attitudes tend to have little or no influence on behaviors identified by modernity theorists as relevant for development. Thus, in addition to measurement and/or conceptual problems, the theoretical and practical significance of modernity is in doubt.

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ERRATUM

Please note the following corrections in the article "Class Segments: Agrarian Property and Political Leadership in the Capitalist Class of Chile" by Maurice Zeitlin, W. Lawrence Neuman and Richard Earl Ratcliff (*ASR* December, 1976, pp. 1006-29). Page 1017, Table 1, 43 should read

53; p. 1013, column 1, lines 7 and 8 from the bottom of the page are transposed; p. 1016, column 2, line 13 from the bottom of first full paragraph, title should read *Anales de la Republica de Chile*; pp. 1023, 1026, the texts of footnotes 13 and 14 are transposed.

ITEMS (Continued)

tions of schools, he is developing research on the effects of educational expansion on the social status of youth.

■ **ALBERT JAMES BERGESEN** (Political Witch Hunts) is Assistant Professor of Sociology at the University of Arizona. He is presently studying status protest and anti-intellectualism in the Northeast.

■ **HOWARD S. ERLANGER** (Reform Organizations and Careers) is Assistant Professor of Sociology at the University of Wisconsin, Madison. He is continuing a study of lawyers' careers and the effects of law school on students' orientation to social reform. He is also doing a series of qualitative studies on political powerlessness and juvenile gang violence.

■ **J. CRAIG JENKINS** (Insurgency of the Powerless) is Assistant Professor of Sociology at the University of Missouri, Columbia, and is also affiliated with the Center for Policy Research. His current research includes studies on oligarchy and goal transformation in voluntary associations, the links between labor market participation, the formation of ethnic communities and ethnic stratification (using recent immigration from Mexico as a case), and agrarian class structures and rural movements (in Russia). His *Farm Workers and the Powers: Insurgency and Political Conflict (1946-1975)* will be published this year. **CHARLES PERROW** is Professor of Sociology at the State University of New York, Stony Brook, and is affiliated with the Center for Policy Research. He is currently completing a study of several social movements of the 1960s. He is also studying forms of control in organizations.

■ **BARBARA LASLETT** (Social Change and the Family) is Associate Professor of Sociology at the University of Southern California. Her work concerns historical studies of change in the family, her major current project focusing on change in the family in Los Angeles from 1850 to 1900.

■ **ROBERT F. MEIER** (The Production of Conformity) is Assistant Professor in the Program in Social Ecology at the University of California, Irvine. His research interests include processes of deviance and social control and he has a continuing interest in white-collar and corporate criminality. He is co-editor, with Gilbert Geis, of *White-Collar Crime* (Free Press, 1977). **WELDON T. JOHNSON** is Assistant Professor of Sociology at the University of Wisconsin, Madison. His interests include the social psychological dimensions of deviance and social control

and an involvement in social policy research. He is currently making a study of the legal and extra-legal conditions under which persons are detected, arrested and convicted for criminal offenses.

■ **MAYNARD L. ERICKSON** (Deterrence and Perceived Certainty of Punishment) is Professor of Sociology at the University of Arizona. He is presently making a three-year study in Southern Arizona, of Measures of Delinquency and Community Tolerance. The present paper is a result of his work in deterrence research. **JACK P. GIBBS** is Professor of Sociology at the University of Arizona. His research interests include the sociology of law, social control, deviance, and human ecology. His most recent book is *Crime, Punishment, and Deterrence* (Elsevier, 1975). **GARY F. JENSEN** is Assistant Professor of Sociology at the University of Arizona. His current research is on delinquency and religion, the social meaning of sanctions, crime among American Indians, and attitudes toward crime.

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■ **ROBERT L. HAMBLIN** (Arms Races) is Professor of Sociology at the University of Arizona. He is presently director of a project on Analytical Models of Conflict, which includes interaction models as exemplified in this paper as well as diffusion, collective learning, deterrence, and innovation models of conflict. The major data set being developed for analysis involves epidemics of political violence and change in Latin America. His *Behavioral Theory in Sociology* (Transaction Books) will be published soon. **MICHAEL HOUT** is Assistant Professor of Sociology at the University of Arizona. His current research interests include socioeconomic influences on fertility within the U.S. and cross-nationally. **JERRY L. L. MILLER** is Associate Professor of Sociology at the University of Arizona. He is studying internal violence in Latin America. **BRIAN L. PITCHER** is a Ph.D. candidate in the Department of Sociology at the University of Arizona. His research interests include the cross-cultural comparison of occupational hierarchies, trends in birth cohort fertility, and models of the diffusion of collective violence.



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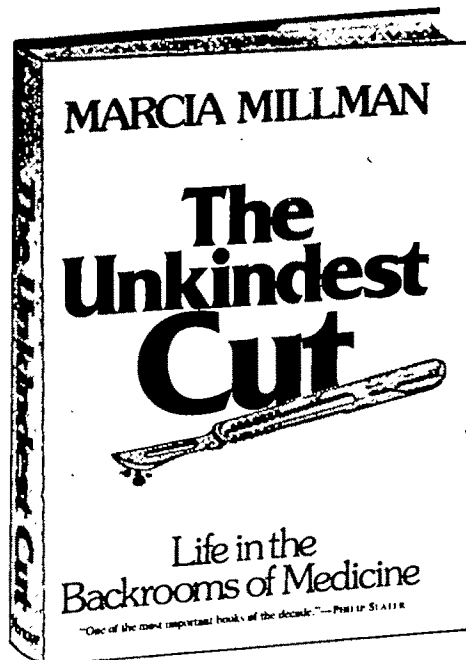
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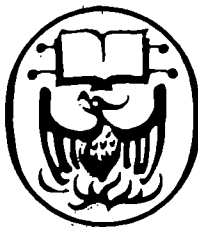
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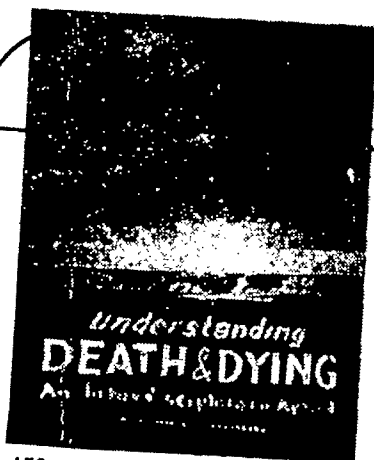
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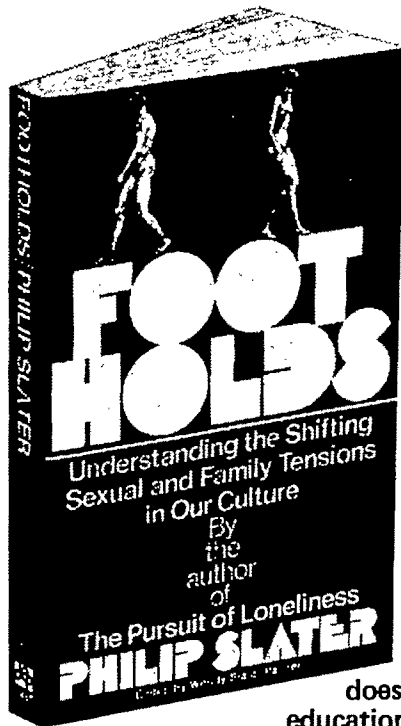
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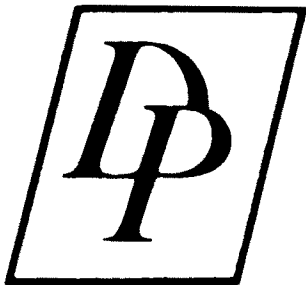
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ITEMS

June, 1977

■ VALERIE KINCADE OPPENHEIMER (Women's Economic Role in the Family) is Associate Professor of Sociology at the University of California, Los Angeles. She is presently working on a book about women's economic role in

the family, tentatively titled *Work, Sex, and the Family*.

■ HERMAN LANTZ (The Changing American Family) is Professor of Sociology at Southern

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THE SOCIOLOGY OF WOMEN'S ECONOMIC ROLE IN THE FAMILY*

VALERIE KINCADE OPPENHEIMER

University of California, Los Angeles

American Sociological Review 1977, Vol. 42 (June):387-406

This paper examines Parsons' argument that wives' socioeconomic contributions to their families are, of necessity, minor in our society and that to the extent a wife does work, family stability requires that it be at a lower occupational level than her husband's. A reassessment of the theoretical considerations involved indicates that Parsons' preoccupation with the potentiality for status competition and conflict between working spouses has led to a distorted view of the problem of status consistency within the family. The argument presented here is that, if the wife is to work, it is important that her occupation reflect favorably on the family's socioeconomic position and this need for status maintenance, if not improvement, offsets the presumed need for her occupation not to be status threatening. In addition, it is suggested that Parsons overestimated the amount of disruptive competition which would necessarily occur if both the husband and wife worked. Some empirical evidence is presented to support this alternative view of women's socioeconomic role in the family.

In spite of the rapidly increasing labor-force involvement of married women since the early 1940s, sociologists have usually assumed women's socioeconomic role in the family to be minimal. This has been particularly characteristic of stratification research which traditionally has concerned itself with measuring and studying the socioeconomic status of males alone. This approach was based on the long-accepted theory that an adult female's socioeconomic status is determined by her husband, and that a woman's major role is that of wife and mother while it is only her husband who has an important occupational role. Occupation, in turn, generally has been considered the major source of socioeconomic status in our society. Hence, by ascertaining the occupational status of a man one

could, at the same time, determine the socioeconomic status of his wife (Parsons, 1943; Centers, 1949; Turner, 1964).¹ Thus, as long as a wife's economic role in the family could be considered negligible, the conceptualization and measurement of socioeconomic status—whether of individuals or family units—had considerable theoretical simplicity.

In recent years, however, there has been a rapidly blossoming interest among stratification researchers in measuring the socioeconomic status and social mobility of women. This is partly because of a resurgence of interest in marriage as a status attainment process and partly because of a growing recognition of the status implications of the enormous increases in married women's labor-force participation. One group of researchers has been primarily interested in mobility, following very much in the footsteps of such classic studies as Blau and Duncan's *The American Occupational Structure* (1967). How-

*This is a revised version of a paper presented at the 1974 annual meetings of the American Sociological Association, Montreal, Canada, and is based on research funded by the Russell Sage Foundation. The author wishes to express her appreciation for the extremely helpful comments on earlier drafts of this paper by Joan Huber, Karen Oppenheim Mason, David Heer and Judith Blake, as well as the anonymous referees.

¹ For a critical discussion of these traditional views of women's socioeconomic role, see Day, 1961; Watson and Barth, 1964; Ackers, 1973; Haug, 1973; Ritter and Hargens, 1975.

ever, instead of just analyzing samples of males, they have focused increasingly on women as well (Chase, 1975; Glenn et al., 1974; DeJong et al., 1971; Treiman and Terrell, 1975; Tyree and Treas, 1974). Their concern has been primarily to measure intergenerational mobility of women, typically taking father's occupation as the origin point. For the destination status, two alternatives generally have been chosen. One is the woman's (usually a wife's) own occupation for a measure of social mobility via occupational achievement (Tyree and Treas, 1974; DeJong et al., 1971; Treiman and Terrill, 1975). The alternative destination status often selected is the socioeconomic status of the woman's husband, the goal here being to study marital mobility (Tyree and Treas, 1974; Glenn et al., 1974; Chase, 1975).

Unfortunately, by opting for only one of these alternatives, such studies present a unidimensional view of socioeconomic status. They look at either the wife's own occupational status *or* her status as derived from her husband, but not both simultaneously. Hence, to the extent a wife's (or a husband's, for that matter) *overall* socioeconomic status is partly a function of her (his) labor-force and occupational status and partly a function of her husband's (his wife's) occupational status, these approaches cannot give us an overall measure of the socioeconomic status of wives, their husbands, or the family as a unit. Nor can they enlighten us on the relationship between the occupational statuses of husbands and wives. They fail in this because, in effect, the rules of the game have changed. Measuring the occupational status of one individual in a family now frequently provides nothing more than a partial indicator of either the *family's* socioeconomic status or that of the individual in question because socioeconomic status will be influenced by the occupational achievements of other members of the family as well—most notably, the spouse for adult married people. This is not to say that these studies necessarily claim that they are measuring overall socioeconomic status and have failed to live up to this claim. What is meant is simply that, whether or not they have tried to construct an overall

measure, they have not succeeded in doing so. Instead, we have a rather fragmented view of the socioeconomic status of both individuals and families. As a result, while the indices used by previous studies to measure socioeconomic status (such as the Duncan Socioeconomic Index) may serve some of the purposes of current mobility analyses, they are becoming less relevant as general measures of socioeconomic status which can be used in a variety of sociological studies in other substantive areas of research.

While current mobility investigators have not attempted to create a composite measure of socioeconomic status for either individuals or family units, a number of other investigators have dealt more explicitly with this problem—arguing that the wife's as well as the husband's occupational status should be considered jointly in determining the socioeconomic status of an individual or family. Day (1961) argued at length for a more extensive consideration of the status impact of the working wife and Barth and Watson (1967) did some exploratory work on the effect of wives' working on their families' life-styles. More recently Rossi et al. (1974) have been involved in research indicating that the wife's socioeconomic characteristics, as well as the husband's, play an important (though unequal) role in the social standing of the household as a whole (see Sampson and Rossi, 1975; Haug, 1973; Ritter and Hargens, 1975).

These investigators have been concerned primarily with the issue of whether wives, as well as husbands, have an impact on the wife's or family's socioeconomic status. They have not been overly interested in the nature of the relationship between the wife's occupational characteristics and those of her husband. However, this is an important question in its own right because of the strong possibility that there exists a causal relationship between the socioeconomic contribution of the wife and her husband's socioeconomic characteristics. Most research on female labor-force participation, for example, has shown that whether or not a wife works—hence, whether or not she has an occupational status

to measure—is related to such socioeconomic characteristics of the husband as his income (Cain, 1966; Bowen and Finegan, 1969; Sweet, 1973; Mincer, 1962). The further question is whether the socioeconomic level of her occupation may not also be influenced by her husband's socioeconomic characteristics. It is this issue of the causal relationship between the wife's socioeconomic contribution to the family and her husband's socioeconomic characteristics that provides the primary focus of this paper. The major goal will be to examine critically certain traditional theories about sex roles and to try to modify them so as to achieve a theoretical approach more in keeping with the emerging realities of the socioeconomic behavior and characteristics of men and women.

Although a number of sociologists have discussed the nature of women's socioeconomic role in the family, one of the most influential theories dealing with this issue is found in the writings of Parsons. Because Parsons' work has been so influential and is one of the more carefully elaborated statements on the topic, the discussion of sociologically "traditional" views of women's economic and family roles will be limited to a critical examination of some aspects of the Parsonian approach. Parsons' work is of particular interest here because his theory about women's economic role in the family is developed mainly in terms of certain hypotheses regarding the causal relationship between husbands' and wives' occupational behavior.

Parsons' discussion of the nature and determinants of women's socioeconomic role in the family has a number of facets. Particularly important is his argument that sex-role segregation is a functional necessity for marital stability in our society (Parsons, 1949:193). As a consequence, Parsons (1949:195) argued, most women must be relatively excluded from the occupational system, "at least in a status-determinant sense." This is so, Parsons maintained, because the marriage relationship is structurally unsupported in an urban industrial society and, as such, is fragile and easily disrupted without protective mechanisms. The major mecha-

nism preventing disruptive competition between husband and wife is sex-role segregation, where the dominant male role is the occupational role and the dominant female role is that of housewife and mother (Parsons, 1949:193). Since the major source of socioeconomic status is occupation in our society, a married woman working raises the possibility of disruptive competition between the spouses. If marital stability is to be maintained, married women must either avoid work or work at an occupation which is not status competitive—for example, it must be at a "job" rather than a "career" and "ordinarily does not produce a comparable proportion of the family income" (Parsons, 1949:193ff.). As a consequence, wives will make an insignificant impact on the family's socioeconomic status (Parsons, 1954a:80).

There are two aspects of Parsons' theory which are of major interest here. First, it is important to scrutinize quite carefully his notion of the role of status consistency in affecting women's economic role in the family. This paper will argue, with Parsons, that status consistency—or at least a liberalized version of the concept which will be called "status compatibility"—is important in maintaining family solidarity. However, Parsons deals with the deleterious effects of competition, while the argument presented here will emphasize the role of status maintenance and status enhancement as the crucial factor which encourages employment of wives under certain conditions and discourages it under others. There is also the question of whether competition is as important a problem in the two-worker family as Parsons hypothesized. This paper will argue that Parsons overestimated the competitive difficulties inherent in such a situation and failed to consider a number of factors which could prevent or neutralize the competitive conflicts which he saw as inevitable.

Although a number of Parsons' views regarding sex roles are under question here, there are two which are not particularly at issue. The first is the assumption that direct competition is probably damaging to intimate relationships. Second, Par-

sons has argued that a long-standing norm in our society is that the husband is the major provider and status determiner in the family. I will proceed under the conservative assumption that these are true.

STATUS COMPATIBILITY AND THE PROBLEMS OF STATUS MAINTENANCE

One reason that Parsons gave for the necessity of sex-role segregation was that conjugal solidarity is inconsistent with drawing class distinctions between husband and wife. If such distinctions did arise, he argued, disruptive competition would occur. It is clear that Parsons' position is based mainly on his assessment of how the internal dynamics of the conjugal family operate. However, the problem might be more profitably approached from a somewhat broader perspective—that is, by focusing, in addition, on the family as a basic unit in the stratification system of the society.² By viewing families within the context of the larger stratification system and, hence, in competition with other families for status, then it is clear that one problem families, as solidary units, face is that of status maintenance or improvement. This perspective on the family is thoroughly consistent with Parsons' own views. He emphasizes the universalistic and achievement orientation (i.e., competitive nature) of our occupational system as well as seeing the family as a basic unit of stratification, one which is inevitably concerned with its status position relative to other families in the society (Parsons, 1954a:77ff., 85ff.; 1954b: 422ff.).

Unfortunately, Parsons did not really follow through on this picture of the family in analyzing the determinants of women's economic role. However, looking at the family as a solidary unit concerned with status maintenance and enhancement should indicate how these larger status concerns can promote rather than minimize wives' economic role while, at the same time, structuring the nature of that

role. It is true that, to some extent, Parsons recognized the importance of economic pressures promoting wives' labor-force participation in his assessment of the more important economic role of women among the "low ranges of occupational and income status" although he also thought a high price was paid for this in marital instability (Parsons, 1949:194). However, economic pressures for the wife to work are not limited to lower-class families but are operating throughout the society and probably increasing over time (Oppenheimer 1974; 1976). Moreover, opportunities for wives to work rose rapidly in the postwar period, encouraging a positive response on the part of wives to a desire for a higher family income (Oppenheimer, 1970; 1973). And the more wives who respond to such economic pressures and economic opportunities—middle- as well as working-class—the greater the relative socioeconomic disadvantage of families without working wives (Oppenheimer, 1976).

Emphasizing the importance of the family as a unit in the larger stratification system leads to a somewhat different interpretation than Parsons' of the role of status consistency. Parsons argued that it is essential to have status consistency within the family because status discrepancies would lead to disruptive competition. Although status consistency is important, there are more compelling reasons for its importance than the possibility of disruptive competition. That is, status consistency is important not so much because highly divergent statuses would lead to competition but because they would threaten the status position of the family as a whole as well as the statuses of individual members of the family. Parsons does not take this possibility into consideration because, first, he neglected to keep in mind the status-maintenance problems of the family and, second, he developed only a one-sided view of the negative consequences of status inconsistency. Thus Parsons' theory implies that the only labor-force involvement of wives compatible with family stability is one where the wife has a lower-level job than her husband—a job which, as a consequence, somehow does not count. Pre-

² One need not think of a single stratification system in the society but of a multiplicity of systems, i.e., any particular family in competition for status with a number of families, not with every other family in the society.

sumably, if she should have a job which is at a higher socioeconomic level, such a discrepancy would "put an intolerable strain on the imperative of class equality" and, hence, on the marriage (Parsons, 1949:195). However, one might just as well argue that if there are negative effects stemming from a wife having a *higher* occupational status than her husband, there will also be negative effects if she has a much *lower* status. Thus, Parsons is not consistent in the development of his basic argument—the importance of status homogeneity within the family. His theory comes perilously close to an argument for the functional necessity *not* of equivalent evaluation within the family but of male superiority.

It is possible to start with Parsons' original idea—the importance of status homogeneity—and develop it in a more relaxed and evenhanded fashion, without any recourse to functional imperatives. First, it seems reasonable to assume that the social statuses of closely associated individuals (such as family members) tend to rub off on each other. In other words, all members of a family tend to share at least some of the benefits and liabilities of their individual status sets. As a result, there is a strain toward status consistency within social groups such as the family; if one or more of the statuses of one member of the family are much lower than the statuses of the other members it reflects poorly on the others. In addition, the individual with the much lower status finds himself at a disadvantage *vis-à-vis* the others and, perhaps, at a disadvantage relative to the other statuses in his own status set. As far as the internal social system of the family is concerned, this may result in a loss of prestige, power or affection. Therefore, a strain toward status consistency is often to the advantage of all concerned. As a consequence, where a publicly-known status of one member of the family group is much below that of other members, certain social mechanisms should come into play to deal with the disadvantages resulting from such a marked status inconsistency.

(1) The offending status is given up, if possible, or, if only contemplated, its attainment is not pursued. One example

might be the case in which an alcoholic wife or husband reforms.

(2) The negative effect of the highly discrepant status is somehow neutralized, especially when there are some benefits from the status as well as some potential liabilities. One way neutralization may be achieved is by a consensus on the temporary nature of the status. For example, the status may be considered appropriate for one stage of the individual's life cycle but not for another. Thus, a man of high socioeconomic status may have no qualms if his son works as a construction worker during the summer while on vacation from school, but the father's feelings will be very different if the son proposes to spend his life doing cement work.

(3) The relationship with the family member who has the much lower status is severed. Thus, at one time, young women who became pregnant with no wedding in sight so compromised their own status and that of their families that many ran the risk of being disowned.

(4) The family opts out of the usual evaluative system. With respect to the social stratification system, for example, they may decide to recognize no hierarchy of occupational statuses at all, and the highly discrepant status is definitionally eliminated. However, as long as any evaluative systems are in force, the dilemma remains.

How are such mechanisms likely to operate in marriages where both spouses are not only working or contemplating employment but where the possibility of highly divergent occupational statuses also arises? In such cases, the operation of these mechanisms has been strongly influenced by the relatively long-standing norm in our society that the man be the major provider and status determiner. To the extent such a norm is flourishing, it will affect which mechanisms come into play because a different meaning will necessarily be attached to the status inconsistency depending on whether the wife's or husband's status is the much lower one.

Take the case where the wife has achieved a much higher occupational status than her husband. Because the normative expectation is that the husband

should be the status-determiner for the family, this limits the kinds of mechanisms which will deal with this type of status inconsistency. For example, he does not have the social option of eliminating the status discrepancy by quitting work. While the negative effects of the discrepancy may be neutralized for a time by interpreting his much lower status as temporary for some socially acceptable reason, he remains at a disadvantage unless he can soon achieve considerable upward mobility. On the one hand, the wife does have the option of eliminating the status discrepancy by quitting work and this may be one resolution of the conflict, probably at some psychic costs to her. On the other hand, she may change husbands—or at least do without the lower-status husband—rather than relinquish a relatively rewarding occupational career for a socioeconomic status that is lower than she could achieve herself. Of course, what happens in any particular case will depend on other aspects of the marriage as well. However, given current family norms, this type of status discrepancy is likely to create severe strains in the marriage relationship and will increase the probability of separation and divorce as the means of dealing with the problem.

Most married women in our society have had socioeconomic status set for them by their husbands. If social statuses are indeed contagious in nature, then in the case where the husband's occupational status is much higher than the wife could realistically achieve on her own (given her current occupational qualifications and current market conditions), her working at a low-status job would reflect poorly on the family's standing in the community. A socially acceptable solution to this problem for the wife, but not the husband, is to quit work. In sum, whether or not a wife works will depend on whether her occupation enhances or detracts from the family's socioeconomic position. Because many wives, unlike most husbands, have had the option *not* to work, they have the option—if not the obligation—not to have an occupation that would reflect poorly on their own or their family's position. If this is true, then the higher a woman's derived

socioeconomic position, the higher her occupation must be if she is to have a serious work commitment. Furthermore, it will be to the advantage of both the wife and her family if she can optimize her occupational status.

Just as giving up a much lower occupational status is more feasible normatively for the wife than the husband, so is the neutralization of the possibly negative consequences of a considerably lower status. For example, many women will not mind holding jobs that would not be commensurate with their present or expected future status as long as these jobs are considered short-term rather than providing a regular self-identifying adult role—working as a secretary, for example, while the husband is a medical intern or working to send the children to college. Other jobs which might be exceptions are those which can blend into avocations and which may be chic, though not necessarily remunerative. Of course, the social distance between a woman's occupational status and her derived social status cannot be too great.

In sum, I am arguing, first, that the status maintenance or enhancement concerns of the family produce pressures on the wife to work. Second, however, the same status considerations influence how serious and long-term her work commitment will be (if she does work) as well as her occupational level. The view presented here is that, for reasons of status maintenance, status *compatibility* among family members is essential although status *equality* is not so important. In terms of the wife's socioeconomic role in the family, this means that she is more likely to work if her potential occupational status—and its economic rewards—serve to *enhance* the family's socioeconomic status. On the other hand, if her relative economic contribution to the family were to be negligible and her occupational status much below that of her husband's, she would be less likely to work as there would be little net socioeconomic advantage to her and the family. Hence, arguing from the standpoint of the external status maintenance problems of the family, rather than the internal complications raised by competition *within* the family,

leads to quite different conclusions about wives' socioeconomic role than those of Parsons.

STATUS COMPETITION

If the family's status maintenance concerns encourage a positive socioeconomic role on the part of the wife, what about the supposedly disruptive competitive problems raised by Parsons? Parsons argued that the norm requiring the husband to be the main, if not the only status determiner and provider for the family while his wife is the homemaker is a functional imperative for marital stability in our society. In other words, such norms presumably developed and persist because if both husband and wife had important work attachments, disruptive competition would occur. Hence, he is arguing that competition is inherent in the situation where both spouses work and that sex-role norms are, in a sense, an *effect* of the competition inherent in the two-worker family. In fact, however, there are a number of reasons for arguing the reverse—namely, that such a norm itself creates conflict where none might otherwise arise. Furthermore, even if competition does develop at times, it may not be as serious a societal problem as Parsons maintains. Taking the latter case first, it is probably true that the potential for conflict undoubtedly will be the greatest when both husband and wife are in career-type occupations—occupations in which geographic mobility is often important, in which the occupation makes heavy demands on both the worker and his or her family (coping while the spouse is away on business or working overtime, business-related entertainment, etc.) and in which there tends to be a heavy ego investment in the occupational role. However, a good part of these conflicts arise out of the enormous and conflicting demands made on each career partner and not necessarily because the spouses are competing with each other for a higher status. Furthermore, competition, where it does occur, may be particularly severe at some points in the career cycle but not others. For example, an especially trying period for most people entering high-level occupations is the early stages where

competition among peers is heavy and few have yet found their niches. Nevertheless, regardless of these problems, the number of people in high-level careers does not represent a numerically important segment of our population. Thus, while it is true that relatively more men than women have "careers" rather than "jobs," it is also probably safe to say that most men, like most women, have "jobs" and not "careers." One might seriously question whether the occupational demands made on the average accountant, schoolteacher, engineer, craftsman or operative are such as to provide the disruptive, competitive features Parsons hypothesized.

Although complete sex-role segregation may be one mechanism for preventing competition, there are other buffer mechanisms which will have a similar effect without excluding wives from a significant work role. For example, one simple type of segregation mechanism places the spouses in different occupations or even in the same occupation but in different work environments. There are a number of ways this can protect spouses from competing directly with each other even if the socioeconomic levels of their occupations are comparable. First, if the pair are in different occupations, their co-workers and the community at large will have difficulty ranking them unless there are gross status inequalities. After all, unfamiliarity with all but very familiar occupations is one well-known problem with occupational prestige studies like the North-Hatt study (Reiss, 1961: ch. 2). Furthermore, the details of each spouse's income are not generally known to the community, making invidious comparisons even more difficult. Parsons' implicit assumption that people in different occupations—or even the same occupation—are typically in direct status competition with each other seems dubious. A more reasonable assumption is that competition among individuals is structured according to rather specific occupational or organizational work situations. Thus, workers in different occupations and even in the same occupation under different employment conditions operate in distinct competitive hierarchies which may be limited to a particular work organization or type of work

organization, a limited subsegment of the larger occupation, or a particular community. Furthermore, many of the rewards that signify the attainment of different prestige levels within the relevant occupational or organizational reference group vary among occupations and within occupations. For example, to what extent is success in academic medicine measured in the same way as success in private practice within a given community? How does one generalize, beyond the confines of a given organization, the meaning of the many subtle and nonmonetary ways in which bureaucratic organizations reward people at different hierarchical levels? In a sense, the occupational ranking systems sociologists have developed—useful as they may be for some purposes—are abstractions, while competition among individuals is a very concrete phenomenon embedded in very particular interaction situations. Hence, the extent to which people operating in different occupational or organizational reference groups directly compete can be rather limited even if they apparently are in competitive positions on one or more of the occupational scales currently being employed by sociologists.

The existence of different competitive hierarchies is particularly obvious in the historical tendency for men and women to concentrate in sex-labeled occupations (Oppenheimer, 1970: ch. 3). Even when the wife is in a white-collar occupation and the husband in a blue-collar occupation, the fact that she is in a traditionally female job will probably reduce the perceived competition between her and her husband.

An overlapping set of factors which will operate to protect working couples from disruptive competition arises from the sources of invidious comparisons. To the extent the evaluations of "significant others" are important in such conflicts, a variety of protective mechanisms may come into play. Since "significant others" often do not know enough to rank couples in different occupations and usually are unfamiliar with the particulars of each spouse's earnings, it is difficult for them to make comparisons. In addition, a community's evaluation of a couple is

based partly on the family's consumption patterns—i.e., its style of life—rather than the occupational sources of the income used for consumption. To the extent this is true, the pooled incomes of husband and wife permit a homogeneous level of socioeconomic status rather than two competing levels. Another way of presenting a "united front" to the community so that invidious comparisons are difficult is collaboration—being in business together or collaborating professionally.

It is interesting to note that in an essay first published in 1940 Parsons himself suggests that mechanisms similar to those discussed above may be operating to prevent invidious status comparisons among close kin. However, he refers to brothers rather than spouses. He argues that "the vagueness of our class structure provides a kind of cushioning mechanism" and that "so long as the discrepancy is not too great, it is then unnecessary for there to be any very exact determination of relatives' class status . . ." (Parsons, 1954a: 87–8). Parsons also thought that geographical separation was an additionally important factor.

For instance, if two brothers are on the faculty of the same university, the question of their relative status is very acute. But if one is a physician in Boston and the other is in business in Chicago, such questions hardly arise at all unless the discrepancy of their relative "success" is very marked. (Parsons, 1954a:88)

However, it seems reasonable to argue that, in the case of the physician and businessman brothers, the cushioning effect of the "vagueness of our class structure" would be operating even if they both lived in the same city. And if it could work for two brothers, the same mechanism may also operate in the case of working spouses.

It is probably true, however, that tensions may arise when the wife's earnings equal or exceed her husband's since money, unlike other status perquisites, tends to be a roughly universal measure of success.³ The question is whether any

³ Even here, however, one may argue that potentially damaging invidious personal comparisons may be defused by the recognition that the earnings of

stresses induced by the equal or better earnings of the wife are inherent in the situation or are a result of the traditional norms governing men's family roles. As long as the spouses' incomes are not too discrepant, there is a distinct economic advantage to the couple and the individual partners in both working and staying married to each other, no matter whose income is higher. The fact that a wife's equal or somewhat greater earnings is interpreted as a threat in spite of its economic utility to the couple suggests that it is the existence of the norm basing the husband's standing and authority in the family on his ability to be the major provider which leads to stress rather than any other inherent features of the situation.

Considering the norm requiring the man to be a good provider for his family as the possible "culprit" in some types of marital conflict casts a different light on Parsons' contention that the higher rates of marital instability in lower socioeconomic groups is due to the more frequent employment of wives (Parsons, 1949:193-4). As other researchers have pointed out, there is good reason to believe that a major factor in the considerable marital instability of such groups is not the wife's labor-force participation *per se* but, rather, the husband's inability to do his share because of chronic job insecurity (see, for example, Rodman, 1963; 1967: 214-5; Rainwater and Yancey, 1967: 65ff.). This is a very different situation from one in which two schoolteachers are

married or a regularly employed plumber has a wife who is a clerk typist.⁴

EMPIRICAL EVIDENCE

Contrary to Parsons' theories, we can expect wives to have a growing socioeconomic role in the family rather than a persistently negligible one. Also, problems of status maintenance will tend to lead to a relatively favorable comparison of husbands' and wives' socioeconomic status, rather than a much lower (and, thereby, presumably non-competitive) status on the part of the wife. Finally, I have suggested that Parsons exaggerated the amount of status competition which would necessarily arise in the two-worker family. The next issue is whether there is any empirical evidence that can help us choose between these two contrasting views of the nature of women's socioeconomic role in the family.

First, trends in female labor-force participation in the postwar period are more consistent with the view that socioeconomic pressures and opportunities have supported an increasingly important socioeconomic role on the part of wives than the view that women's economic roles must, of necessity, be minor. Women's work-rates have risen considerably at all stages of the family cycle (Openheimer, 1970; 1973). Furthermore, research in progress by this investigator indicates that the work rates of wives of white-collar workers rival those of blue-collar wives and that it is only the wives of the highest-level workers who exhibit somewhat depressed work-rates. However, such upper-middle-class wives showed some of the greatest increases in labor-force participation in the 1960-1970 period.

Second, research on the determinants of wives' labor-force status suggest that the greater the potential relative contribution of the wife, the more likely she is to work. This finding is hardly consistent with the stress Parsons puts on the wife

"successful" people in some occupations may not be in the same league as the earnings of an average worker in another occupation. That is, earnings differentials are a feature of occupations, not of the performance of individuals within these occupations and, hence, carry no personal stigma. Parsons (1954a:87) provides a rather nice example of this process when he compares a world-famous scientist in a university with a corporation lawyer:

so long as the scientist is able to maintain a "respectable" standard of living, entertain his friends well, dress his family adequately, and educate his children well, the fact that he cannot afford the luxuries of a hundred-thousand-dollar income is a matter of relative indifference. He simply does not compete on the plane of "conspicuous consumption" which is open to the lawyer but closed to him.

⁴ Divorce rates have been going up in recent years (see, for example, Glick and Norton, 1973), but whether this can be mainly attributed to work-induced competitive conflicts is doubtful.

making a negligible contribution to the family's socioeconomic status. Thus, using the 1/1000 sample from the 1960 census, Sweet (1973:91-2) found that the wife's education was positively related to her employment status. More importantly, perhaps, he found that her education had a greater effect on employment the lower her husband's income (Sweet, 1973:131). That is, if the husband had a relatively low or moderate income, high education on the part of the wife led to a higher employment rate than the same educational attainment would if her husband's income were high. The level of educational attainment of wives is usually interpreted by labor-force analysts as an indicator of her potential occupational and wage level (Sweet, 1973:51; Bowen and Finegan, 1969:53). Hence, Sweet's findings suggest that, where economic pressures for an additional income seem to exist, the greater the potential relative socioeconomic contribution the wife could make to the family, the more likely she was to be working. However, these findings do not appear to be consistent with Parsons' basic argument, as they are indicative of conditions that are most likely to result in similar (and presumably competitive) occupational statuses between husband and wife as well as a significant rather than a negligible socioeconomic contribution on the part of the wife.

Additional data on the socioeconomic contribution of wives come from preliminary results of an analysis of the 1970 Public Use Samples (PUS). The data are from a 3/1000 sample of white couples (Spanish descent excluded) where the husband was a household head, 18-64 years old and currently or last reported as working in a nonfarm occupation.

The sample of couples was divided into several socioeconomic groups based on the husband's occupation. In this way, all wives could be assigned a socioeconomic position regardless of their labor-force status, and it would still be possible to do a detailed comparison of the socioeconomic characteristics of husbands and wives.

One main problem was to find a suitable occupational classification system for the

men. The system, and its rationale, is explained in greater detail elsewhere (Oppenheimer, 1974). The major goal of the occupational classification system was to define a small number of occupational categories, each of which was relatively homogeneous and distinct with respect to certain important social and economic characteristics—mainly earnings and occupational type. The procedure followed was to examine the median 1959 earnings by age of men in each detailed occupation on which published data were available in the 1960 census (1970 data had not yet been published when the study began). This analysis of published data then was used to group the detailed occupations within each nonfarm major occupation group according to the peak 1959 median earnings among different age groups in that occupation. For example, the highest median 1959 earnings of Accountants and Auditors were for men aged 45-54 years old and were \$7,615. Hence, Accountants and Auditors were classified into the Group II Professions—those professional occupations with peak 1959 median earnings in the \$7,000-8,999 range. All detailed occupational categories were classified in a similar way—into peak median earnings groups within major occupational groups. Then the men in the PUS sample were classified into one of these categories on the basis of their current or last occupation. For the 1970 PUS, the same occupations were classified in exactly the same way, regardless of their 1969 earnings—at least to the extent this was possible, given the numerous changes in the 1970 detailed occupational classification system (U.S. Bureau of the Census, 1972). In this fashion, an occupational classification scheme was created which measured the general earnings level of the occupation as well as preserving important differences within earnings groups—namely the occupational type, such as professional or managerial. The same occupational classification system was used for the wives.

The findings are relevant to two major aspects of the theoretical alternatives under discussion in this paper. First, there is the extent of the socioeconomic contribution wives make via their economic

contribution to the family. The question at issue is whether or not this contribution is so small as to be negligible in a "status-determinant sense." Second, there is the question of the joint occupational characteristics of husbands and wives. Are wives' occupations uniformly at a lower status level than their husbands' or is there, instead, evidence of status similarities with, perhaps, certain minimum acceptable occupations for wives? Moreover, is there any evidence of labor-force selectivity for wives at different occupational levels? Are wives who might operate at a supposedly competitive level with their husbands more likely to have dropped out of the labor force (consistent with Parsons' theories) or are such wives more likely to be working, and the drop-outs more likely to be among those whose occupational level might have a minor or negative impact on the family's socioeconomic status (the argument presented in this paper)?

The Wife's Economic Contribution

What is the nature of the wife's socioeconomic contribution to the family? Let us first consider wives' economic contributions and how these affect the socioeconomic status of their families. A wife's earnings will undoubtedly affect the general status position of the family and not just its economic position because the wife's earnings will probably modify the family's consumption patterns (i.e., its life style), which are an important factor in the community's evaluation of the family's status. It is interesting to note that Parsons himself stressed the importance of money as a criterion of status, especially in view of the fact "that its expenditure is largely for other symbols of status in turn" (Parsons, 1954a:85). Sociologists have been able to avoid seriously considering life style as an independently important determinant of social status because the assumption typically has been that only the husband makes an important socioeconomic contribution to the family. Hence, only his occupation will determine the family's income and its resulting style of life. Since occupation has appeared to be a good indicator of socioeconomic

status for other reasons as well (e.g., occupational prestige), there has been little motivation to go beyond the consideration of the husband's occupation alone. However, when serious consideration has to be paid to the possibility of two incomes in the family, then the *status* impact of the second income, as well as its *economic* impact, can no longer be safely ignored (for discussion of this problem, see also Barth and Watson, 1967; Duncan, 1961:118).

The issue of the working wife's economic impact on the family's socioeconomic status does not involve just the question of whether wives make an impact but the whole question of the criteria used to decide whether that impact is significant or not. In other words, just how large do the wife's earnings have to be for her employment to have an impact on the family in an economic or "status determinant sense"?

Table 1 shows the ratio of the median earnings of wives to those of husbands for couples where both spouses had net positive earnings in 1969 and for families where wives had extensive work experience during the year. It indicates that the lower the occupational earnings group of the husband, the higher the ratio of wives' median earnings to husbands'. This is true for couples where the husband was a white-collar worker as well as those where he was a manual or service worker. For example, among professional husbands, the ratio of median earnings of wives to husbands rises from 28 percent for Professionals I to 43 percent for Professionals III which is a higher ratio than for several manual groups. The ratios are even higher, of course, when considering couples where the wife appeared to be working full time most of the year, although a relatively small proportion of couples fell into that category. Where the husband was in a Professional I occupation, for example, the ratio of median earnings went up to 50 percent if the wife had worked full time.

Although wives' earnings start to approach those of husbands in the middle and lower occupational groups, women still earn considerably less than their husbands. Given this fact, the question is

Table 1. Wives' 1969 Median Earnings as a Percent of Husbands', by Husband's Occupation and Wife's Work Experience: White Couples, 1970*

Husband's Occupation	Ratio of Wives'/Husband's Median Earnings (Percent)		Percent of Families with an Earning Wife	
	All	Wives with Earnings Full-Time Wives	All Wives with Earnings	Full-Time Wives
Total	39	59	50	20
Professional and Technical Workers				
I	28	50	41	12
II	34	54	51	18
III	43	66	61	23
Managers, Officials and Proprietors				
I	30	48	42	15
II	33	49	46	18
III	38	55	48	21
Sales Workers				
II	37	56	48	19
III	45	63	54	54
Clerical Workers				
III	43	62	56	24
IV	52	67	60	28
Craftsmen and Foreman				
II	33	48	47	19
III	37	56	50	20
IV	42	61	49	20
Operatives				
III	39	60	52	21
IV	43	62	51	21
Service Workers				
III	39	60	52	21
IV	54	76	57	26
Laborers				
IV	44	69	50	19

* Two groups of wives with net positive earnings were considered: (1) "All"—all wives with earnings; and (2) "Full-time"—those wives who worked 40–52 weeks in 1969 and 35 or more hours in the week before the census. Forty weeks was selected as the minimum for "full-time" employment in order not to exclude teachers.

Source: 1970 3/1000 sample of white couples drawn from the U.S. 1970 Public Use Sample, 5 percent, County Group Sample.

whether the contribution of wives can really have a "status determining" impact on their families. The implication of Parsons' argument is that the wife's earnings are not important because they do "not produce a comparable proportion of the family income" (Parsons, 1949:194). The traditional absence of concern regarding wives' earnings in stratification research indicates that this view has been generally shared. However, this approach is inconsistent with the criteria usually employed

in stratification research to determine socioeconomic status and social mobility. No scholar has maintained, for example, that social mobility only occurs when a man moves from one occupation to another that pays at least twice as much. Hence, to assume that the wife's income must be equal to her husband's for it to have an effect confuses the difference between making an impact on the family's socioeconomic status and making the same impact as the husband does.

A comparison of the median 1969 income of families where the wife had earnings in 1969 to those where she did not provides a rough indication of the impact of wives' earnings on their families (Table 2). Such a comparison shows that the earnings of a working wife (regardless of the amount of time worked) frequently are sufficient to provide a functional income substitute for upward occupational mobility on the part of the husband. Thus, the median family income was higher for families of Professional III men *with* earn-

ing wives in 1969 than for the families of Professional II without earning wives. Families of men in the Manager III group who had an earning wife all but closed the gap between themselves and the families of men in the Manager II group who did not have wives with earnings. This is the case generally: the earnings of the wife usually puts the family in the next higher income group. If the wife worked 40-52 weeks in the year and 35 or more hours in the week before the census, her impact was even greater, of course. For example,

Table 2. Median 1969 Income of Families by Husband's Occupation and by Earnings Status and Work Experience of Wife: White Couples, 1970 ^{a, b}

Husband's Occupation	Wives without Earnings	Wives with Earnings	
		All	Full-Time Wives
Total	10,369	12,302	13,585
Professional and Technical Workers			
I	16,834	17,065	18,516
II	13,156	14,683	16,331
III	10,930	13,578	15,048
Managers, Officials and Proprietors			
I	16,587	14,978	17,000
II	13,616	14,996	16,072
III	11,326	13,423	14,415
Sales Workers			
II	14,064	14,620	15,824
III	10,438	12,573	13,568
Clerical Workers			
III	10,098	12,423	14,085
IV	8,466	11,109	12,738
Craftsmen and Foreman			
II	12,477	14,356	15,773
III	10,037	12,258	13,668
IV	8,411	10,596	11,760
Operatives			
III	8,866	11,221	12,865
IV	8,293	10,555	11,842
Service Workers			
III	10,189	12,484	14,295
IV	7,399	10,023	11,268
Laborers			
IV	7,055	9,452	10,955

^a Median income in dollars of families with net positive incomes.

^b Two groups of wives with net positive earnings were considered: (1) "All"—all wives with earnings; and (2) "Full-time"—those wives who worked 40-52 weeks in 1969 and 35 or more hours in the week before the census. Forty weeks was selected as the minimum for "full-time" employment in order not to exclude teachers.

Source: 1970 3/1000 sample of white couples drawn from the U.S. 1970 Public Use Sample, 5 percent, County Group Sample.

for such families, if the husband was a Craft III worker, the family income went up to \$13,668—more than the income of families of Professional II men who did not have wives with earnings in 1969.

Furthermore, such figures actually *understate* the impact of the wife's earnings on the family's income because the income of husbands of nonworking wives is typically higher than that of husbands of working wives. For all occupational groups combined, for example, the median 1969 income of husbands of wives with earnings was only 86 percent of the median income of husbands of wives without earnings. Hence, the husband's dollar share of the family income for couples where the wife had earnings was less than in families where the wife did not work.

In sum, the evidence indicates that whether or not wives worked extensively during the year and in spite of the fact that their median earnings were not comparable to the earnings of husbands, wives' earnings were high enough, on the average, to have a considerable impact on family income—enough, in many cases, to eliminate the income difference between a working wife's family and the family of a man in a much higher paying occupational group whose wife did *not* work. Therefore, in assessing the impact of the working wife's income on the family's socioeconomic status the most important issue is *not* whether her earnings equal those of her husband's but the extent to which her earnings provide a functional substitute for upward occupational mobility on his part or a counterbalance to downward mobility.

Status Compatibility of Husband's and Wife's Occupations

By and large, data on the joint occupational characteristics of husbands and wives support the view that status compatibility is an important factor, since relatively few wives and husbands are at highly distinct occupational levels (Table 3). Furthermore, the data even suggest that it is more important for the wife to have an occupation which reflects favorably on the status position of the family

rather than one which guarantees a low profile. Thus, a very small proportion of the wives of white-collar men are found in blue-collar or service occupations. For example, while 17 percent of all employed wives were in manual occupations and 15 percent in service occupations, only three percent of Professional I wives were in manual occupations and six percent in service occupations.

Most wives of white-collar men were to be found in middle-level occupations—that is, the status difference between themselves and their husbands was not too great. Professions, especially the lower-paid professions (i.e., many of the female professions) were particularly important for the wives of professionals. For example, while only 18 percent of all employed wives were in professional occupations, 47 percent of the wives of Professionals I and III men and 33 percent of the wives of Professional II men were in the professions. In general, the wives of white-collar men were much more likely to be in professional and technical occupations than the wives of manual workers. In addition, for all white-collar wives clerical occupations were of great importance, with the proportions in these occupations varying from 32 percent for the wives of Professionals III to 47 percent for the wives of Sales II and Clerical III men. In sum, the wives of most white-collar men do not seem to be working in occupations at a considerable status distance from their husbands'.

White-collar couples in which the greatest amount of status discrepancy is evident are those where the husband was in a Group I occupation. Although relatively few of the wives of Group I Professionals or Managers were in blue-collar or service occupations, it was also true that not many were in occupations at the same or adjacent earnings levels to their husbands. This is particularly true for the wives of men in the Managers I and II group. These patterns certainly appear to be consistent with Parsons' notion that the risk of status competition leads to the avoidance of this type of situation. However, they are also consistent with my view that such competition is most serious for higher-level workers. Furthermore,

Table 3. Occupation of Employed Wives by Occupation of Husbands: White Couples, 1970 ^a

Husband's Occupation	Wife's Occupation (Percent Distribution)								
	Total	Professionals		Managers	Sales		Clerical	Blue- Collar	Service
		I & II	III		II	III			
Total	100	4	14	4	1	7	38	17	15
Professionals									
I	100	13	34	3	2	4	34	3	6
II	100	9	24	3	1	6	44	5	7
III	100	4	43	2	1	5	32	6	7
Managers									
I and II	100	5	18	6	1	8	45	9	8
III	100	3	11	12	..	10	42	8	12
Sales									
II	100	4	20	4	3	8	47	6	8
III	100	4	15	4	1	11	44	9	11
Clerical									
III	100	4	14	4	1	7	47	12	13
IV	100	3	8	2	..	8	46	22	12
Craft									
II	100	2	11	3	1	8	38	25	13
III	100	2	8	3	1	8	40	22	16
IV	100	1	8	3	1	8	33	26	20
Operatives and Laborers	100	2	6	2	..	7	31	31	19
Service									
III	100	1	14	4	..	6	47	12	16
IV	100	3	9	3	..	6	27	20	31

^a The occupational classification system was collapsed to simplify presentation.

Source: 1970 3/1000 sample of white couples drawn from the U.S. 1970 Public Use Sample, 5 percent, County Group Sample.

given women's socialization against strong occupational commitments and given the many other difficulties and obstacles involved in entering a high-level male occupation and combining it with marriage and a family, one would not really expect a high proportion of the wives of upper-white-collar males to be working at a comparable occupational level. This is especially true, given the variety of alternative activities available—both work and nonwork—which provide the opportunity for nonfamilial involvements but which make the total demands on the woman less monumental in character.

While relatively few wives of Group I or II men are in high-level occupations themselves, the data indicate that if a high occupational level is achieved there is little evidence of withdrawal from work to

avoid competition with the husband. Although occupational data on actual withdrawal from the labor force are not available, information exists on women in the labor reserve—those women not currently employed who have worked in the ten years preceding the census. Presumably, women in "status-threatening" positions will be more likely to drop out of the labor force in order to preserve their marriages and, therefore, we can expect to find some evidence of a buildup of such women in the labor reserve. Table 4 shows the current or last occupation of all wives who were working or who had worked at some time during the ten years preceding the census—women we shall call "recent workers." The measure used is the proportion of recently-working wives in a given occupation who were

Table 4. Percentage of Recently-Working Wives in a Given Occupation Who Were Currently Employed, by Occupation of Husband: White Couples, 1970 ^{a, b}

Husband's Occupation	Wife's Occupation								
	Total	Professionals			Sales		Clerical	Blue-Collar	Service
		I & II	III	Managers	II	III			
Total	57	66	65	70	62	50	56	54	54
Professionals									
I	48	64	50	61	69	32	43	46	45
II	53	67	59	64	..	44	51	44	53
III	62	67	72	71	..	52	56	51	53
Managers									
I & II	56	62	58	66	60	48	54	58	51
III	63	58	66	82	..	66	62	51	57
Sales									
II	56	69	61	64	62	52	54	54	48
III	62	66	70	77	60	59	61	56	58
Clerical									
III	60	71	69	74	56	50	58	58	60
IV	65	.. ^c	77	60	68	63	53
Craft									
II	56	..	70	77	..	45	52	57	54
III	57	69	71	68	66	48	57	55	54
IV	56	58	75	68	54	53	56	55	51
Operatives and Laborers	57	70	73	69	64	50	58	57	51
Service									
III	56	..	72	62	..	45	54	54	54
IV	66	67	78	78	..	54	68	63	67

^a The occupational classification system was collapsed to simplify presentation and to maintain a sufficient cell size.

^b Recently-working wives are those women with an occupation in the 10 years preceding the census—i.e., employed women plus women in the labor reserve.

^c Percentages were not computed where the base was less than 25.

Source: 1970 3/1000 sample of white couples drawn from the U.S. 1970 Public Use Sample, 5 percent, County Group Sample.

currently employed. To the extent that women drop out of the labor force because they are in status-threatening jobs, we would expect the proportions in the table to be low. On the other hand, if such jobs are not really a deterrent but are actually advantageous, the dropout rates should be low and most of these women will be employed. In that case, the proportions will be high. Unfortunately, women who move into less status-threatening occupations rather than dropping out of the labor force entirely cannot be detected. In addition, marriages that break up due to these status conflicts cannot be detected from these data either.

As Table 4 indicates, there is little evidence of a selective dropping out of women most recently in higher-level occupations; in fact, just the contrary. While only 48 percent of all recently-working

wives of Professional I men were currently employed in 1970 (compared to 57 percent for all wives), the proportion goes to 64 percent for those whose current or last occupation was in the Professional I and II groups.⁵ In general, for women in both the Professional I and II groups and the Managers group, the proportions employed were higher than for all wives. This also generally holds for all occupation groups of the husband where sample size is sufficient to permit measurement. For example, while only 56 percent of all the recently-working wives of Managers I and II men were employed, the proportion was 62 percent for those whose current or

⁵ When the Professional I and II groups are considered separately, the proportion of wives of Professional I men recently working in a Professional I occupation and who were employed in 1970 goes up to 71 percent.

last occupation was in the Professional I and II group.

In sum, there is little evidence of a substantial reservoir of non-employed women in high status occupations among the recently-working wives of upper-middle-class men—women who presumably dropped out of the labor force to avoid competing with their husbands.⁶ On the contrary, the reverse seems to be the case. Higher-level wives are more likely to be employed and wives whose most recent jobs were in the lower-white-collar occupations were less likely to be employed. For example, for wives of Professional I men, only 32 percent of those who were most recently in the Sales III category were currently employed.

Turning to the wives of manual and lower-level service workers, there is evidence that these couples seek to maximize the socioeconomic benefits of the wife's employment rather than ensure that it is at a low, nonthreatening level. This is indicated mainly by the high proportion of such wives in white-collar occupations—particularly the clerical occupations (Table 3). Not counting the Service III men, who are probably better included among non-manual workers (these are men in the protective services), the proportion of wives in clerical occupations varies from 27 percent for the wives of Service IV men to 40 percent for the wives of Craft III men. This compares to a range of 32 to 47 percent for the wives of white-collar men. Hence, the differences are not exceptionally great, especially when we consider the lower educational level of the wives of blue-collar men.⁷ Nor are these clerical wives of manual workers mainly concentrated in the lower-level Clerical IV occupations. This is a relatively unimportant occupational group for the wives of both manual and nonmanual men. Further-

more, data not reported here in tabular form indicate that there is little difference between the median earnings of clerical-worker wives of men in both high and low-level occupations. For example, the 1969 median earnings of full-time, 40+ weeks, clerical-worker wives of Professional I men were \$5,221 as opposed to \$5,086 for the wives of Craft III men, \$5,076 for the wives of Service IV men and \$4,771 for the wives of Operative IV men. The total range in medians was only \$637. However, the typical differences were usually much less—for example, the difference between the medians for the clerical-worker wives of Professional I and Craft III men was only \$135. In sum, there is little evidence that the clerical group is highly differentiated with the wives of white-collar men at the top of a pronounced hierarchy and the wives of blue-collar men clustering at the bottom.

Although small sample sizes raise some problems, the data on labor-force withdrawal indicate that, for the wives of manual as well as nonmanual workers, higher-level occupations seem to exert a strong motivation to stay or return to work (Table 4). Women whose most recent occupation was in the Professional III group provide a particularly interesting case. Very high proportions of these wives were currently employed for most occupation groups of the husband. Among wives of blue-collar or service workers, fully 70 percent or more of the wives in the Professional III group were currently employed for all occupational groups of the husband. Among the wives of white-collar men, however, the situation is somewhat different. If the husband was in a Group III occupation and the wife most recently in a Professional III occupation, then the proportions employed were also relatively high—72 percent for the Professional III wives of Professional III men, for example. However, where the husband was in a Group I or II occupation, the attraction of the Professional III occupation for the wife seems to be less. For example, for the wives of Professional I men, only 50 percent who had most recently been in a Professional III occupation were employed at the time of the census.

⁶ In addition, preliminary results of a multiple regression analysis on the 1970 data indicate that the greater the potential relative socioeconomic contribution of the wife compared to the husband (as measured by relative educational attainments), the more likely she was to be working in 1970.

⁷ For example, while 77 percent of the wives of white-collar men in 1970 had completed 12 or more years of schooling, the proportion was only 48 percent for the wives of blue-collar and service men.

In sum, the data on the employed/labor-reserve status of wives in different occupations indicate that the higher the socioeconomic level of the wife's occupation, the more likely she was to be currently employed. Thus, there is little evidence here of a marked dropping out of wives in potentially status-threatening occupations.

CONCLUSION

The major goal of this paper has been to evaluate critically the theoretical merits of some of Parsons' ideas regarding both the nature of women's socioeconomic role in the family and the determinants of this role. In doing this, the paper has suggested that alternative views concerning women's socioeconomic role and the forces shaping it are more theoretically appealing as well as more consistent with the evidence regarding the extent and nature of women's work behavior.

First, the argument developed here is that we must view families as units in the stratification system, not just as small groups faced with the internal problems of maintaining good relations among members. When we place the family in the context of the larger stratification system, we see that one concern of families will be status maintenance and status enhancement. To the extent this is true, wives can have a potentially valuable socioeconomic contribution to make to their families' competitive position. Therefore, there should be rather powerful, and probably growing pressures for wives to work. Moreover, the evidence certainly indicates that wives have been increasing their labor-force participation for a number of years.

Second, the argument presented here is in agreement with Parsons concerning the importance of status consistency—or at least status compatibility—in family stability. However, from a theoretical point of view, this paper has suggested that Parsons' preoccupation with competition has led to a distorted analysis of the problem of status consistency within the family. An alternative proposed here is that for reasons of status maintenance, status compatibility among family members is

important. This means, however, that negative consequences will ensue if the wife has a much *lower* as well as a much *higher* socioeconomic status than the husband. Hence, depending on her personal qualifications and on market conditions, if she can make a positive contribution to the family's socioeconomic position, her labor-force participation is encouraged. As a result, wives who are working generally will be in occupations that reflect favorably on their families' socioeconomic position as derived from the husband. Data from the PUS indicate that there is considerable empirical support for this position. The socioeconomic distance between the occupations of husbands and wives was particularly pronounced only for wives of men in the highest level occupations. Even here, highly discrepant manual or service occupations are avoided by these wives. Other evidence contradicting the view that a lower-level occupation is a functional necessity of the working wife comes from an examination of the occupations of recently employed wives. Recent workers in relatively high-level occupations are more likely to be currently employed than recent workers in lower-level occupations.

The PUS data indicate that although wives earn considerably less, on the average, than husbands, wives' economic contribution to their families can still be highly important—enough, in many cases, to provide a functional substitute for upward occupational mobility on the husband's part, or to compensate for a husband's relatively low earnings compared to other men in his occupational group. Thus, the unfavorable earnings position of women versus men in our society should not prevent us from recognizing the economic significance of wives' earnings, low though they be.

Regarding competition, I have argued that Parsons greatly overestimated how disruptive it would be if both the husband and wife worked. For a variety of reasons, competition is less of an important factor than Parsons maintained. Working spouses usually operate in different occupational and/or organizational work groups which protect them from direct

competition with each other. Conflicts are most likely to arise between husbands and wives in very high-level occupations. However, high-level couples—and especially high-level working wives—are a relatively small segment of the population. Finally, this paper suggests that rather than traditional sex-role norms being a teleological result of the conflicts inherent in the two-worker family, they may be a major factor in the cause of such conflicts when the wife works.

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THE CHANGING AMERICAN FAMILY FROM THE PREINDUSTRIAL TO THE INDUSTRIAL PERIOD: A FINAL REPORT

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This report represents the completion of a content analysis of magazines designed to examine the American family from 1741 to 1865. The final report compares the last period, 1850-1865, with previous periods and includes a new discussion of family conflict in magazine content. It is suggested that the period 1850-1865 may have been a period of significant family change. The theoretical synthesis of findings for the 1741-1865 period is presented. Significant variables include the family as a change agent and the role of ideology and developmental factors (i.e., urbanization and industrialization). Some observations on causal relationships are made and basic limitations in the use of literary sources of data are noted.

This report represents the completion of a content analysis of magazines designed to examine the American family from 1741, essentially a preindustrial, pre-urban period, to 1865, a period in which large-scale industrialization was well under way. Three earlier reports (Lantz et al., 1968; 1973; 1975) dealt with the important findings for each of the periods. The essential focus of each of these studies was to examine power patterns between the husband and wife, the extent to which romantic love was important, motivations for marriage, and sanctions for sexual deviance. The results of the final period, 1850-1865, deal with each of the above plus an analysis of the

additional dimensions of conflict between parents and conflict between parents and children. These facets of family dynamics had not appeared in the magazine content in our earlier investigations, and this appearance of family conflict represented a new and possibly significant element to explore.

The purpose of this final paper is to compare the findings of the three earlier periods with the final period, 1850-1865, and to present the theoretical implications of the entire project. The data for each of the earlier time periods was taken from Mott (1930), who has broken the development of magazines into distinct periods. As described in our earlier re-

ports, each period has its special social, political and economic characteristics. The final period is characterized by Mott (1938) as one of significant change in the American magazine, as well as in American society. Nearly all important novelists, poets and critics made frequent contributions to magazine literature. Mott indicates that during this period magazines were a major force in the diffusion of democratic and equalitarian ideals. Content of the periodicals cut across social backgrounds and classes more than in earlier periods (Mott, 1938:46-101).¹

Perhaps the most noteworthy characteristics of magazines for the period 1850-1865 were the intellectual issues and debates which emerged. There was a general questioning of the position and authority of the male. The interpretations of Marxist writings, along with the writings of utopian socialists, appeared in literary works and magazines. Utopian efforts sprang up in America during the period and some, such as the Oneida community, had their own magazine, *The Oneida Circular*, which expounded spiritual, sexual and economic equality (Mott, 1938:207). The Civil War also brought into focus greater demands for social, political and legal equality. In addition to the slavery issue, women's rights received considerable attention. Frequently women's rights and the civil rights of blacks were discussed in the same article, indicating a more encompassing view of equality (Mott, 1930:50). The social issues of the period 1850-1865 were significant for changes in American society as a whole, and the changes could have had a bearing on what was happening to the family.

Source, Method and Procedure

Mott (1938:218) lists 71 important magazines for the 1850-1865 period.² Of these

magazines, 36 were included in our analysis.³ Of the 35 magazines not represented in our sample, at least 21 magazines (possibly more) had specialized content that clearly was not reflective of family patterns, one magazine emphasized foreign literature and thirteen magazines could not be located. The sample for this last period consisted of all available issues of the 36 magazines that were published from 1850-1865. All available issues were examined in order to eliminate any sampling error. Our data for the 1850-1865 period are based on our observations from a total of 6,559 issues which we examined.⁴

The procedure for examining the magazines was the same as for earlier periods. The table of contents for each periodical issue was carefully scanned. Article titles or descriptions were used as the basis for initial selection; any article which appeared to involve one of the topics or issues under consideration was included. We were guided by titles and we also examined the first few paragraphs of the articles because titles were not always informative enough. If the first page of the article contained information on one of the variables we were coding, the entire article was examined and the appropriate tabulations were made. If none of the topics was found on the first page, the article was dismissed. Reliability was established by applying the criteria for classification in a consistent manner. The schedule and

influential magazines of each period have been examined. For a detailed discussion of the historical periods designated by Mott, the nature of a magazine and the character of an important magazine, see Lantz et al. (1968:414-6).

³ The titles and dates of these magazines for the 1850-1865 period can be obtained by writing to the senior author.

⁴ It is important to keep in mind that the number of issues examined for the different time periods varies considerably. Even though the earlier periods were considerably longer in terms of the length of years covered, the number of issues published and examined in these earlier studies was much smaller. For the various periods, the number of issues represented in our reports was: 1741-1794, 546 issues; 1794-1825, 2,373 issues; 1825-1850, 5,334 issues; 1850-1865, 6,559 issues. For a discussion of the tremendous expansion of periodicals during the 1800s, see Mott (1930).

¹ See Mott (1938:10-3) for more extended comments on the circulation of periodicals during this period.

² It should be emphasized that the populations in all four of our studies are based on the list of magazines designated as important by Mott. It is only because of his work that the reasonable and significant assumption can be made that the most

specific categories facilitated this procedure.⁵

The unit of analysis was the explicit discussion of one of the variables. While more than one discussion of the same variable could come from the same article, this seldom happened except in the case of discussions of power and romantic love. A schedule was used which allowed for the duplication of the tables used in the earlier studies. These tables concern power in the man-woman relationship, romantic love, motivations for marriage, and sanctions involving premarital or extramarital sex. In addition, tables were constructed to include discussions of conflict in husband-wife and parent-child relationships.

Our analysis for the 1850-1865 period involved an examination of the frequency and proportion of observations presented in the various tables.⁶ With regard to comparisons between periods, we have reported estimates of the average number of issues examined for any one particular type of discussion (e.g., one discussion of romantic love for every 5.4 issues examined). This method is useful in providing a basis for determining the relative amount of attention devoted to particular topics in the magazine literature during the four periods.⁷

⁵ For a discussion of Sebald's observations on this approach, see Lantz et al. (1968:416).

⁶ We also recorded the sex of the magazine editors and the authors of the discussions which we examined. Our analysis indicated that our study displayed a selective preference for female authors, but that male editors were clearly predominant. Thus, while female authorship may have skewed our findings in one direction, the predominance and control which male editors exercised over the final version of the published material would seem clearly to offset the possibility of serious bias resulting from an overrepresentation of female writers.

⁷ In our earlier studies, chi-square goodness-of-fit tests were used to facilitate comparisons within these tables. Since we were unable to obtain some of the magazines for the 1850-1865 period, the assumption of a random sample could not be justified and we have not reported the significance levels of the findings.

With regard to estimates of the average number of magazines examined per unit type of discussion, some cautions are necessary. Magazine content became increasingly specialized in the nineteenth century, and some of the periodicals for the latter periods contained relatively few discussions of the

Power in the Man-Woman Relationship

Various aspects of the power relationship between males and females during the 1850-1865 years were investigated. Power, as in previous studies, was defined as the ability of one individual to dominate another or others, to coerce and control them, obtain obedience, interfere with their freedom and compel their actions in particular ways. Power was coded as being exerted overtly by the male, overtly by the female, subtly by the female, or by males and females cooperatively.⁸

The data from our literary analysis of power in the man-woman relationship are presented in Table 1. Our attention was first directed toward a comparison of those discussions in which power was exerted overtly by the male and female. During this period, we recorded a total of 227 such discussions in the male category and 757 discussions in the female category. When the male-female totals were examined in the five individual categories of *existing* power, our data revealed that there was little difference in the number of discussions involving males (N=77) and females (N=54) as the source of power in the general power category. In regard to the four remaining categories, there were considerably more discussions of overt female power over morality, in courtship situations, over finances and in other areas. With respect to power in other areas, discussions centered on female power in the literary, intellectual and educational fields as well as in domestic and child-rearing situations.

Shifting our focus from existing power relationships to those discussions which *advocated* that overt power should be held by males or females, Table 1 indi-

issues which we analyzed in our research. It is also possible that, as magazines expanded from quarterlies to monthlies, bimonthlies or weeklies, the size of the issues decreased in some cases. While we do not believe that these changes seriously distort the rates we have reported, it is important to note that these rates should be interpreted only as crude estimates of the relative amount of attention given to different topics in the various time periods.

⁸ Instances of subtle male power were rarely discussed in the magazine literature and the few discussions we observed have been omitted from our analysis.

Table 1. Various Dimensions of Power in the Man-Woman Relationship and Source of Power by Character of Discussion (Fiction or Nonfiction), State of Power (Advocated or Exists) during 1850-1865 (in Number of Discussions)

Source of Power	Power in General 1850-1865						Power over Morality 1850-1865						Power in Courtship 1850-1865						Power over Finances 1850-1865						Power in Other Areas 1850-1865						Row Sums
	Advoc.			Exists			Advoc.			Exists			Advoc.			Exists			Advoc.			Exists			Advoc.			Exists			
	F ^a	N ^b	T ^c	F	N	T	F	N	T	F	N	T	F	N	T	F	N	T	F	N	T	F	N	T	F	N	T				
Overtly by Male	12	73	85	27	50	77	0	1	1	1	2	3	1	0	1	17	14	31	0	1	1	2	3	5	2	10	12	1	10	11	227
Overtly by Female	7	144	151	23	31	54	3	9	12	2	126	128	0	1	1	47	9	56	2	8	10	9	12	21	11	104	115	15	194	209	757
Subtly by Female	5	14	19	20	30	50	1	9	10	2	8	10	2	2	4	90	20	110	1	0	1	3	2	5	2	10	12	8	29	37	258
Subtly by Male	-	-	-	-	-	-	-	-	-	-	-	-	-	0	1	1	9	0	9	-	-	-	-	-	-	-	-	-	-	-	10
Mutual Co-operation	4	40	44	14	3	17	0	6	6	0	21	21	0	1	1	1	0	1	1	1	1	2	0	1	1	0	1	1	1	2	96
	N=1348																														

* Fiction, ^b Nonfiction, ^c Total.

cates that a majority of the discussions advocating power in general involved women (64%) as compared with those discussions advocating power in general on the part of men (36%). While the number of discussions advocating female power over morality was not large ($N=12$), we recorded only one discussion advocating male power over issues related to morality. We also observed considerably more accounts calling for overt female power over financial matters as well as in the miscellaneous category.

Overt Male Power Compared with Overt and Subtle Female Power

Evidence for a shift in the distribution of power between men and women can be given additional credence by examining what was taking place in the area of subtle female power. As in our previous studies, subtle female power refers to situations where the female is in control even though the male may be unaware that he is being manipulated. Since we were basically interested in the overall power of the male and female, the discussions which indicated either overt or subtle female power were combined to permit comparison with discussions of male power.

We recorded 227 discussions of overt power by the male and 1,015 discussions of female power (subtle and overt) across all five categories (see Table 1). When comparing the male-female totals within the *Exists* columns of the individual categories, we found that there were slightly more discussions of female power in general ($N=104$) than male power in general ($N=77$). In the four remaining categories, our analysis revealed considerably larger percentages of discussions of female power in each case: power over morality (99%), power in courtship (81%), power over finances (84%) and power in other areas (96%).

We also compare data involving discussions which advocated overt male power with those which favored overt or subtle female power. With respect to the discussions of power in general, a greater number of discussions advocated overt and subtle female power ($N=170$) than male power ($N=85$). Almost all of the dis-

cussions concerning power over morality involved women. While the total number of discussions advocating power in courtship was rather small, our findings revealed a considerable majority of discussions advocating female power in both of the remaining categories: power over finances (92%) and power in other areas (91%).

A disproportionate share of these discussions came from predominantly female- and family-oriented magazines. Thus, it is possible that these periodicals might have exaggerated the changing roles of the sexes as well as the influence and power of women during this period. Mott (1938) notes, nevertheless, that these periodicals endured because they reflected the ideas, interests and concerns of the magazine subscribers and readers.

Perhaps the most compelling evidence of changing ideas about the power of men and women in mid-nineteenth-century America emerges from the variety of discussions of overt female power in the educational, intellectual, literary, political and legal fields.⁹ The intense debate over women's rights during these years would seem to demonstrate that the threat to the traditional order was often taken quite seriously. One male writer, for example, was so incensed over the state of affairs that he advocated the inauguration of a "Man's Rights Convention" to combat

⁹ If we had anticipated such a large number of discussions of female power in so many different areas during this period, we would have been better prepared to categorize and tabulate these discussions within more specific dimensions and areas of influence. Since individual discussions were not identified at the outset of the research, the following percentages should be viewed as approximate estimates of the number of discussions in the miscellaneous category. Discussions of female power in the *Existent* category included: literary power, or the ability of women to influence others through literature (26%); intellectual power, or the influence of women through reasoning, knowledge and intelligence (26%); power in formal education, especially as teachers (24%); power in child-rearing (20%); power in terms of legal rights or political influence (4%). Discussions *advocating* female power consisted of the following: political power and legal rights to vote, run for office, hold property, etc. (40%); intellectual power (21%); power in child-rearing (18%); power in education (15%); literary power (6%).

Table 2. Total Row Sums of Power in the Man-Woman Relationship by Source of Power and Time Period

Source of Power	Time Period			
	1741-1794	1794-1825	1825-1850	1850-1865
Overtly by Male	136 (42%)	33 (29%)	199 (31%)	227 (17%)
Overtly by Female	42 (13%)	15 (13%)	147 (22%)	757 (56%)
Subtly by Female	105 (33%)	35 (31%)	193 (30%)	258 (19%)
Mutual Cooperation	38 (12%)	30 (26%)	113 (17%)	96 (8%)
	N=321(100%)	113(100%)	652(100%)	1338(100%)

"force with force" (*Littell's Living Age*, 1859:774-7).

Power in Man-Woman Relationship: a Comparison of Trends

The following analysis may serve to clarify the changes in family power which provoked so much debate in the second half of the nineteenth century. A comparison of the number of discussions of power and the proportion of discussions in each category for the different time periods is reported in Table 2.

We should consider first the large number of discussions dealing with power in the man-woman relationship in the 1850-1865 period. In fact, we recorded more discussions of power during this fifteen-year period (N=1,348) than in all three earlier periods combined (N=1,086).¹⁰ This finding may be especially important, given that we were not able to examine all magazines for the period. The difference in number of discussions was largely accounted for by the high number of nonfictional discussions recorded from 1850-1865 (N=1,002)

compared with nonfictional discussions for all previous periods (N=455). The attention which nonfiction writers devoted to commentaries on power in the man-woman relationship would seem to provide an important index of the interest and concern which these issues provoked in the mid-century years.¹¹

When we focus attention on the actual expressions and distribution of power, three distinct trends emerge in our literary analysis. The first trend involves a change in the expression of power. In the earlier periods (especially from 1741-1825), authority was normatively ascribed to the male, but magazine content revealed that power was not exclusively exercised by the man. Through subtle persuasion and indirect means for gaining influence, women were able to exert considerable power in matters of morality, courtship, child-rearing and other domestic situations. From 1825 to 1850, the expression of power began to change in the magazine discussions. As the percentages in Table 2 demonstrate, a trend from subtle to overt female power was greatly intensified in the 1850-1865 period.

The second pattern revealed in our content analysis for the several periods indi-

¹⁰ While we are impressed by the considerable amount of attention given to discussions of power in the man-woman relationship during the 1850-1865 period, the absolute number of discussions can be somewhat misleading. The absolute number and rates for the various periods are useful here: 1741-1794, 321 discussions or about one discussion for every 1.7 issues examined; 1794-1825, 113 discussions or about one discussion for every 21.0 issues examined; 1825-1850, 652 discussions or about one discussion for every 8.2 issues examined; and 1850-1865, 1,338 discussions or about one for every 4.9 issues examined. On this basis, the first and last periods represented the most attention to power in the man-woman relationship in terms of the issues which we analyzed.

¹¹ The fantasy and sentimentality of many fictional accounts often present monumental problems in making valid inferences about social reality. The importance of the nonfictional discussions in the later period stems from the greater probability that these discussions reflect realistic issues and situations in the man-woman relationship during the mid-nineteenth century. The substantial number of discussions involving existing power arrangements (N=858) compared with those advocating different sources of power (N=490) would further reinforce our conclusion that important changes were in progress during these years.

cates that female power grew in scope, involving new areas from one period to another.¹² From 1741–1825, discussions of female power were primarily restricted to the family environment and courtship situations. Female power in general or women's rights and influence outside the home were seldom discussed and rarely advocated in the periodical literature of the earlier periods. After 1825, however, these issues and topics became more common. The taken-for-granted nature of traditional roles was challenged by a growing controversy over the rights and responsibilities of women in the world outside the area of the family.

The third trend in our data involves discussions of mutual cooperation in the man-woman relationship. The data in Table 2 indicate that the percentage of discussions of mutual cooperation reached a peak from 1794–1825, and then declined considerably in relation to discussions of other types of power in the periodical literature from 1825–1865. This trend was especially evident in the magazine literature from 1850–1865 when only 8% of the discussions were classified in the mutual cooperation category. The explanation for a decline in the proportion of discussions involving mutual cooperation is interesting and deserves special comment, especially since we previously identified the pattern of mutual cooperation as one aspect of a move away from patriarchal dominance of the family. If one views the changes in family power in terms of an ongoing process, the concern for mutual cooperation in the eighteenth- and early nineteenth-century periodicals could have represented one phase of a move toward greater female power. By the middle of the nineteenth century, however, the emphasis for women was less on mutual cooperation, which may have been achieved in some respects, and more on additional dimensions of female power. The phenomenon of rising expectations on the attainment of power for women may also have been operative.

¹² The following discussion is based on an analysis of the distribution of power in the man-woman relationship. The specific data concerning the respective areas of power can be found in the three articles dealing with earlier periods.

Women had been achieving greater authority and power in earlier periods. Yet changes in the distribution of power and authority, once started, may have moved at a relatively slow pace. This tension for women concerning the realization of some measure of power, in a context in which the process of further power changes was inevitably slow, may have become intensified by the middle of the nineteenth century. This may help explain why the matter of the distribution of family power and authority was the dominating issue in magazine content of family life in the 1850–1865 period. If this interpretation is valid, this period could have been a crucial one in which basic changes in the power structure of the family were taking place, changes which set the direction and orientation of the family for a long time. In this sense, some of the basic changes which led to the contemporary democratic-companionship family of the twentieth century were greatly accelerated in America in the 1850–1865 period.

Conflict in Family and Marital Relationships

The data we have presented on changes in family power structure could have additional meaning if we examine the area of conflict for the period 1850–1865. The presence of conflict may have been a factor which helped to produce change.¹³

The same procedures for the selection of articles were used as in our analyses of power, romantic love and so forth. Overt conflict was defined as a situation (or situations) in which sharp disagreement and tension resulted from antagonistic interests, ideas or behavior. Covert conflict involved the presence of latent tensions, but tensions not openly acknowledged by any of the members involved. The data from our analysis are shown in Table 3.

Overall, our findings show 197 explicit

¹³ Since we did not collect information on family conflict in the earlier periods, we do not have a baseline to compare our findings for the 1850–1865 period with earlier years. With regard to marital conflict, however, a number of other writers (Furstenberg, 1966; Barnett, 1968) have noted that the topic was not openly discussed to any large extent until the middle of the nineteenth century.

Table 3. Various Dimensions of Conflict in Husband-Wife and Parent-Child Relationships by Character of Discussion (Fiction or Nonfiction) and Type of Conflict (Overt or Covert) during 1850-1865 (in Number of Discussions)

Source of Conflict	Conflict in General 1850-1965			Conflict over Domestic Roles 1850-1865			Conflict over Finances 1850-1865			Conflict in Other Areas 1850-1865			Row Sums
	F	N	T	F	N	T	F	N	T	F	N	T	
Overt between Husband and Wife	38	22	60	4	4	8	8	2	10	22	2	24	102 (52%)
Covert between Husband and Wife	23	14	37	5	1	6	5	0	5	9	0	9	57 (29%)
Overt between Parents and Children	7	5	12	0	0	0	0	0	0	13	5	18	30 (15%)
Covert between Parents and Children	3	4	7	0	0	0	0	0	0	1	0	1	8 (4%)
	N=197 (100%)												

discussions of family conflict for the 1850-1865 period. Nearly half (49%) of all these observations involved discussions of general conflict between the wife and husband. Some conflict over domestic roles and finances was discussed by the magazine writers, but few distinct patterns emerge with respect to other specific situations of marital conflict. In fact, we were impressed by the variety of alleged causes for marital troubles (lack of communication, drinking, finances, personality conflicts, in-law problems, extramarital affairs) and the similarity of these themes to contemporary complaints and explanations of marital disruption.¹⁴ The difference between discussions of overt conflict (N=102) and covert conflict (N=57) in husband-wife relationships could suggest that marital tensions were brought out into the open during this period. Since the majority of these discussions (72%) came from fiction, however, this interpretation should be viewed cautiously.

Nevertheless, the literary concern with conflict would seem to reflect certain telling anxieties about the quality of marriage and family life in the mid-nineteenth century. The rhetorical question posed by the editor of the *Continental Monthly* (May, 1864:23) typified this concern: "Do any of you who may be our readers know half a dozen happy families in your circle of friends?" A writer in the *Boston Quarterly Review* (October 1859:492) took the matter even more seriously: "The family, in its old sense, is disappearing from our land, and not only our free institutions are threatened, but the very existence of our society is endangered." Magazine literature during the 1850-1865 period began to expose problems in marriage and family life which previously had been confined to the privacy and secrecy of the home. In this respect, recognition of marital and family conflict may have had the effect of generalizing the nature of the problem and weakening the sense of personal guilt about marital incompatibility.

¹⁴ In many ways, the sources of conflict noted in nineteenth-century magazine literature bear a remarkable resemblance to the complaints of modern divorced women reported by Goode (1956:ch 10).

Table 4. Various Dimensions of the Romantic Love Complex and Character of Discussion (Poetry, Other Fiction and Nonfiction) during 1850-1865 (in Number of Discussions)

	1850-1865			
	Poetry	Other Fiction	Nonfiction	Total
Idealization of the loved one	303	161	16	480 (40%)
The one and only	144	162	7	313 (26%)
Love at first sight	15	60	4	79 (6%)
Love wins out over all	24	153	1	178 (15%)
Glorification of personal emotions	110	47	2	159 (13%)
	N=596	583	30	1209 (100%)

*The Romantic Love Complex:
A Comparison of Periods*

We now shift our focus to another dimension of the relationship between the sexes during the 1850-1865 period. As in our earlier studies, romantic love was defined in terms of five dimensions: (1) idealization of the loved one, (2) the one and only, (3) love at first sight, (4) love wins out over all and (5) glorification of personal emotions. The results of our analysis for 1850-1865 are presented in Table 4.

While discussions of romantic love were quite common in the magazine literature during this period, they appeared somewhat less frequently than in the previous periods. For the two earlier periods, we recorded 337 discussions of romantic love in the 1741-1794 period, or about one discussion for every 1.6 issues and 761 romantic love discussions in the 1794-1825 period, or about one discussion for every 3.1 issues examined. For the 1825-1850 years, we recorded 2,261 discussions of romantic love; this is equivalent to one discussion for every 2.4 issues. In the latter period from 1850 to 1865, 1,209 discussions were observed, or about one discussion for every 5.4 issues. A similar decline of interest in romantic love also has been documented by Hall (1976), who undertook a content analysis of novels, and by Hendrix and Peters (1976), who examined nineteenth-century ballads.¹⁵ The waning preoccupation with romantic

love in the periodical literature would also correspond with the gradual transition from romantic idealism to realism in American literature. As literary critics began to plead for more " 'veritable and veracious' " accounts of " 'the way life really is,' " the emotional sentimentality of earlier periods may have become less appealing (Stovall, 1967)

*Motivations for Marriage:
A Comparison of Periods*

As in the previous articles, three motivations for marriage were considered: happiness, wealth and status. We also noted whether these motivations were held by the individuals entering marriage or by their parents. The data for the 1850-1865 period are presented in Table 5.

Although we recorded 344 discussions concerning various motives for marriage during this period (about one discussion for every 19 issues examined), there was a considerable decrease from the prior period when 1,315 such discussions were found (about one discussion for every four issues examined). This finding would seem to confirm our earlier observation that the magazine literature from 1850-1865 reflected a growing concern with problems of marital and family life (power and conflict) rather than more peripheral, sentimental issues (reasons for getting married and romantic love).

While a general decline in discussions of marital motives is evident, the magazine data are otherwise consistent with previous findings which indicate a strong emphasis on happiness as the predominant motive. Of the total number of discussions, 81% showed this prevailing

¹⁵ Detailed information on these studies is available by writing to Stephen Hall, Department of Sociology, Appalachian State University, Boone, North Carolina or Lew Hendrix, Department of Sociology, Southern Illinois University, Carbondale, Illinois.

Table 5. Marital Motive and Holder of Motive by Character of Discussion (Fiction and Nonfiction) during 1850-1865 (in Number of Discussions)

1850-1865							
Motive	Fiction			Nonfiction			Total
	Parent	Ego	Not Specified	Parent	Ego	Not Specified	
Happiness	9	177	88	0	3	3	280 (81%)
Wealth	10	28	4	0	2	0	44 (13%)
Status	6	4	7	2	1	0	20 (6%)
	N=25	209	99	2	6	3	344 (100%)

concern, while 13% indicated wealth and only 6% indicated status. Parental desires were occasionally considered, but the most common motivation was personal happiness. This finding would indicate an important index of individualism on the part of persons entering marriage—with marriage being primarily a matter of personal choice and happiness. Parental interests were not only less important for the most part, but tended to reflect a more traditional view of marriage as a family affair—a means for acquiring or transmitting property, wealth and status through the family line.

Table 6 provides the data from our content analysis of motivations for marriage for all the periods we have examined. As noted earlier, happiness on the part of ego has accounted for a large proportion of the discussions in each period. Relative to economic and utilitarian motivations, the emphasis on personal happiness is especially significant in the final two periods. A comparison of the distributions of parent and ego motives provides evidence for generational differences in all four studies. Although personal happiness has been the most common topic with regard to the person entering marriage, wealth and happiness have accounted for most of the discussions of parental motives; but the pattern varies considerably from one time period to another.

Perhaps the most noteworthy trend in these data concerns the proportionate decline of discussions of parental desires during the final two periods. In our first two studies, 44% and 33% of the discussions were concerned with parental motives regarding the marriage of their children. In the last two periods, only 8% of the discussions examined from 1825-

1850 and 11% of the discussions in the final period treated the parents' considerations.¹⁶ These literary findings suggest important changes in the basis of marriage as well as in the role which parents and marriage partners played in the process of mate selection. One finds that from 1741 to 1865, we have moved to a position in which the motives of the couple assume more importance in relation to the influences which parents may have had. These findings lend additional credence to the basic family changes which we have previously examined.

Sexual Standards

The magazine content also was examined for evidence regarding the normative sexual structure of the period. Since there were few direct references to the sexual mores, we employed indirect indices in the form of sanctions invoked in premarital or extramarital relations. The three categories of sanctions were punishment, ostracism and sympathy. We also distinguished between those discussions which indicated that a particular sanction *should be* implemented toward a person and discussions in which a particular sanction *actually was* implemented toward an individual. Hence, the three forms of sanctions were classified into one of two categories—attitudes or behavior.

While love, marriage and family problems were important themes of discussion and controversy in the magazine literature of the 1850-1865 period, we found relatively few discussions of sexual sanctions,

¹⁶ These percentages are not presented in the tables, but they can be easily calculated from the data presented in Table 6.

Table 6. Marital Motive (Happiness, Wealth or Status) and Holder of Motive (Ego or Parent) by Time Periods (in Number and Percentage of Discussions)

Ego's Motive	Time Period			
	1741-1794	1794-1825	1825-1850	1850-1865
Happiness	57 (76%)	5 (45.5%)	1112 (92%)	180 (84%)
Wealth	15 (20%)	5 (45.5%)	75 (6%)	30 (14%)
Status	3 (4%)	1 (9%)	17 (2%)	5 (2%)
	N=75 (100%)	11 (100%)	1204 (100%)	215 (100%)

Parent's Motive	Time Period			
	1741-1794	1794-1825	1825-1850	1850-1865
Happiness	17 (29%)	2 (40%)	52 (51%)	9 (33%)
Wealth	30 (51%)	3 (60%)	28 (27%)	10 (37%)
Status	12 (20%)	0 (0%)	22 (22%)	8 (30%)
	N=59 (100%)	5 (100%)	102 (100%)	27 (100%)

sexual deviance or sexual behavior. There were only 41 discussions of the various sanctions either advocated or implemented against persons engaged in sexual misconduct.¹⁷

Perhaps the most important finding for the 1850-1865 period involves the discrepancy between sanctions against male and female sexual deviance. Practically all the discussions in the male categories involved punishment or ostracism, while sympathy was the most frequent response to sexual deviance on the part of the female. This finding can be best understood within the framework of nineteenth-century notions of sexual morality. Walters (1974) and many other commentators have shown that the Victorian lady was exalted for her chastity virtue and innocence. Mid-century males, on the other hand, were constantly advised to keep vigilance over their fierce passions, animal instincts and destructive impulses.

A second observation about the data from our content analysis deserves elaboration. There appears to be a rather sharp discrepancy between attitudes (discussions that sanctions should be implemented) and real behavior (discussions

of the actual implementation of sexual sanctions). In general, the attitudes expressed by the magazine writers tended to be more liberal than the discussions describing the actual enforcement of sexual sanctions. To the extent that these limited findings reflect changing ideas about sexual deviance in the nineteenth century, they may provide an important background for the revolution in sexual behavior during the twentieth century.

In comparison with magazine literature of the eighteenth century, the data for 1850-1865 continue to reflect a lack of literary discussion of sexual sanctions in the nineteenth century. For the first period investigated, we recorded about one discussion for every four issues in the 1741-1794 period and found considerable literary evidence to document the existence of the double standard. After 1794, however, such discussions have become much less common. In the 1850-1865 period, for example, there was approximately one discussion for every 160 issues.¹⁸ It is difficult to determine the attitudes toward sexual deviance solely on the basis of an examination of popular magazines. The general tendency to avoid discussions of sex during these years may indicate that this period was characterized by change and confusion for both sexes. Schlesinger (1966:32), for example, suggests that men were too preoccupied

¹⁷ We recorded one discussion of sexual sanctions for approximately every 160 issues examined in the 1850-1865 period. In comparison with other topics such as power, conflict and romantic love, discussions of sexual behavior and attitudes toward sexual deviance were difficult to find in the magazine literature.

¹⁸ The discussions of sexual sanctions were so infrequent during the 1850-1865 period that we have not included the tabular presentation of findings.

with economic success to devote much time to passion. Women were entering a period in which reason and intellect were valued. Schlesinger maintains that these factors resulted in prudishness about sex, as well as considerable frustration for both sexes. This issue remains an important one for further investigation.

Discussion

The content analysis of magazines from 1741 through 1865 suggests that certain aspects of the family may have been undergoing change. While we recognized that the inability to include some magazines of the 1850–1865 period represented a problem, independent studies by Hall (1976) and Hendrix and Peters (1976) generally confirm the observations we shall make. These studies suggest that traditional male and female roles (especially in regard to the expression and allocation of power) were far more complicated than our own time-bound stereotypes of early American family structure would lead us to believe. Equally important, however, is the realization that ideas about the man-woman relationship may have been undergoing steady modification during the preindustrial period. While the dislocation and blurring of roles was probably intensified as social and technological changes accelerated, the transition in everyday marriage and family life probably occurred over a relatively long period of time.

If we reexamine each of the previous studies, we can observe a pattern that may be relevant. The magazine content in each period suggests changes in the distribution of power, in motivations for marriage, in the importance of romantic love and in attitudes toward sexual deviance. However, the relationship of each variable to the others appears to undergo change, with the distribution of power emerging as the variable of greatest concern in our final period, 1850–1865. If magazine content was an accurate reflection of family change, one might postulate that the process of family change proceeded first in those areas least antagonistic to the power structure of the traditional family. There is no evidence of major at-

tacks on the structure of the family in magazine content in earlier periods. Instead, we find important but lesser changes in the dimensions that we have examined. Yet, nowhere in any of the previous periods studied is there any evidence for drastic efforts to change the family such as those that appeared in the 1850–1865 period. This could mean, first, that earlier family changes set the stage for significant family change in the 1850–1865 period. Second, the data suggest that all the important variables necessary for basis family change in family power structure were not present prior to the 1850–1865 period.

If one accepts that these magazines reflected changes, then it is necessary to explain why these changes occurred. Invariably the linkage between broad macroscopic events and change in social structures is undeveloped. Moreover, the independent and dependent variables in social analysis involving lengthy periods of time do not relate in the same way to one another. Thus, the family, in order to change, can be viewed at one point in time as dependent on external change agents; yet at another point in time, the family can be seen to be an initiator of change that influences external events. The problem is compounded further by the fact that external change agents can differ in their relationships to each other over time. At one point, industrialization may have been responsible for the development of particular ideologies; at another point, ideologies may have had an impact on industrialization. There are at least three major variables that seem to have a bearing on American family changes during the period of our studies—the family structure itself, social ideology and economic development, especially urbanization and industrialization.

The Family

Social structures like the family are capable of changing themselves because they contain some provision for the introduction of new perspectives and values. No single type of family structure can solve permanently the problems which emerge from a given family structure. The

structure of the family at any given point in time achieves some goals at the expense of other possible goals. For example, a patriarchal family of the type we believe existed in America in 1741 achieved integration through an unequal distribution of power. Hence, this patriarchal family carried within it potential ingredients for dissatisfaction among family members; the result was a challenge to patriarchal authority. This challenge, as revealed in our data, took the form of a pattern of subtle female power in particular areas of family life, mutual sharing of power and, in time, even overt power on the part of the female. Such efforts to produce change are not unusual from a sociological perspective. No social structure with a markedly disproportionate distribution of power is devoid of efforts on the part of those without power to change such an arrangement. Such was the case even under traditional caste systems when external ideologies and influences were at a minimum.

Similarly, the goal of imposing a utilitarian, materialistic basis for mate selection may be accomplished at the expense of minimizing the significance of romantic love. Parental domination of mate choice is achieved by negating individual choice. The data for all of our studies suggest that adaptations were probably made by family members in order to cope with the unequal distribution of power. Efforts were made to assert the right of individuals to choose marriage partners for themselves. This dialectical tension in family structure—the imbalance it may create and the alternative ways of dealing with such an imbalance—is an important and neglected component of family change. Nevertheless, internal family variables are insufficient to account for family change.

The Role of External Sources of Family Change

The relationship of a developing economy to family change can be specified conceptually as follows. Traditional family structures encounter difficulties in remaining static in a society in

which the economy is developing because such development creates opportunities for people to introduce change from the outside. Throughout the period of our investigation, the American economy continued to grow and expand—initially through agriculture, trade and commerce. Family change was probably facilitated during America's agricultural phase by the availability of cheap, rich land on the frontier. Young married people who migrated could develop their own patterns of family life without the constraints of their elders. Often the nature of the hardships necessitated a restructuring of traditional family roles. Moreover, frontier areas, with their disproportionate sex ratio favoring women, often created opportunities for the redistribution of roles and power.

In the nineteenth century, the economy began to shift from an agricultural to an industrial base. By 1865, an urban-industrial complex was well underway. This resulted in the rapid growth of cities, an unprecedented need for labor, and freedom from traditional role constraints.

The success of the American economy could have been important in affecting family change in yet another way. One of the reasons family structures in underdeveloped economies change very slowly is that existence is precarious. Thus, people may come to believe that their existing family structure is the only one which facilitates survival. Under these circumstances, change in family structure can be perceived as a threat to the family's capacity to survive. Once a society reaches a stage of economic development where its main concerns are not tied solely to economic survival, and an economic surplus is possible, changes in family structure can be considered. Families then may be more readily able to accept alternative structural arrangements, either those which are generated from within or those from without. The perceptions and energies of people associated with old arrangements can be redirected toward new possibilities. The expansion of the American economy, and the opportunities it provided, made it possible for the family to face the development of new patterns

of family life and could well have facilitated and hastened the possibilities for change.

The Role of Ideology

The development of ideological issues which could have a bearing on changes in family structure have their origins in two major sources. The eighteenth century was a period of considerable political and religious ferment in America. Conventional political structures in the form of English control of the colonies and conventional religious structures and authority came under widespread attack. New trends representing a concern for social justice continued into the nineteenth century in the form of the issue and women's rights.

By the late 1840s, the women's movement became more radicalized, addressing itself not only to the lesser issues of dress and temperance, but also to basic questions about the entrapment of women into the roles of wives and mothers. According to Duberman (1975:11), the efforts of women on behalf of blacks resulted in questions about their own role in society since women often became activists on behalf of minorities, and this kind of activity made women more conscious of their own lowly status.

The second major source of change in ideology arose from the contradiction of wealth and poverty in an economically successful society. The pursuit of materialism was identified as creating false values, and ideological issues arose. The emergence of the utopian community, especially the reform communities prominent in the 1850-1865 period, represented an effort to deal with these concerns. As Muncy (1973:6-7) indicates, the utopian reform communities, which reflected a strong element of socialism, represented a disenchantment with the American family. Reformists believed that family life, at the time, was destructive to personal and social development and was too competitive, beset by rivalries and jealousies. They saw the economy as playing an important role and believed that an economic change could result in a change in

the structure and organization of the family and in the fulfillment of the self and the human personality. These utopian communities could develop in America at this time because the climate of concern for matters of social justice for all groups who felt underprivileged was already present in America. While the communities themselves may have had little lasting impact on the direction of family change, they did help to emphasize certain major ideological themes associated with the search for social justice (political and economic) and for human rights for all groups. However diverted from these principles American society became from time to time, these themes were recurrent. While we have conceptualized a possible relationship between ideology and family change, we cannot delineate or specify the conditions under which ideology resulted in family change, nor do we have data which bears on this question. It is, however, important to speculate on the implications.

We have explored the role of the family as an agent of change itself and we have dealt with the economy, including industrialization and urbanization, and ideology. Nevertheless, while change variables from the family, from ideology and from an agricultural economy were present from the beginning, urbanization and industrialization, both aspects of economic development, did not develop fully in America until much later (Lantz et al., 1975).

An important problem that remains is whether any statements about causal relations can be made. Clearly, the family in America was in a condition of change from the very first, and such change was part of a greater process of change that started much earlier in Europe. Humanistic ideologies, the growth of cities, and development of industry all undoubtedly played a part in such change at different times.

An examination of the European experience would lead one to believe that, from the standpoint of family change, humanistic ideologies were followed by the economic developmental variables of urbanization and industrialization. It is reasonable to assume that some European

immigrants who came to America had already undergone preparation for family change, a change that would result in questioning patterns of authority, the basis of mate selection and motivations for marriage. Efforts to pursue such change continued to emerge in a context of support from America's own preoccupation with the questioning of traditional structures in religion, in government and in relations between people.

Nevertheless, it is doubtful that family change could have developed in America at the speed it did without significant economic development, including both industrialization and urbanization. While virtually every modern characteristic of the family and every contemporary pattern of the man-woman relationship can be found in a limited way in America's past, the data in these studies indicate that the speed at which America became urbanized and industrialized hastened the process of family change, although the process had begun much earlier. This fact should not diminish the role of other variables. For while the causal connection between the several variables influencing family change remains unclear, it is an oversimplification to attribute such change to the consequences of urbanization and industrialization without important qualifications. Such qualifications may involve a recognition of a wide range of social processes whose relationship to urbanization and industrialization is still ambiguous.

Limitations

There are several basic limitations to all of our studies. Although we have discussed the variables which bear a relationship to family change, we have been unable to explain satisfactorily how the family deals with the internal and external variables that tend to create such change. We have merely suggested a process that may be relevant. The problems of using literary sources, such as magazines, remain. There is the problem of accounting for editorial bias in the selection of material. We have tried to account for some of the potential biases, but we cannot be certain that this was accomplished satisfac-

torily. A crucial and unresolved problem with the use of the magazine in history is the problem of understanding the motivations of authors writing about the family. The fact that, in our research, we coded only explicit discussions in magazines, avoiding the implicit meaning of discussions, makes it difficult to deal with the motivations or intentions of authors.¹⁹ Nevertheless, even if one grants the possibility that the picture of female power, especially in the 1850-1865 period, was somewhat exaggerated by authors, it is important to ask why it was exaggerated. One reasonable conclusion is that magazine writers were concerned about female discontent, about power changes already taking place in man-woman relationships and about efforts to seek further changes.

There is always the general problem of ascertaining whether the concerns and contents of magazines corresponded to the real behavior of people. This problem is similar to any sociological effort that tries to ascertain the relationship between the concerns of people as they may be expressed in surveys and their actual behavior. We deal with probabilities. Magazines in each period have been described as increasingly more reliable as an index of public opinion (Tebbel, 1969:ch. 3). Thus, examining the magazine data from all of our studies, 1741-1865, and comparing these results with any other set of social indicators, it becomes apparent that magazines have reflected changes in the American family—changes which have emerged much more fully in the twentieth century.

This picture seemed to emerge initially in an essentially subtle form with a minimum of dramatic content. Thus, for many decades beginning in 1741, fiction and nonfiction magazine content reported preoccupations of people, which appeared to reflect general family change. As we moved into the 1850-1865 period, the issues of family change in magazine content were not only more pronounced

¹⁹ See Welter (1966) for a discussion which attempts to unravel the implicit meanings and hidden motivations of nineteenth-century writers with regard to the "cult of true womanhood."

but also were concerned with the broader issue of improving the position of all those who found themselves under unjust constraints—whether it was women in or out of the family, youths or blacks suffering under slavery. It is therefore appropriate to examine the possibility that literary sources, such as magazines, reflected the early signs of family change in America and may also have been instrumental in fostering that change. If this interpretation is correct, magazine content may be a neglected research tool for those involved in the socio-historic examination of institutions.

Finally, the subject of our investigation from 1741–1865, the reexamination of social institutions (i.e., the family) in historic perspective, is not simply an esthetic exercise that enhances the intellectual content and depth of sociology. It is, perhaps, the most appropriate way in which we can study the process of institutional change in order to increase our capacity to develop generalizations that will stand the test of time.

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THE AMERICAN PERCEPTION OF CLASS AND STATUS*

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Most American sociological research has conceptualized the occupational structure as a continuous prestige hierarchy. The research reported here questions the generality of that conceptualization by examining the relative importance of manual-nonmanual class differences and prestige differences for several attitudes and behaviors. First, the perception of "working class" and "middle class" more closely reflects a manual-nonmanual dichotomy than a continuous prestige scale. Voting behavior and party identification are also better predicted by the dichotomy. Second, the relevance of bounded class and continuous status models varies according to the issues involved. Thus, some interpersonal behaviors and individual satisfactions are patterned according to continuous prestige rankings while opinions on societal issues reflect dichotomous class differences. Third, individuals vary in their propensity to use class or status models according to social structural influences. For instance, a prestige orientation is fostered by small, traditional industries while the class dichotomy is more important in large, bureaucratic industries. The evidence also indicates that occupational prestige is more a middle-class concern, with little importance for manual workers.

Most recent quantitative research on stratification has been based on a status continuum model of the stratification system which assumes a graded hierarchy of occupational prestige (or secondarily, income and education). According to this view, American society is classless because no large cleavages can be found which separate the population into homogenous class groups. The 1947 North-Hatt prestige studies (Reiss et al., 1961) implicitly assume a continuous hierarchy¹ and the replication by Hodge et al. (1966) and Siegel (1971) makes this assumption more explicit:

The cleavage between white-collar, blue-collar, and farm occupations—if it exists at all—is based not so much upon matters of

societal evaluation as perhaps upon the character of dress and work in the three groups. (Hodge et al., 1966:327)

The widespread use of the Duncan SEI and the NORC occupational prestige scales has reinforced the implicit acceptance of the status continuum model.

A second type of model, less accepted by American sociologists, emphasizes distinct and bounded social classes. These classes may be defined by status groups (Warner, 1949), conflict groups with opposed interests (Marx, 1951) or a manual-nonmanual barrier to mobility (Blau and Duncan, 1967). Although status differences exist within or between these classes, the critical difference is the categorical nature of the class concept.² Class dichotomies are intended as more than crude approximations of the status hierarchy; they have an independent sub-

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¹ Hatt (1950) did emphasize discontinuous status as an important variable, but considered occupations to be ranked continuously within each status.

² For convenience, we will sometimes refer to bounded models as class models while presenting no evidence concerning market situations or relationships to the means of production. Similarly we will sometimes call continuous rankings "status models," despite legitimate noncontinuous status distinctions which are not continuous (e.g., ethnic groups, Warner's status groups). This should cause no confusion in this paper since we will be contrasting a status model which is continuous (occupational prestige) with a discontinuous class model (the manual-nonmanual gap).

stantive meaning not captured by the continuous scales.

The perception of the stratification system by the general population is often assumed to be based on a continuous model. This is implicit in Hodge and Treiman's (1968) use of the class-identification question and explicit in Lenski's (1952) study of status perceptions in a small community. This is in contrast to work by European sociologists (e.g., Goldthorpe et al., 1969; Popitz et al., 1969) who have sought to describe the class models held by their samples. We will begin with a reassessment of the importance of bounded classes in perceptions of the American stratification system and then extend this analysis to other types of attitudes and behaviors. We will show that, in appropriate circumstances, a wide variety of perceptions, attitudes and behaviors are organized around class divisions rather than along a continuous status hierarchy.

It is necessary to specify the different conditions under which class and continuous models arise. Situational determinants of class and status models have been ignored because of the incorrect assumption that these models are invariant properties of the individual. Moreover, cognitive models vary across individuals in different social positions as well as within a single individual across situational contexts. Certain types of behavior lend themselves more appropriately to one or the other model and certain situations encourage one or the other pattern. Research ought to be directed toward defining these influences rather than championing a given model.

CONDITIONS FOR CLASS PERCEPTIONS

First, position in the social structure influences class and status perceptions. Class and status models have ideological implications that make them attractive to different parts of society. Individual mobility within a social structure suggests a continuous ranking model of society; conflict between opposed groups depends on class perceptions. The continuous hierarchy is, therefore, a middle-class model, consistent with the belief that persons move along the continuum through

individual effort. This image justifies the position of the successful (Mannheim, 1936; Ossowski, 1963). However, the prestige differences that are important to the middle class may have little significance for the working class. Subordinant groups are more likely to hold a dichotomous class image in which society is structured to prevent the lower class from rising to the position of the dominant class. In essence, this is a "them against us" view. Huber and Form (1973) found income in a small American city related to these types of models, and several non-American studies also have found differences in class images based on position in the social structure (e.g., Hiller, 1975).

Second, class models should develop where bureaucratic definitions of formal roles make explicit the social structure of the organization. Less standardized organizational structures should promote more informal patterns of interaction that would blur sharp class divisions. For example, manual workers in process technology have more interaction with technicians and engineers and a more egalitarian and consultative style of interaction than the assembly line worker (Blauner, 1964; Woodward, 1965). The continuous model also may predominate in the service sector. The differences between tailors, policemen, hospital orderlies and library assistants are based more on status gradations than on sharp distinctions in class position. Service work may foster more heterogeneous contacts on the job, contacts which may reduce class consciousness (Hodge and Treiman, 1968; Jackman and Jackman, 1973). Thus, certain technologies (manufacturing rather than service, mass production rather than continuous process) tend to emphasize the distinctions between bounded classes as opposed to rankings along a status continuum. By looking at the relative incidence of discrete and continuous models in different industries, the role of technology in determining class and status perceptions can be evaluated.

Third, the two models are not mutually exclusive in any psychological sense. An individual is likely to have several models; class models may be relevant in some contexts and continuous models in others.

Following Weber, we suggest that responses to societal issues tend to be structured along class divisions but differences in individual life styles are more often distributed along a status continuum. Perceptions of class and society, voting, and political behavior are examples of issues that are societal in focus and are therefore organized more by class divisions. However, individual life styles are typically status concerns and thus more susceptible to continuous rankings. On these issues, class divisions should be less apparent.

STATUS CONTINUUM RESEARCH

The conditions for perception of society as a continuous hierarchy or as discrete classes can be studied empirically with existing data. Unfortunately, much past research has assumed one of the models *a priori*, and those few studies which did attempt to test the models are methodologically inadequate.

The occupational prestige studies (Reiss et al., 1961; Hodge et al., 1966) found a status overlap between lower-white-collar and skilled blue-collar occupations that is sometimes interpreted as evidence against a class model of society (Duncan, 1966; Glenn, 1975). However, the existence of an overlap on this particular task of a synthetic rating of "general standing" does not indicate that a manual-nonmanual distinction is unimportant in all contexts. Duncan (1961) has recommended a direct empirical comparison of the dichotomy with the more continuous scales as a test of their relative importance. Yet, in all the studies of the importance of occupational prestige, this simple statistical comparison has rarely been made. Duncan's own analysis (1966) as well as Glenn's (1975) more recent work did compare the manual-nonmanual dichotomy with income and education as determinants of occupational prestige and found the dichotomy to be unimportant. However, the prestige scale itself has not been compared with the dichotomy in predicting other behavior and attitudes.

Other research, from a different background, has claimed to support the status continuum model. For instance, Lenski (1952) evaluated the Warner scheme of

status groups by asking informants to divide a sample of 173 families into groups according to their "social standing." He found no consensus about the number or size of these status groups. The respondents also were willing to further subdivide the categories they had developed—a willingness Lenski interpreted as evidence of the arbitrary character of any categorical scheme.

Laumann (1966) tried to uncover "subjective class boundaries" through statistical analysis of survey results. Using measures of perceived social distance, Laumann tested for the statistical significance ($p < .05$) of the distance between any two occupations. He found at least three such points that might be interpreted as evidence of class boundaries. Overall, however, the final ranking of the occupations in the Laumann research fits a status continuum model quite closely.

These various empirical efforts raise a number of conceptual and methodological issues. None of the methods yet attempted have been equal to the task of deciding between a bounded class or status continuum model. The following issues must be addressed before any satisfactory resolution can be made.

1. The search for gaps in a status hierarchy is misdirected as a method for detecting class boundaries. First, it confuses two alternative approaches to stratification. Classes are not merely positions along a status hierarchy, but are an entirely distinct phenomenon that must be defined independently (Dahrendorf, 1959:76). Second, the attempt can always be foiled empirically by the inclusion of some interstitial occupation or by the division of an occupation into distinctive subfields. The most foolish of these attempts is a search for statistically significant gaps, since the significance level depends as much on the size of the sample as on the size of the gap. But the basic flaw in this research has been the attempt to develop categories from a unidimensional scale that is inherently continuous, such as income (Hamilton, 1966), social distance (Laumann, 1966), attitudes (Glenn and Alston, 1968) or status perceptions (Hamblin, 1974). Categorical concepts should be categorically defined.

2. Research supporting continuous or discrete models in one analysis cannot be generalized to all perceptions and behaviors. As we argued earlier, the models are not invariant characteristics of individuals. The context of the interview and the wording of the questions will influence the salience of the models. For instance, a request to distribute occupations into five ranked levels of "general standing" may elicit a status model while a request to label them as working or middle class may elicit a class model.

3. The existence of class models does not depend on respondents' ability to spontaneously describe the model as sociologists would (Gross, 1953; Gordon, 1958; Lopreato and Hazelrigg, 1972). It has long been accepted that rules which people cannot verbalize directly influence behavior. Speech, for instance, follows regular rules long before a person can verbalize the rules of grammar. The recent advances in cognitive psychology largely have been the result of use of techniques to uncover these implicit models and rules. While our research uses a more conventional approach, it follows the same strategy of abstracting general rules from respondents' judgments of concrete cases. Other empirical work which has required respondents to explain spontaneously their models of social structure (e.g., Goldthorpe et al., 1969; Popitz et al., 1969; Lopreato and Hazelrigg, 1972; Hiller, 1975) is less promising.

4. Lack of consensus within a community about the boundaries of classes does not imply that individuals hold continuous models. Each may have a somewhat different model which, if aggregated across individuals, may appear to the analyst as a continuous model. While such lack of consensus might imply a low potential for class conflict, it should not be interpreted as evidence for a continuous model as Lenski claims.

5. Finally, respondents' ability to subdivide categories does not necessarily imply a continuous model. The smaller subdivisions may be seen as distinct groups nested within the larger categories. Marx (1951:49) had just such an image of the social structure—nesting two groups, industrial owners and financiers, within

the capitalist class and both capitalists and the landed aristocracy within the bourgeoisie. The Indian caste system also exemplifies this capacity for dividing each group into smaller segments. Neither the divisibility of all categories nor the hierarchical ranking of the subcategories nullifies the fact that the cognitive model is clearly one of bounded groups. No one could claim that the traditional Indian system was continuous.

CLASS IDENTIFICATION RESEARCH

A detailed analysis of class identification offers several advantages for investigation of class and continuous models. First, the class identification question used by the Michigan Survey Research Center encourages a class model by offering two choices—middle and working class—rather than several levels of social standing. If a class model exists, it ought to be reflected in responses to this question. Second, the judgments are concrete—the individual's own position—and the rules by which people define themselves as working or middle class can be abstracted from the concrete judgments.³

Unfortunately, most recent class identification research has implicitly adopted a continuous status model. Multiple regression studies have confirmed what Centers (1949) first reported—occupation has the strongest effect, followed by income and education (Hodge and Treiman, 1968; D'Souza and Sethi, 1972; Jackman and Jackman, 1973). However, the use of least-squares statistics has encouraged the inclusion of continuous variables as predictors; occupation has been measured by prestige scales and education by years in school. Implicit in this approach is the view that responses to class identification questions are only crude rankings along a status hierarchy. This assumption has been reinforced by a recent revision in the dependent variable (e.g., Hodge and

³ Unfortunately, the process of abstraction requires aggregation across individuals; therefore, the lack of consensus on the class boundary will appear as a continuous model. Thus, the test for the existence of a class model will require consensus within the population.

Treiman, 1968) that divides the middle-class response category into middle and upper-middle ranks.

Another possibility exists. Respondents may react to the working and middle alternatives by using a bounded class model. "Working class" may mean more than low status in a continuous hierarchy; it may imply some categorical attributes that set it off from "middle class." Dalia and Guest (1975) have suggested that most of the independent effect of occupational prestige noted by Hodge and Treiman (1968) and by Jackman and Jackman (1973) is probably due to the white-collar/blue-collar subjective class schism. However, this conclusion was not based on any direct comparison of the occupational prestige scale with the dichotomy. Following Duncan's (1961) suggestion, we need only to include the prestige scale and the class dichotomy in the same regression equations to determine whether class identification reflects continuous or class distinctions. If "middle class" and "working class" signify something more than crude approximations to a status continuum, then a class dichotomy should predict those identifications, over and above what is predicted by the continuous status measures.

The research thus seeks to identify one type of stratification image by investigating why people label themselves as either working or middle class. Are persons middle class because they see themselves as positioned high along a continuous status hierarchy (as Hodge and Treiman, 1968, imply), or is it because they see themselves as belonging to a bounded social group (as Centers, 1949, originally intended)? We seek the causes of this self-identification behavior in the traditional way—by determining the strength of relationships to possible predictors. If respondents mean class and not continuous status by working and middle class, the distribution of those identifications should follow some class distinction. If the meanings of these labels reflect bounded classes, there is at least one class model recognized by this population.

Social structural influences on stratification models can be tested by including interaction terms in the analysis. Dalia

and Guest (1975) discovered a strong interaction between the manual-nonmanual dichotomy and the status measures of education and income. This interaction confirms the Ossowski model, that non-manuals are more status-oriented than manuals. The relative importance of occupational prestige to the two classes has not been tested, however.

METHODS

Research Design

To compare the dichotomous class effects and the continuous status effects, we have undertaken a version of analysis of covariance modified for a dummy dependent variable. Figures 1a – 1d represent possible outcomes of this analysis. All would be consistent with positive zero-order relationships between class identification and both the class and status predictors.

In Figure 1a, the class effect is subsumed by the continuous status measure. In such a case, the manual-nonmanual dichotomy can be interpreted only as a crude approximation to the status variable. In Figure 1b, the reverse is true. The continuous status measure explains nothing beyond the manual-nonmanual dichotomy. In this case, we would be justified in rejecting the interpretation of class identification or the manual-nonmanual dichotomy as merely a crude status ranking. In Figure 1c, both class and status effects can be observed. The slopes of the curves are equal but the manual curve is displaced to the right, indicating a substantially lower probability of middle-class identification at any given level of status. Finally, Figure 1d represents an interaction of the class and status variables in predicting class identification. Here status has an effect on class identification only among nonmanuals. Figure 1d conforms most closely to our predictions, based on Ossowski's observations, that continuous models are predominantly middle-class images of the stratification system.

Next, we hypothesize that the use of class and continuous models will vary according to industrial contexts. The effect of the class dichotomy on subjective iden-

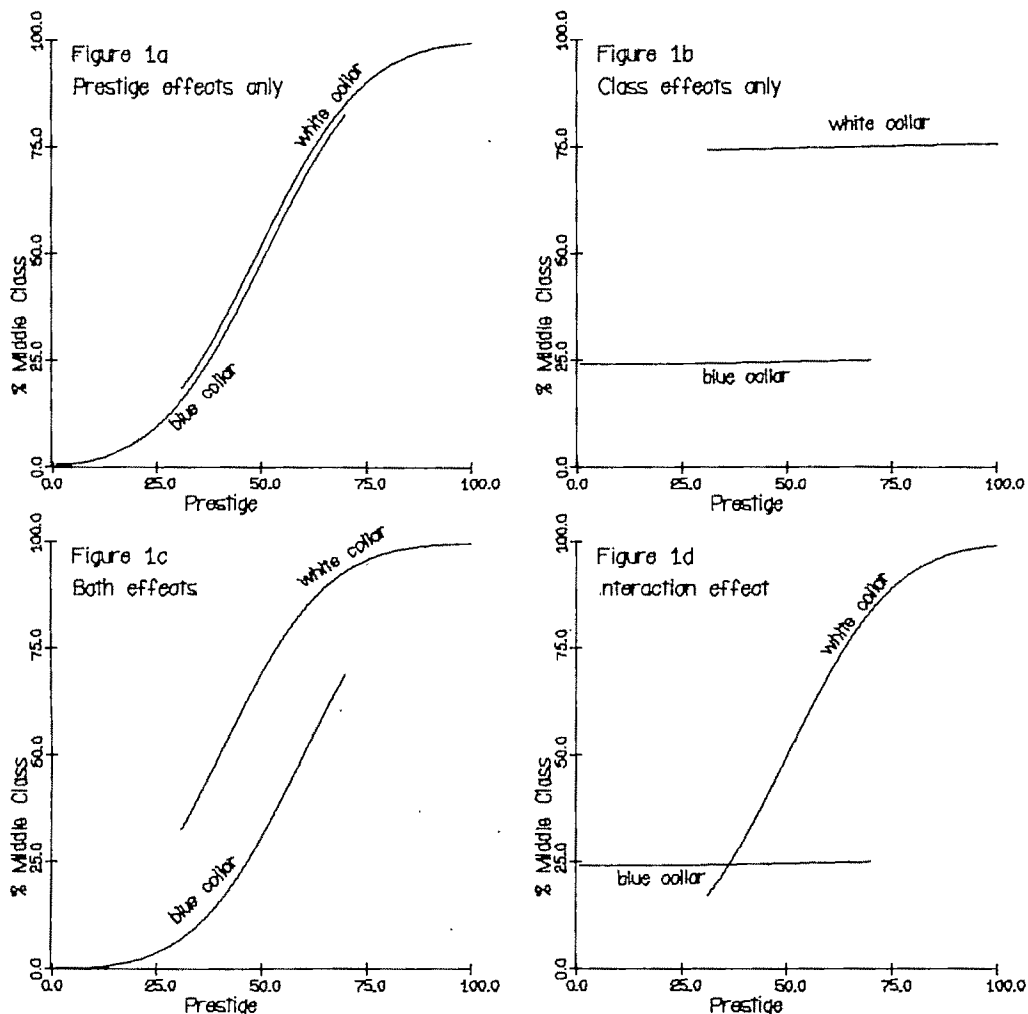


Figure 1. Effects of Class and Prestige on Subjective Identification

tification should be greater for workers in manufacturing than in service, but within manufacturing it should be relatively weak in continuous process industries. On the other hand, the status continua should have stronger effects in service than in manufacturing and be stronger for the process industries. These industrial effects should be explained by the association of class models with bureaucratic organizations.

Finally, we will investigate the scope of class effects by considering a wide range of attitudes and behavior. If subjective identification were the only observable consequence of class divisions in American society, it might be possible to dismiss class divisions as having little signifi-

cance. (Although, even then, we would argue that there was latent potential for class-oriented behavior.) One way to observe class divisions in American society is to examine voting patterns. It is important to note that the chances of observing class differences are not entirely dependent on the psychology of the voters themselves. The American political system is not known for offering clear class choices to the electorate. Thus, while the population may indeed be divided into distinct groups, if the political system does not directly address that division, we would expect little relationship between any class division and voting behavior.

Countless other behaviors and attitudes have been linked to a person's position in

the stratification system. However, no attention has been directed to whether these relationships are due to class distinctions or to variation along a continuous status hierarchy. In particular, we predict that, because of their societal focus, anomia (Srole, 1956) and confidence in national leaders (Roper, 1972) are determined by class differences; but organizational memberships (Goldthorpe et al., 1969; Verba and Nie, 1972), life satisfactions (Campbell and Converse, 1975) and sociability patterns (Goldthorpe et al., 1969) are largely matters of life style that reflect gradations along a continuous prestige scale.

Class and Status Measures

Throughout this paper we will utilize the manual-nonmanual dichotomy to operationalize the bounded class distinction. We recognize that this variable may be only a first approximation to the actual perceived division in American society. Indeed, there is substantial empirical evidence accumulating (Hamilton, 1966; Vanneman, 1977) that the line ought to be drawn to separate the managerial-professional occupations from all others. Further work (McNamee and Vanneman, 1976) explores the nature of the perceived division in greater detail and evaluates in particular the importance of authority (Dahrendorf, 1959) and autonomy (Blauner, 1964). However, we will limit ourselves here to the standard manual-nonmanual distinction because it has the longest research tradition (e.g., Lockwood, 1958) and still has substantial theoretical support (e.g., Giddens, 1973). To the extent we have misidentified the perceived gap, our results would be biased against uncovering class effects.

Occupational prestige was coded according to the most recent rankings reported by Siegel (1971) based on the 1963-1965 replication of the original NORC study. These scores are derived from a direct attempt to measure subjective prestige and, therefore, should represent the best contrast to an objectively defined class distinction.⁴ Education is

scored as years of school completed except that respondents with incomplete college were coded as 14 years, respondents with post high school non-college training as 13 years, and respondents with graduate degrees as 18 years. Income was recoded to the actual dollar value of the midpoint of the survey categories.

Samples

The principal data were collected by the Survey Research Center at the University of Michigan in four, biyearly pre-election interviews, from 1966 to 1972. Respondents were assigned weights so that the sample size for each year equalled the harmonic mean of the four sample sizes. Most of the analyses reported here focus on the white male non-farm labor force. Earlier research (Jackman and Jackman, 1973; Evers, 1975) has found the class identification process for blacks to be substantially different from the process for whites. Only labor force members were included because of the different interpretations given to occupation and even income among the non-labor force. Farmers were excluded because of their ambiguous position in a dichotomous class structure. The largest exclusion, women, was necessitated by the uncertain importance of husband's and own job in determining class identification (Ritter and Hargens, 1975) and evidence, to be presented, that the class identification process is different for women. For the class identification analyses, respondents with missing data (5.6% of the sample) had to be eliminated. This left a main sample of 1,865 respondents, which, after weighting, yielded an effective N of 1728.40.

Data from the 1972, 1973 and 1974 General Social Surveys also were utilized to supplement the main analyses with the SRC data.⁵ Again, the white male non-

using the Duncan SEI instead of the NORC prestige index. The results do not yield different interpretations although, in general, the Duncan SEI does have stronger associations. The results from the SEI are available from the authors.

⁵ These data utilize a four-category response: lower, working, middle and upper. The small lower and upper categories were recoded into the working

⁴ We also have computed most of the analyses

farm labor force was the main sample, in this case 1,000 respondents.

Probit Analysis

The objective of much of the statistical analysis is to determine the effects of various stratification measures on class identification. In this case, it would be incorrect to assume linearity between a continuous independent variable and the dichotomous dependent variable throughout the range of the continuous variable. For example, an increase from \$30,000 to \$40,000 in income would not cause the same increase in middle-class identification as would an increase from \$5,000 to \$15,000. More appropriate for the statistical analysis required here is a normal ogive model (Lord and Novick, 1968) in which an increase in status at either end of the hierarchy will have less effect on class identification than an increase in the middle of the hierarchy. The assumptions of the normal ogive are exactly the assumptions implicit in the status hierarchy interpretation of class identification. If class identification were only a dichotomization of self-placement along a continuous status hierarchy, the relationship between a continuous status variable and a dichotomous class identification would follow a normal ogive. But the assumptions of the linear model would "penalize" the continuous linear variables by requiring a uniform slope throughout the range of the status variables. Dichotomous independent variables would not be handicapped in this way and so might appear more powerful in the linear model. Thus, the use of the normal ogive provides a more rigorous test of the importance of class distinctions.

Because of the superiority of the normal ogive model, probit analysis (Goldfeld and Quandt, 1972) will be utilized rather than multiple linear regression. The two methods yield analogous results — the

only difference is that multiple regression tries to fit the data to a straight line while probit analysis seeks a fit to the normal ogive. A maximum likelihood solution is developed through an iterative technique.

CLASS IDENTIFICATION

Figure 2 graphs the observed relationship of occupation on class identification. The individual points were obtained from a sliding average at each prestige level based on all scores two points and less above and below the point being graphed. The smooth curves represent the best normal ogive approximations to the data.

These results demonstrate the importance of the manual-nonmanual distinction after controlling prestige level. Higher prestige blue-collar workers see themselves as substantially more working class than white-collar workers at the same prestige level. These results are confirmed after controls for education and income have been added (Table 1). In the SRC data, the standardized coefficient for the class dichotomy is significant and substantially higher than the prestige coefficient. The effect of the manual-nonmanual dichotomy is equivalent to about 47 points on the prestige scale, or about 3 1/3 standard deviations of prestige. In the GSS data, the prestige coefficient is not statistically significant while the manual-nonmanual gap is still large.

We should emphasize that this result is not an artifact of relating one dummy variable to another, since probit analysis seeks out the optimum point of inflexion in relating the continuous prestige index to the dichotomous dependent variable. This implies that no dichotomization of prestige would yield a better fit than the manual-nonmanual split. In fact, when the best dichotomization, at a score of 49, is substituted for the manual-nonmanual dichotomy, the fit (chi-square = 519.4) is substantially poorer and the coefficient of the dichotomy (.269) is smaller.⁸ Substan-

and middle alternatives in order to provide comparable results. The half-sample in 1973, which presented five possibilities including an upper-middle alternative, was eliminated from the class identification analysis.

⁸ The best prestige dichotomy was determined simply by trying every possible break point. The score of 49 was "best" in the sense that it gave the highest chi-square (and thus the maximum likelihood function). Only 30% of the respondents had prestige scores greater than 49.

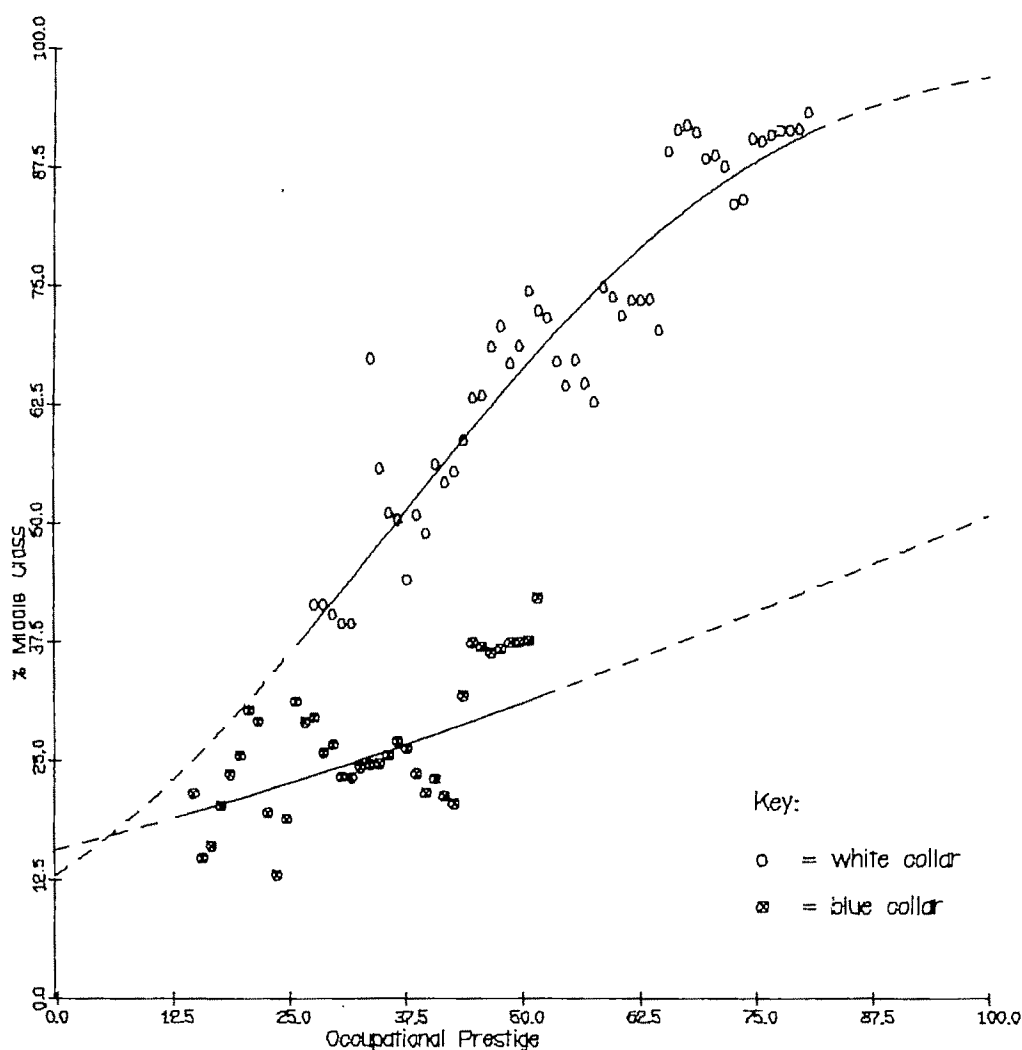


Figure 2. Middle-Class Identification by Occupational Prestige

tively, this means that the working-class and middle-class labels reflect the content of the class division, i.e., manual and nonmanual labor, and are not merely prestige judgments.

Table 1 also indicates the importance of education and income in determining class identification.⁷ While their importance would seem to support the continuous status-ranking interpretation, we should note that the education effect is itself close to being discontinuous. Much of the education effect is subsumed under the

⁷ Although in this analysis the occupational effect is divided between two variables, when either variable is dropped, the occupation effect still remains weaker than the education or income effects.

college versus noncollege distinction. Middle class in American society now implies having gone to college; it is not just the extra years of school, but the categorical distinction of being "college educated." The importance of this distinction is evident after the effects of occupation and income are controlled. When the some-college versus noncollege dichotomy is substituted for the continuous years of education, the standardized coefficient of the dichotomy (.329) is close to the coefficient for the continuous variable (.405) reported in Table 1. When a second dummy variable identifying college graduates is added to the equation, the fit is even better than for the equation

Table 1. Probit Analysis of Middle-Class Identification

	Probit Coefficient	Standardized Coefficient	T-test
<i>SRC data: (N=1,865, weighted N=1,728.4)</i>			
Manual-Nonmanual	.4700	.235	6.96
NORC Prestige	.0100	.138	3.07
Education—Years	.1232	.405	9.34
Family Income—\$1,000s	.0428	.295	7.13
Constant	−3.2118	−.086	
Chi-square ^b =544.65			
R square ^c = 0.2891			
<i>GSS data: (N=1,000, weighted N=892.3)</i>			
Manual-Nonmanual	.6062	.303	6.54
NORC Prestige	.0045	.063	1.02
Education—Years	.0766	.251	4.30
Family Income—\$1,000s	.0497	.393	6.99
Constant	−2.1072	−.044	
Chi-square ^b =250.50			
R square ^c = 0.2559			

* Standardized coefficients were obtained by multiplying the probit coefficients by their standard deviations, yielding effects estimates in terms of standard units of each of the predictor variables.

^b Calculated as -2 times the log of the likelihood ratio.

^c Although not as appropriate in this maximum likelihood model, the variance of class identification explained by the calculated probability is reported for those accustomed to this statistic.

with the continuous years of school (chi-square = 545.98).

Table 2 reports the equations fitted for working women and wives of working men. For women's jobs, the manual-nonmanual coefficient is less than twice its standard error and is, in fact, negative. If there is a perceived class division, it is not the manual-nonmanual distinction. Occupational prestige, moreover, is more important for women than for men ($b = .020$ versus $b = .010$). The difference in the class identification process for women is probably a result of the different jobs that women have, rather than evidence of

a different social psychology. Married women's interpretation of their husband's jobs is quite close to the men's results.

Interactions

Figure 2 also suggests that prestige is more important for white-collar workers than for blue-collar workers. This interaction is confirmed when separate analyses are made on manual and nonmanual workers. The occupational prestige coefficient for manual workers (-0.0005) is only a fraction of its standard error and thus may be considered to equal zero. For

Table 2. Middle-Class Identification of Women

	Working Full Time Own Job		Married Husband's Job	
	Probit Coefficient	Standardized	Probit Coefficient	Standardized
Manual-Nonmanual	−.0768	−.036	.4133	.207
NORC Prestige	.0198	.275	.0121	.164
Education—Years	.1482	.394	.1194	.300
Family Income—\$1,000s	.0315	.210	.0366	.249
Constant	−2.8250	−.019	−2.9279	−.102
Chi-square	153.71		262.50	
R square	.2084		.2097	
N=	811		1,365	
Weighted N=	682.9		1147.1	

blue-collar workers, therefore, the higher prestige of skilled work makes little difference for their subjective identification (although the extra income of this work is important). However, for nonmanuals the prestige coefficient (.0136) is over three times its standard error. Thus, the lower prestige of some of the clerical and sales work does imply a working-class identification — not merely because of the lower income and education associated with these jobs, but because of the lower prestige of the job itself. Similarly, education is almost twice as important for nonmanuals (.1547) as for manuals (.0861), although both coefficients are more than twice their standard errors. Only income has approximately equivalent effects for manuals (.0428) and for nonmanuals (.0440).

INDUSTRIAL CONTEXTS

A close comparison of manufacturing and service contexts fails to support the greater prestige orientation of service work. In the global comparison of workers in service and manufacturing, the prestige coefficient is indeed larger in service (.0136 versus .0012) and the class coefficient slightly smaller (.4051 versus .6389). However, the seemingly large difference in prestige effects can be explained by nonmanual predominance in service industries. We already have noted the greater importance of prestige among such nonmanuals. When the nonmanual-manual interaction is controlled, the difference between manufacturing and service disappears. Prestige has little effect among manuals in either sector (−.017 for manufacturing, .008 for service—both less than twice their standard errors), while among nonmanuals the effect of prestige is even greater for manufacturing (.019) than for service (.013)—exactly the opposite of the prediction.

Education also has roughly equal effects in manufacturing and service sectors (.084 and .090 for manuals, .147 and .147 for nonmanuals). There is thus no evidence to support the greater importance of education for subjective identification

in the “post-industrial” service sector (cf. Bell, 1973).

However, manufacturing and service are themselves heterogeneous categories. A more detailed investigation of industrial contexts was undertaken by dividing the sample into sixteen industry types.⁸ Separate normal ogive equations were fitted for each industry. Several of these coefficients fit our expectations. In particular, process industries have a larger prestige effect (.029) than any other manufacturing industry (e.g., machinery and automotive, .008) and smaller, indeed negative, class effects (−.123 versus .342 for machinery and automotive). Also, the small, less bureaucratized services tend to have larger prestige effects (e.g., personal services, .041) than the large service bureaucracies (e.g., public administration, .016) and smaller class effects (.263 versus .613).

In order to interpret the size of the prestige and class effects in all sixteen industries, the coefficients calculated from the sixteen probit analyses were correlated with other industrial characteristics. The sixteen industries used as units of analyses for these correlations were weighted by the number of respondents in the industry. The results of this analysis (Table 3) are suggestive although hardly conclusive because of the small base of sixteen industry categories as well as the small samples in each industry (average sample = 119) used to calculate the prestige and class coefficients.

The size of the class and prestige effects are moderately related to two indicators of bureaucratization (Stinchcombe, 1965). Class effects are larger and prestige effects are smaller in industries with a large percentage of clerks. Conversely, industries with a high percentage of self-employed workers have small class but large prestige effects. Both of these indicators suggest that in small, traditionally organized enterprises there is not a wide gap between manuals and nonmanuals, although status differences are important. However, in the larger rationalized bureaucracies, workers divide themselves

⁸ The list of industries and data are available from the authors.

Table 3. Bivariate Correlation Coefficients** between Industry Characteristics and Size of Probit Coefficients as Predictors of Class Identification

Industry Characteristics	Size of Probit Coefficients			
	Class	Prestige	Education	Income
% Clerical	.27	-.30	.33*	.70*
% Self-Employed	-.24	.39*	-.39*	-.33*
% Urban	.43*	-.36*	.21	.24
% White-Collar	.18	-.08	-.26	.37*

* $p < .10$ ($N=16$).

** Correlations weighted by the number of respondents in each of the sixteen industries.

more into two distinct groups, but status differences within these groups are not especially important. Also congruent with this interpretation is the association of urbanized industries with class effects and rural industries with prestige effects.

Education and income effects also vary largely according to the percent of high-status, white-collar workers. Both education and income effects are strongest in bureaucratic industries (i.e., high percent clerical, low percent self-employed). But income effects are positively related to percent white-collar while education effects are negatively related. In high-status industries, income best differentiates middle-class persons from working-class persons. In low-status industries, education best defines middle-class membership.

The evidence for these contextual effects is only indirect since there is no information for each respondent about the size or bureaucratization of his own organization. This type of information would clarify the picture substantially. Nevertheless, the percentages self-employed and clerical have been shown to be quite stable characteristics of individual industries that reflect the age of the industry and the extent of rationalization of the work process.

CLASS-ORIENTED BEHAVIOR AND ATTITUDES

Political Behavior

Data from three presidential elections are included in these surveys. The 1964 election was covered retrospectively in the 1966 and 1968 surveys; the 1968 elec-

tion was covered retrospectively in the 1970 and 1972 surveys in addition to post-election interviews in the 1968 survey. Discriminant function analysis was required since there are three distinct behaviors possible in each election—a Republican vote, a Democratic vote or no vote.

The 1964 and 1968 results are quite similar. In each, only one discriminant function is significant (Table 4). This function is a general dimension of status which ranks Republican voters highest, non-voters lowest and Democratic voters in between. What is important for our purposes is the greater weight of the simple manual-nonmanual dichotomy than the prestige scale. Again, most of the influence due to occupational position is not the result of an individual's location along the prestige hierarchy but his placement in either the manual or nonmanual groupings.

The 1972 election reveals a quite different pattern. Here two separate functions achieve significance. The first predicts non-voting and the second, a McGovern vote. In both functions, the role of the manual-nonmanual dichotomy is weak, which supports the impression that the 1972 election was not fought along these class lines.

Linear regressions on party identification confirm the importance of the class dichotomy for American political behavior. Table 5 reports the standardized coefficients for the four basic indicators utilized throughout this paper as well as for a more complete set utilized in other studies of party identification (Knoke, 1972). In both cases, the standardized coefficient for the occupational dichotomy is much larger than the occu-

Table 4. Discriminant Function Analyses on Voting Behavior

	1964	1968	1972	
	Function I	Function I	Function I	Function II
Canonical Correlation:	.238	.322	.265	.115
Chi-square:	43.0**	135.4**	51.1**	7.9*
Group Centroids:				
Democrat	-.017	-.046	+.088	.085
No Vote	-.183	-.199	-.339	-.003
Republican	+.149	+.124	+.107	-.030
Standardized Coefficients:				
Manual-Nonmanual	.166	.176	-.101	-.079
Occupational Prestige	.100	.017	.341	-.082
Education: Years	.254	.123	.296	.406
Income	.060	.216	.347	-.272

* $p < .05$.** $p < .001$.

pational prestige coefficient. The college versus noncollege dummy is used in these equations since it is a better predictor than the continuous scale ($\beta = .123$ versus $\beta = .086$ for the four-variable model). The results indicate that most political behavior studies have erred in using the more complete continuous scales rather than the simple dichotomies. More important, the theoretical implication is that, even in the United States, political behavior is organized more along class than status lines.

Other Behavior and Attitudes

Five domains of attitudes and behavior were selected from the 1974 General So-

cial Survey to explore further the scope of class and status effects in American society⁹. Each domain was measured by several items in the national survey; therefore, the relationship of each domain to the stratification measures was tested through canonical correlation. The first canonical variate was interpreted as the principal way in which these behaviors were patterned by the stratification system. Table 6 reports the standardized coefficients for the four stratification variables; they are equivalent to standardized beta weights in a regression on a variable defined by a linear combination of the dependent variables.

In comparing the relationships of the manual-nonmanual dichotomy with occupational prestige, the more clearly societal areas—confidence in national institutions and anomia show stronger class relationships and negligible prestige relationships. However, life satisfactions and organizational memberships, areas which reflect interpersonal life styles, show the opposite pattern—prestige relationships without any class relationships. Sociability patterns, contrary to the prediction, do not vary with occupational position.

None of these relationships are noticeably large. We should note, too, the general importance of education and, for life satisfactions, income. Nevertheless, the

Table 5. Regression on Party Identification

	Standardized Weights	
	4 Pre-dictors	8 Pre-dictors
Manual-Nonmanual ^a	.097*	.116**
Occupational Prestige	.012	-.003
College Education ^a	.123*	.119**
Income	.075*	.081*
Protestant ^a		.250**
Catholic ^a		.054
South ^a		-.126**
Age		-.033
Multiple Correlation	.235	.319

^a Dummy variables.* $p < .01$.** $p < .001$.

⁹ The exact items and their weights in the canonical factors are available from the authors.

Table 6. Class and Status Effects on Attitudes and Behaviors

	Canonical Correlation	Standardized Coefficients			
		Manual-Nonmanual	NORC Prestige	Education	Income
Confidence in Institutions	.475	-.269	-.024	-.228	-.058
Anomia	.500	-.130	-.058	-.331	-.107
Life Satisfaction	.354	-.040	.207	-.019	.268
Organizational Memberships	.662	.037	.279	.398	.086
Sociability Patterns	.210	.013	-.056	.236	-.091

important result is that the type of behavior being studied determines, to a large extent, the relative size of the class or prestige relationships. Those attitudes and behaviors that address societal issues are determined more by class than prestige. However, in the realm of personal life styles, prestige is more important than class differences. In this light, it is impossible to say that any given society is strictly class or status oriented since this orientation is a function of the type of behavior.

DISCUSSION

The results suggest that the division in American society between manual and nonmanual workers is more important than previously thought. The division is something more than a status distinction of occupational prestige, education and income. Although there are large status differences within manual and nonmanual groups, there is a perception of common categories—working-class or middle-class. Indeed, if we consider Glenn's (1975) results, we would have to conclude that nonmanual work carries no independent prestige value of its own.

This paper has sought to reorient the debate between continuous and discontinuous models of the stratification system in order to provoke a search for those conditions which encourage one or the other model. In this search, the models become variables themselves rather than invariant properties whose truth or falsity must be established. Some circumstances and issues favor the use of the class model while others favor a continuous model. For example, it appears that status differences are more important to nonmanual workers than to manual workers. In fact,

consistent with other theory (Ossowski, 1963) and research (Huber and Form, 1973), concern for status and the image of society as a continuous status hierarchy of minute gradations are largely middle-class phenomena. Small, rural industries also seem to encourage the prestige models. We suspect that the importance of class and status models also varies cross-nationally. Is American society less sharply divided than European societies? If so, would the more continuous gradation in the U.S. be explained by its higher percentage of nonmanual workers.

Finally, the relative importance of the two models varies not only across individuals but also within individuals across different domains of attitudes and behavior. Although the sample of such attitudes and behaviors included in this study is small, one criterion can be seen in the results. If the issue concerns the structure of the society, the manual-nonmanual division is most important. Politics is the best example of this type. However when the issue has a more individual focus, the continuous status variables best describe the observed differences.

If we admit that class and status models are not invariant properties of individuals, we should rethink the implications of cognitive models for mass social action. Class consciousness does not require the exclusive use of bounded models; rather, it is sufficient that people are able to respond meaningfully in terms of classes in some contexts. Given this, they can be mobilized for class conflict appealing to their class interests. Thus, the search for "spontaneous" images is misdirected. Mass social action does not depend on each person individually constructing an appropriate response. Protest behavior is typically a socially structured alternative

available to people. Their participation does require that the alternative meaningfully fits some image they have of society, but it is not necessary that they use only that image in all contexts. The results of this research demonstrate that bounded class models are meaningful to Americans, especially blue-collar Americans, and that important attitudes and behaviors are organized around class divisions.

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CLASS, STATUS, AND "REACTIVE ETHNIC CLEAVAGES": THE SOCIAL BASES OF POLITICAL REGIONALISM

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Two conflicting perspectives on the nature of political regionalism in Britain are examined. The first, based on a developmental perspective, argues that there is a high degree of regional uniformity in party support in Britain. The second, based on the theory of reactive ethnic cleavages, supports the view that Celtic political distinctiveness is a persistent feature of British politics. Both perspectives are found to be inadequate because of errors in conceptualization and design. One such error is the assumption that class affinity and status-group affinity as bases for political action are mutually exclusive orientations. Supporters of both perspectives argue that the predominance of class implies that status affinity is relatively unimportant and vice versa. A serious design defect concerns vote dichotomization; typically, researchers have examined the votes for only a single party. This paper demonstrates that the analysis of Conservative votes supports the theory of reactive ethnic cleavages, while the analysis of Labour votes supports the developmental perspective. Together, these findings support a third argument—that in economically peripheral areas, an appeal to regional or "ethnic" interests by the local dominant strata may be provoked by local class cleavages.

Many researchers have emphasized the uniformity of voting across regions in Britain, with class as the primary homogenizing force (see Alford, 1963:151; Blondel, 1963; Pulzer, 1967:98; Butler and Stokes, 1969:150). Blondel (1963:26), for example, states that class divisions are fundamental in Britain "simply because, elsewhere, local characteristics often unite people of all classes to an extent that is rarely attained in Britain." These authors usually employ a *developmental perspective*. In this perspective, functional, class-based cleavages are thought to supersede status-based cleavages as structural differentiation progresses over time. The clearest statement of this approach is in Lipset and Rokkan's (1967:1-64) introduction to *Party Systems and Voter Alignments*. They construct a complex, developmental typology based on an application of Parsons' A-G-I-L scheme to political cleavages in Western democracies.

The relevance of this perspective to the British case has been challenged recently by a number of researchers. For example, Rose (1974:20-3) argues that, sociologically and politically, Britain is a *multi-national* state. Noting that the Conservative Party is stronger than Labour in Eng-

land, while the reverse is true in Scotland and Wales, he argues that political parties in Britain have *national* as well as class images. Hechter (1975:208-35) cites the anti-Conservatism of Scotland and Wales as support for the *theory of reactive ethnic cleavages*. According to Hechter, the political distinctiveness of Celtic areas in Britain is a product of the internal colonization of these areas (the periphery) by England (the core).¹ One consequence of internal colonization is a "cultural division of labor" (Hechter, 1975:38). This results from the application of particularistic (e.g., cultural) criteria in the allocation of roles and resources. Following Barth (1969) and Gellner (1969), Hechter (1975:39-40) argues that *ethnicity* may arise from the salience of cultural distinctions in the system of economic stratification. This cultural division of labor may occur at any (societal) level of structural differentiation according to the theory of reactive ethnic cleavages (Hechter, 1975:223).

Although these two perspectives are clearly in conflict, both endorse an ana-

¹ For a succinct exposition of the concepts of core (or center) and periphery, see Galtung, 1971:81-117; Wallerstein, 1974:347-57.

lytical distinction between two bases of political action, *class* affinity and *status-group* affinity (see Weber, 1946:180-95). According to the developmental perspective, class cleavages in Britain have *superseded* status-based cleavages, while, according to the theory of reactive ethnic cleavages, the persistence of a status cleavage (Celtic sectionalism) represents the *failure* of class to become predominant. The present article shows that the contradiction between these two arguments is more apparent than real by demonstrating that Celtic sectionalism has roots in both class and status-group affinity.

REFORMULATION AND CRITIQUE

The Relationship between Class and Status-Group Affinity

The developmental perspective and the theory of reactive ethnic cleavages both adhere to Weber's distinction between class and status-group affinity. Both perspectives classify cleavages as class based *or* status based, and both support the notion that class affinity prevails at the expense of status-group affinity and vice versa.

Cohen (1976:1160), however, argues that Weber's discussion of class and status does not support the notion of *mutually exclusive* categories of group orientation. He points out that Weber's distinction between class and status-group affinity is an *ideal typical* contrast. Indeed, Weber (1946:187) argues that status honor "can be knit to a class situation: class distinctions are linked in the most varied ways with status distinctions." In this light, Weber's (1946:190) argument that the "style of life expected for members of a status group is usually conditioned economically" should not be interpreted to mean that economic conditions must eliminate status affinity. Rather, as Cohen (1976:1160) argues, "Weber . . . appears to indicate that the economic circumstances in which class affinity originates *circumscribe* or *condition* the social circumstances in which status affinity originates" (original emphasis).

Class, Status, and Political Regionalism

There are many world-wide examples of core-periphery conflicts occurring within the territorial boundaries of nation-states. In Western democracies, core-periphery conflicts usually gain expression via sectionalism. A pattern of political distinctiveness that is confined to a reasonably well-defined peripheral area is called peripheral sectionalism.²

The analysis of the social bases of peripheral sectionalism is greatly facilitated by a repudiation of the simplistic dichotomous distinction between class and status-group affinity. Since peripheral areas are usually culturally distinct from core areas (that is, these areas are "culturally peripheral"), a status interpretation of peripheral sectionalism is enhanced. However, peripheral areas usually experience specialized economic development (as a consequence of the geographic division of labor—see Wallerstein, 1974:347-57), and the class structures of peripheral areas reflect this specialization. Class factors, then, may condition or limit the growth of peripheral sectionalism.

Linz (1973:69-72), for example, contrasts two peripheral areas in Spain, Catalonia and the Basque Provinces. Historically, the Basques have been united in their opposition to Castilian Spain, while the Catalonians have been divided between two class-based peripheral opposition groups. Linz (1973:69-72) argues that in the Basque country the PNV (Basque separatist party) was able to take a populist stance "appealing to petty bourgeois anti-capitalist sentiments and supporting its own trade union movement" because in the Basque Provinces there are "many small entrepreneurs, highly skilled and paid workers, and well-distributed rural property." In

² Hechter uses the term "peripheral sectionalism" to refer to *culturally*-based sectionalism, but his usage is inappropriate because *periphery* refers to a particular type of area, one that has experienced specialized economic development by a core. Peripheral sectionalism is used in this paper to refer to sectionalism based in these areas of specialized development, without regard to its specific source (i.e., structural versus cultural).

Catalonia, however, a "three-cornered conflict emerged among employers, workers, and [Castilian] national authorities." According to Linz, this class conflict in Catalonia limited the development of peripheral sectionalism.

Regional political tendencies in Norway also reflect differences in economic specialization and in class structure. Rokkan (1967:415) contrasts the different regions of Norway along these two dimensions. His fourfold typology is based on two dichotomies: hierarchical communities versus equalitarian communities and inland economy (agriculture and forestry) versus coastal economy (fishing). There are four possible combinations. Rokkan specifies the political tendencies associated with each type. In some regions, the pattern of political distinctiveness is the product of the highly uniform support of one class for a particular political party; in others, it is more the product of general cross-class support for a particular party.

These examples are suggestive of the great variety of peripheral political responses. They also suggest that class factors are frequently important in the origin and evolution of these responses. This emphasis is very different from the emphases of existing treatments of regional factors in British politics. These arguments are extreme in their emphases, one supporting the idea of regional homogeneity based on class, the other maintaining the failure of class politics in peripheral areas and the consequent persistence of a status-based cleavage.

The Social Bases of Celtic Sectionalism

The specialized economic development of Scotland and Wales has been primarily industrial. This industrialization has led to the creation of a large, industrial working class in the Celtic periphery. Thus, industrial conflict has been more intense in the periphery than elsewhere in Britain (see, e.g., Pelling, 1958; 1968). Wallerstein (1974:353) argues that it is class conflict in peripheral areas that encourages the formation of culturally based, peripheral opposition to core areas:

Geographic concentration of particular economic activities serves as a constant pressure to status-group formation. When the local dominant strata are threatened by the incipient class consciousness of lower strata, emphasis on local culture serves well to deflect local internal conflict, creating instead local solidarity against the outside. If, in addition, these local dominant strata feel themselves oppressed by higher strata of the world system, they are doubly motivated to pursue the creation of local identity.

Obviously, one does not construct an identity out of thin air. One builds on what one finds—in terms of language, religion, and distinctive life-styles. Nonetheless, it is quite clear that both linguistic and religious homogeneity and passion (a fortiori devotion to separate life-styles) are social creations which cannot be accounted for as simple continuities of tradition eternal. They are social creations molded with difficulty in times of travail.

As the contrast between Catalonia and the Basque Provinces clearly illustrates, however, the local dominant strata of a peripheral area may be unsuccessful in creating cross-class solidarity.

Following Wallerstein, one would expect sectionalism in the Celtic periphery to *originate* in the local dominant strata. Following Linz, one would want to ascertain the degree to which this process of status-group formation is limited, if at all, by class cleavages. The fact that the Labour Party is strong in Scotland and Wales suggests that the local dominant strata have not generated local solidarity. Hechter (1975:292), however, argues that the Labour Party has served as one of several *outlets* for Celtic sectionalism. Indeed, he argues that the Labour Party is strong in Scotland and Wales *because* of Celtic sectionalism (1975:264–310). The basic argument of this paper is that (1) the Labour Party has not served as an outlet for Celtic sectionalism and, by implication, (2) the process of status-group formation in Celtic areas has been severely limited by class cleavages.

Review of Existing Analyses of Political Regionalism in Britain

Two contemporary studies of British voters, Butler and Stokes' (1969) *Political*

Change in Britain and Alford's (1963) *Party and Society*, have examined regional variations in voting behavior. Both support the notion that there is a high degree of regional homogeneity in British politics.

Butler and Stokes present data on Britain showing the cross-tabulation of class and political choice for each region. They note a strong, consistent relationship between class and party support in all areas of Britain. This is qualified, however, by a distinction between two areas within Britain, the "depressed north" and the "expanding south" (Butler and Stokes, 1969:142). They find that in the north the working class is much more unanimous in its support of the Labour Party. Noting that the relative size of the working class is also much greater in the north, they hypothesize that class has a contextual effect. Thus, the larger the working class is in an area, the greater the probability that working-class individuals will support the Labour Party. Conversely, the greater the size of the middle class, the greater the probability that middle-class individuals will support the Conservative Party. They demonstrate these arguments with scattergram analyses of the proportion of the working class supporting Labour and the proportion of the constituency which is working-class (Butler and Stokes, 1969:149). They conclude that regional variations are unimportant in Britain once the contextual effect of class is taken into account (Butler and Stokes, 1969:150).

Alford's (1963) analysis of region and class is somewhat different from that of Butler and Stokes, though the conclusions are very similar. Alford concentrates on regions which might have the greatest possibility for a sectional response, Wales and Scotland. (This is very much in contrast with the emphasis of Butler and Stokes [1969:149]; in one scattergram of regions, they exclude Scotland and Wales altogether.) Alford considers the possibility that a reaction in the Celtic periphery to the colonial-like exploitation of the area by the English might pull both classes over to one party. Thus, a united opposition would confront the exploiters. He fails to find such opposition. Alford (1963:144) does find greater support for

the Labour Party in Wales and Scotland, but he also finds higher class voting in these areas. Thus, even though there is strong support for the Labour Party, the *difference* between the percentage of the working class voting Labour and the percentage of the middle class voting Labour is greater in Wales and Scotland than elsewhere according to Alford's analysis. He concludes that there is no evidence of an anti-colonial reaction in Scotland and Wales.

In support of the theory of reactive ethnic cleavages, Hechter examines historical, aggregate data on British counties. He analyzes the determinates of Conservative voting at eight points in time in British political history. The theory of reactive ethnic cleavages argues that there is no necessary relationship between the existence of ethnic boundaries and the degree of complexity of social organization. Thus, it is expected that Celtic differences should persist into the modern period, given the persistence of a cultural division of labour.

Hechter (1975:208-33) regresses county levels of Conservative support against a set of interrelated indicators of industrialization. These indicators include the occupational breakdown of the working population into primary, secondary and tertiary sectors, population indicators such as percentage sixty-five years of age and older and population growth rate, and an indicator of urbanization. He notes that the level of explained variance in Conservative vote by these variables does not increase over time (Hechter, 1975:320). After analyzing total explained variance by these variables at each point in time, Hechter examines the effects of cultural variables associated with Celtic identity, such as level of religious Nonconformity and percentage Welsh- or Gaelic-speaking, on county levels of Conservative support, in an attempt to demonstrate the persistence of cultural factors. Before regressing cultural variables on the Conservative vote, however, Hechter removes the effects of the structural variables from each cultural variable. Thus, Hechter analyzes the importance of "cultural residuals." Regression analysis shows that the cultural residuals consistently explain

as much, if not more, of the variance in Conservative support than the various indicators of industrialization (Hechter, 1975:319). Hechter concludes from his analysis that Celtic peripheral sectionalism has persisted in British politics despite structural differentiation.

Critique

Given that Alford, and Butler and Stokes (developmental perspective) and Hechter (theory of reactive ethnic cleavages) all employ a dichotomous conception of the relationship between status and class, it may seem surprising that their empirical findings are fundamentally different. The contrary findings these researchers report are explained by the fact that each restricted the scope of his analysis. Since World War I, political competition in Britain has been multiparty. Prior to World War I, there were four parties; but the Liberal Unionists were allied with the Conservatives while Labour was allied with the Liberals, thus narrowing competition to two groups. Analysis of political data is relatively uncomplicated if there are only two competitors, since support for one party is perfectly, negatively correlated with support for the other. This is not the case, however, if there are three or more separate parties.

Since there are only two *major* parties in post-World War I Britain, researchers have tended to analyze support as if there were only two parties. Alford, for example, analyzes only Labour versus non-Labour support; that is, he lumps Conservative, Liberal, and Celtic nationalist support into one category. Hechter, on the other hand, analyzes only Conservative support. He combines Labour, Liberal, and Celtic nationalist support together, arguing that each of these parties has served as an outlet for Celtic anti-Conservatism. Butler and Stokes systematically exclude "third parties" by analyzing only proportions of Labour plus Conservative support, thus denying the reality of Liberal Party and Celtic nationalist party supporters in their empirical analysis of regional variations. Hechter (1975:218n) argues that the analysis of Conservative Party support is most suited for the examination of regional variations

since Celtic political responses have been primarily anti-Conservative. Alford (1963:79-86), on the other hand, argues that the Labour-non-Labour split is most appropriate for the examination of class voting.

To gain a complete understanding of political regionalism in Britain, it is necessary to examine separately the social bases of Conservative Party and Labour Party support. The analysis of Labour voting appears to support the developmental perspective, while the analysis of Conservative voting appears to support the theory of reactive ethnic cleavages. Clearly, the only way to adjudicate between these two perspectives, or at least explicate the contradiction between them, is to analyze independently the support for each party.

Hypotheses

If the theory of reactive ethnic cleavages is correct, then cultural factors associated with Celtic identity should have a negative effect on Conservative Party support and a positive effect on Labour Party support. This is consistent with this theory's argument that Celtic sectionalism has been expressed via an anti-Conservative posture, with the Labour Party serving as one of several outlets. If the developmental perspective is correct, cultural factors should have little effect on either party, especially after the rise of Labour as a major national party. This prediction is based on the notion that as the societal level of structural differentiation progresses over time, functional, class-based interests should prevail. If, however, status-group formation in a peripheral area originates among the local dominant strata and is arrested by class cleavages, as the arguments of Wallerstein and Linz suggest, cultural factors associated with Celtic identity should have a negative effect on Conservative support and *no* effect on Labour support.

DATA AND METHOD

Data

The data used in this study are in most respects very similar to those used by

Hechter: county-level, historical census and election data.³ For the sake of comparability, I analyze data on the same elections that Hechter analyzed, the elections of 1885, 1892, 1900, 1910, 1924, 1931, 1951 and 1966. The census data are from the *County Reports* of the *Census of England and Wales* and the *Census of Scotland* for the years 1881, 1891, 1901, 1911, 1921, 1931, 1951 and 1961. The *County Reports* form a special volume in each of the British censuses.

There are 86 counties in Britain; 40 English counties, 13 Welsh counties and 33 Scottish counties. While Celtic areas constitute only about one-seventh of the population of Britain, over one-half of all British counties are Celtic. Regression analysis of the county data is comparable in many respects, therefore, to stratifying a sample of individuals by Celtic/non-Celtic. These data thus magnify differences between people who live in Wales and Scotland and those in England.

The political data employed here are based on the aggregation of parliamentary election results into county data. Within counties, the number of constituencies varies with population.

Measurement

Political disposition. The developmental perspective emphasizes that economic development increases the importance of class-based political cleavages in the polity. Thus, in a fully developed political system, regional variations in the sizes of different classes should explain most, if not all, variations in party support. A primary concern, then, is the nature of regional, political variations which *cannot* be explained by *class* factors.⁴ If they ap-

pear to be random, or at least unsystematic, then the argument that class factors alone are important is supported. If a pattern of excessive support for, or opposition to, a particular political party (that is, *excessive* relative to what one would expect given knowledge of an area's class composition) is detected, then this is evidence of sectionalism. Thus, the central concern is for level of party support *standardized* on class variables. I call this measure of political support *standardized* on class variables "political disposition."

To measure political disposition at the county level, it is necessary to regress county levels of party support on the sizes of different classes and then to examine the residuals from this regression. If an area has more Conservative support than expected according to prediction by class variables, for example, then the political disposition of this area can be described as pro-Conservative. The general form of this estimation technique is:

$$Y = A + B_1x_1 + e,$$

where Y = observed level of party support, x_1 = class variables and e = measure of political disposition. Thus, if an area is mostly working-class but is also a Conservative stronghold, then it probably will have a very large positive residual.

Since the objective is to measure degree of conformity to national patterns of party support, the appropriate dependent variables are the levels of support for the major national parties. In Britain, these are the party in power and the party considered by the electorate to be the loyal opposition. Before World War I, these were the Conservative Party and the Liberal Party. Since the Labour Party, during this period, was an adjunct of the Liberal Party and the Liberal Unionists were allied with the Conservatives, the analysis of the vote for the Liberals and their allies is simply the inverse of the analysis of the vote for the Conservatives and their allies. Thus, only one dependent variable is necessary for the first period.

³ Aggregate ecological analysis is the only mode available for doing quantitative, historical research. Dogan and Rokkan (1969) discuss some of the problems of aggregate analysis.

⁴ Hechter regresses the Conservative vote against a set of interrelated indicators of industrialization, rather than indicators of class composition. However, the developmental perspective emphasizes that with economic development, *class* factors should become more important. Thus, measures of the importance of class in politics should be related to structural differentiation. To regress the vote on indicators of structural differentiation is a totally inap-

propriate test of the developmental perspective. For a review of other analytical and theoretical errors in *Internal Colonialism*, see Ragin (1976).

Since World War I, however, there have been *three* distinct national parties, even though only two have had any real chance of gaining office. Thus, the continued success of the Liberal Party as a third national party and the more recent success of Celtic nationalist parties dictate the use of two dependent variables, Conservative support and Labour support. Furthermore, the Labour and the Conservative Parties are both class-oriented during this period. Labour's primary constituency has been the working class, while the Conservative Party's primary constituency has been the middle classes. Since (1) the vote for one party is *not* the inverse of the vote for the other and (2) both are oriented toward class support, it is necessary to analyze the support of each party separately.

The independent variables of interest here should indicate the nature of the class composition of different areas. The developmental perspective emphasizes that, with economic development, class should dominate politics. Thus, levels of party support should be regressed against measures of the size of different classes in different areas. The goal here is to obtain the best possible predictive model, since the residuals are treated as measures of political disposition. Thus, multiple indicators of class composition are used despite the likelihood of multicollinearity. The following variables are used as measures of class composition:

1. Percentage of the adult male work force employed as *manual workers* in general manufacturing occupations;
2. Percentage of the adult male work force employed as *manual workers* in mines and quarries;
3. Percentage of the adult male work force employed as *manual workers* in heavy industries;
4. Percentage of the adult male work force employed in commercial and professional occupations (nonclerical);
5. Percentage of the adult female work force employed as household domestics.

Note that the first three variables do *not* include *all* adult males employed in those sectors—only those employed in manual

occupations.⁵ Note also that the fourth variable specifically excludes lower white-collar workers. The fifth variable measures class differentials. This measure is particularly suitable because (1) it includes only female *household* domestics and (2) the measure simultaneously indicates the propensity of the middle and upper classes to consume and the need for lower-class women to sell their labor.

Measurement of cultural peripherality. To ascertain the overall importance of cultural (ethnic) factors, Hechter used a set of interrelated indicators of cultural distinctiveness: percentage Nonconformist, percentage Established Church, percentage who speak a Celtic language and a measure of religiosity.⁶ He used multiple indicators in order to exhaust all of the variance in Conservative support that could be explained by cultural factors. However, when he analyzed changes over time in the parameters of a particular model, he used only percentage Nonconformists. Inclusion of all indicators would have introduced severe multicollinearity and rendered the coefficients uninterpretable at each point in time, thus making longitudinal comparisons impossible. I also use a single measure of cultural peripherality, percentage Nonconformists, since I examine a particular model at several points in time.

This measure, percentage Nonconformists, will allow the examination of the extent to which a status factor which indicates a cultural component of ethnicity has influenced Conservative and Labour disposition in British counties over time. The stronger the effect of percentage Nonconformists on the measures of disposition, the stronger the evidence that status-based peripheral sectionalism is a factor in British politics.

Controls for national-regional effects. A pattern of political distinctiveness may be uniform within a peripheral area. This might obtain for historical, particularistic

⁵ The crudeness of census occupational categories, of course, limits precise measurement of numbers of manual and nonmanual workers.

⁶ The measures of religiosity and church affiliation are based on marriage data (see Hechter, 1975:164–206).

reasons not directly addressed by the theories discussed here. For example, an area might consistently and uniformly support a particular party because of the superior organization of the party in that area. When political subdivisions are historically defined, as in the case of Britain, this pattern is particularly favored.

To examine national-regional effects, it is necessary to include in the analysis an indicator for the counties of Scotland and Wales. This can be accomplished by computing two dummy variables, one coded "1" for Scottish counties and "0" for all others, and the other coded "1" for Welsh counties and "0" for all others. The inclusion of these dummy variables allows a more rigorous test of the reactive ethnicity argument.

Cultural peripherality (measured here as percentage Nonconformists) varies within and between national regions. Thus, Wales and Scotland experience relatively higher levels of Nonconformity than England (Hechter, 1975:321-2), but Nonconformity also varies *within* Scotland and Wales. It is necessary, therefore, to control for national-regional effects since Nonconformity is more concentrated in Scotland and Wales. The omission of these controls could permit the effects of uniform national-regional differences to be attributed to Nonconformity.

ANALYSIS

Political Disposition

Table 1 shows the results from regressing Conservative and Labour voting on the five indicators of class composition. The unstandardized regression coefficients reported in this table are used to compute Conservative disposition at eight points in time and Labour disposition at four points in time. These derived, residual measures are used in the analysis below. Note that the causal efficacy of class variables is much greater in the case of Labour support.⁷ This pattern is consistent with the

argument that Conservative and Labour support are fundamentally different and, to gain a complete understanding of political regionalism in Britain, it is necessary to analyze support for both parties separately.

Degree of Sectional Response

The skew of the residuals from the regression of party support on class variables (see Table 2) shows the *degree* of sectional response. A strong skew indicates the possibility of sectionalism because (1) the largest residuals should obtain for *sectional* counties since these counties should conform least to national patterns of party support and (2) the largest residuals should be all positive or all negative since a single pattern of political distinctiveness is expected.⁸ Thus, for example, if Hechter's (1975) argument is correct, Conservative political disposition should manifest a strong negative skew; Labour political disposition, a strong positive skew. This would obtain because Hechter argues that Celtic sectionalism has found expression in an anti-Conservative, pro-Labour posture.

The findings for Conservative disposition are much more consistent than those for Labour disposition. For all but two of the eight elections analyzed, Conservative disposition manifests a strong negative skew. The skew is much stronger after World War I because the Liberal Party remained strong in isolated pockets (mostly at the expense of the Conservative Party). The data for the elections of 1892 and 1900, however, show only very slight skew. These anomalies are probably due to the split between the Liberals and the Liberal Unionists during this period.⁹

⁸ Of course, these measures of skew would be inappropriate if there were two distinct pockets of peripheral sectionalism, with one expressing its sectionalism via strong support for a national party and the other via strong opposition to the same party.

⁹ Votes for the Liberal Unionists have been included as Conservative votes since the Liberal Unionists were allied with the Conservatives. In some areas of the Celtic periphery there was little or no traditional Liberal opposition to the Liberal Unionists. Consequently, these areas appear to be pro-Conservative when they are, in fact, anti-Irish nationalist.

⁷ Since there is a great deal of multicollinearity among these measures of class composition, it is hazardous to interpret the regression coefficients.

Table 1. Regression of Conservative and Labour Votes on Class Composition

Year of Election	% Miners	% Heavy Industries Workers	% Professional and Commercial	% Female Domestics	% Other Industrial Workers	R ²
Conservative Vote						
1885	-.221* (-.340)	-.128 (-.378)	-.113 (-.232)	.495* (1.510)	.276* (.283)	.344
1892	-.216 (-.300)	-.076 (-.237)	.187 (.318)	.069 (.225)	.214 (.191)	.156
1900	-.312* (-.531)	.034 (.216)	.346* (1.107)	.022 (.154)	.200* (.209)	.353
1910	-.233* (-.341)	.363* (.993)	.209* (.479)	.491* (3.555)	.071 (.096)	.466
1924	-.361* (-.474)	.041 (.082)	.018 (.030)	.220 (1.063)	-.136 (-.177)	.269
1931	-.275* (-.463)	.137 (.399)	.164 (.289)	.222 (1.477)	-.040 (-.047)	.239
1951	-.368* (-.720)	.068 (.137)	-.152 (-.153)	.162 (1.124)	-.138 (-.114)	.120
1966	-.332* (-.738)	-.016 (.282)	.020 (.019)	.181 (1.175)	.035 (.028)	.166
Labour Vote						
1924	.471* (.707)	.212* (.494)	.101 (.191)	-.075 (-.416)	.176 (.262)	.386
1931	.587* (.898)	.282* (.748)	.234 (.372)	-.080 (-.485)	.172* (.185)	.558
1951	.469* (.914)	.288* (.582)	.238* (.238)	-.193 (-1.337)	.109 (.089)	.540
1966	.398* (1.049)	.450* (.934)	.167 (.187)	-.121 (-.937)	-.103 (-.096)	.482

Notes: Data are on countries. Unstandardized coefficients in parentheses.

* Denotes significance at .05.

Overall, these data give moderate support to the argument that some sort of sectionalism (anti-Conservatism) has persisted in British politics.

Table 2. Skew of Residuals from the Regression of Conservative and Labour Support on Class Variables

Year of Election	Conservative Residual	Labour Residual
1885	-.858	
1892	-.083	
1900	-.096	
1910	-.516	
1924	-1.017	.387
1931	-.896	.104
1951	-1.365	-.993
1966	-.916	-.039

For Labour disposition, the greatest skew is negative, thus contradicting the reactive ethnicity argument that the Labour Party has served as an outlet for sectionalism. Little, if any, skew is associated with the residuals from the 1931 and 1966 regressions. The one clearly positive skew is for 1924, but this skew is very small in contrast to the large negative Conservative skew for that same election. Overall, there appears to be little support for the argument that the Labour Party has served as a vehicle for sectionalism.

The Social Bases of Sectionalism

The specific nature of sectionalism can be ascertained by regressing Conservative

Table 3. Regression of Conservative and Labour Political Disposition on Nonconformity and Regional Dummy Variables

Year of Election	Nonconformity	Scottish Dummy	Welsh Dummy	R ²
Conservative Political Disposition				
1885	-.609* (-.367)	.045 (.009)	.125 (.033)	.285
1892	-.351* (-.223)	.002 (.005)	-.339* (-.107)	.306
1900	-.446* (-.445)	-.198 (-.069)	-.281* (-.141)	.483
1910	-.628* (-.570)	.199 (.059)	-.176 (-.069)	.476
1924	-.441* (-.396)	.059 (.015)	-.214 (-.087)	.306
1931	-.548* (-.695)	-.188 (-.061)	-.072 (-.033)	.340
1951	-.485* (-.422)	.165* (.039)	-.229 (-.075)	.563
1966	-.782* (-.692)	-.363* (-.078)	.052 (.015)	.487
Labour Political Disposition				
1924	.049 (.045)	.218 (.058)	.135 (.056)	.072
1931	-.087 (-.073)	.066 (.014)	.082 (.025)	.007
1951	-.321 (-.205)	-.375* (-.064)	.354* (.086)	.207
1966	-.328 (-.264)	-.181 (-.036)	.376* (.100)	.081

Note: Unstandardized regression coefficients in parentheses.

* Denotes significance at .05.

and Labour disposition on cultural peripherality (percentage Nonconformists) and the two regional dummy variables (see Table 3). Conservative disposition is analyzed at eight points in time; Labour disposition, at four points.

The findings here support those in Table 2. Analysis of Conservative disposition from 1885 to 1910 shows the persistent association of Nonconformity with anti-Conservatism. The effects of the national regional dummy variables are slight or nonsignificant or both.¹⁰ From 1924 to

1966, percentage Nonconformists again explains much of the variance in Conservative disposition. The unstandardized coefficients show that after World War I the effect of Nonconformity is at least as strong as before the War. Also, the effects of the national-regional dummy variables are again weak or nonsignificant.¹¹

Analysis of Labour disposition produces an entirely different picture. For none of the elections analyzed does percentage Nonconformists have a statistically significant effect. The dummy variables show occasional effects, but these

¹⁰ Wales experienced uniform anti-Conservatism in two of the four elections during this period. This pattern is probably due to the salience of issues associated with the Welsh educational system and the disestablishment of the Church of Wales.

¹¹ The effects of the dummy variable for Scottish counties for 1951 and 1966 reflect the Conservative revival that affected Scotland in the 1950s and its subsequent demise.

effects are neither strong nor consistent. In 1951, for example, Scotland appears to be relatively anti-Labour, while Wales is relatively pro-Labour.

Overall, these data show that while there is evidence of anti-Conservatism in culturally peripheral areas, the Labour Party has not served as an outlet for this sectional sentiment. Indeed, working-class support for the Labour Party appears to be very much the same in all areas of Britain.

DISCUSSION

These findings do not support the theory of reactive ethnic cleavages. This theory is dependent on the argument that an ethnic cleavage will persist if there is a cultural division of labor, and this cleavage will inhibit the development of class-based cleavages. My analysis demonstrates that there are no cultural peripheral effects in Labour support; this suggests that working-class politics are very much the same throughout Britain. To the degree that there is an "ethnic" response, it must occur independently of working-class politics and Labour Party support.¹² A political phenomenon that affects only one of two class-based parties can hardly be described as ethnic in the usual class-denying sense of the term. If peripheral sectionalism in Britain were an ethnic response, then cultural peripheralism would have had a positive effect on Labour disposition. Hechter (1975:292) treats Labour voting as ethnic; the analysis here demonstrates that, in fact, it is not.

With respect to the developmental perspective, the findings here show that the rise of class-based political action does not imply a concomitant decline in political regionalism. Lipset and Rokkan (1967:51) argue that working-class political mobilization in Western democracies during the late nineteenth and early twen-

tieth centuries led to concerted middle-class countermobilization through nationwide mass organizations. This countermobilization has been less than nationwide in Britain, as the Conservative Party has failed to attract its potential supports in the Celtic periphery. This is true despite the relatively uniform class bases of Labour support across regions. Apparently, peripheral sectionalism continues to be a possibility after the development of class politics, but the strength of the peripheral response is limited by class factors.

The analysis of Conservative disposition consistently shows the effects of cultural peripherality, while the analysis of Labour disposition consistently shows the absence of such effects. The non-Conservative proportion of the total vote also must show the effects of status factors since the non-Conservative proportion of the vote is perfectly, negatively correlated with the Conservative proportion. The Labour proportion of the total vote, however, shows no such effects. Therefore, peripheral sectionalism is associated primarily with support for the Liberal Party and the Celtic nationalist parties (i.e., the non-Labour, non-Conservative proportion of the vote).

Alford examined only Labour Party support; he found no significant pattern of political regionalism. He did find higher levels of class voting in Scotland and Wales, but he concluded that the higher levels of class voting in these areas reinforced his conclusion that class alone is important in Britain.¹³ Butler and Stokes, on the other hand, limited their analysis to Conservative and Labour Party supporters, systematically excluding the supporters of the Liberal Party and the Celtic nationalist parties. This procedure obscures the association between cultural peripherality and support for the third parties. Finally, Hechter analyzed only Conservative Party support. Hechter's partial findings have been duplicated here; how-

¹² This is not to imply that political mobilization based on status is necessarily associated with, or generated by, the middle and upper classes. Certain structural conditions such as a split labor market (Bonacich, 1976:34-51) may encourage status-based political mobilization within the working class.

¹³ The fact that there is a higher level of class voting in Scotland and Wales supports the argument that I have presented here—that class cleavages in peripheral areas encourage the local dominant strata to emphasize local culture.

ever, the analysis reported in this paper does not support his argument that the Labour Party has served as an outlet for Celtic peripheral sectionalism.

Peripheral areas in Britain exhibit sectionalism with respect to Conservative Party support, but this does not mean that class factors are irrelevant in these areas. In the Celtic areas of Britain, there is a concomitance of class factors in the vote for Labour and status-based opposition to the Conservative Party. The relative weakness of the Conservative Party, therefore, represents not a failure of class-based political action, but a failure of the Conservative Party among its "natural" constituency.

These findings support the argument that (1) status-based peripheral opposition originates among the local dominant strata of a peripheral area and (2) such opposition is arrested, or at least limited by class cleavages. Thus, emphasis on local culture in peripheral areas may be based in both class and status factors; it cannot be explained simply by one or the other.

The fact that core-periphery conflict originates in and is limited by class cleavages suggests that the primary theoretical question is not whether peripheral opposition is a product of economic gaps between regions. Of greater concern is the relationship between status-group affinity and class affinity in peripheral areas. Some class cleavages in peripheral areas have been transformed into unified opposition to a core area. The evidence I have presented here, however, suggests that the class cleavages that may accompany industrial development (as in Scotland and Wales) hinder the extension of status-group affinity. Thus, the specialized, industrial development of these areas may have stymied the formation of Scottish and Welsh "nations."

Though Hechter misspecified the nature of Labour support in Scotland and Wales, his contrast between Ireland, on the one hand; and Scotland and Wales, on the other, is consistent with this view:

Irish secession is best explained by the *particular mode of dependent development* which emerged in Ireland during the period 1846-1921. This involved the evolution of a relatively capital intensive agrarian regional

economy which did not lead to the substantial inter-regional [working class] organizational affiliations as occurred following the highly restricted industrial development of Wales and Scotland. (Hechter, 1975:292, emphasis added)

Indeed, it is the particular mode of dependent industrial development in Scotland and Wales which best explains the type of (limited) peripheral opposition illustrated in the analysis above.

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THE EFFECT OF POLITICAL DEMOCRACY AND SOCIAL DEMOCRACY ON EQUALITY IN INDUSTRIAL SOCIETIES: A CROSS-NATIONAL COMPARISON*

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This paper considers the effect of political democracy on the stratification systems of non-communist industrial societies. In contrast to previous research, this study assumes that the effect of democracy will be incremental and, therefore, that the historical experience of democracy must be considered rather than the current political situation. Two hypotheses are suggested: that democracy itself will lead to equality and that only the election of socialist legislatures will lead to equality. The historical experience of democracy and that of socialist legislatures is related to five measures of inequality. It is concluded, after taking into account the level of economic development and the growth rate, that although democracy itself has little effect, the experience of democratic socialist parties is significantly related to variations in inequality. The stronger the democratic socialist parties, the more egalitarian is the contemporary class system.

One important controversy in political sociology and stratification theory concerns the effect of political democracy upon social equality. Some writers argue that political democracy has resulted in a significant modification of the class system in an egalitarian direction. Others take the view that there is no consistent association between political democracy and economic equality, or that the association is spurious once such factors as the level of economic development are taken into account. This paper considers the effect that political democracy has had upon

the stratification systems of non-communist industrial societies.¹

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¹ Since the effect of political factors may be very different in developed and underdeveloped nations, the analysis has been limited to those countries which can be considered "modern industrial." A nation was defined as modern industrial if it was above the mean in both per capita income and per capita energy consumption in 1965 according to the tables in Taylor and Hudson (1972: Tables 5-5, 5-7). Thirty-four countries fitted both these criteria, and four countries fitted one. The thirty-four countries were reduced to twenty-five by the exclusion of seven communist nations and two countries (Kuwait and Iceland) for which no equality measures could be found. The measures of equality used in the study refer in almost all cases to the 1960-1965 period. A further reason for focusing on developed countries is given by Wilensky (1975:xiii) who argues that it is "necessary because of well-known limitations of data. Rich countries are at once rich in goods and services and rich in data; statistical records of less affluent countries are, in contrast, quite weak." The position adopted in this paper is similar to Wilen-

THEORETICAL BACKGROUND

The view that political democracy leads to greater economic equality is an old one and was held by both radicals and conservatives in the late eighteenth- and early nineteenth-century period. During the debate over the 1832 Reform Bill in England (which increased the franchise to 3.1% of the total population), John Stuart Mill foresaw the prospect of a revolution that would "exterminate every person in Great Britain and Ireland who has £500 a year." John Wilson Croker, the leading Tory publicist of the day, expected the result would be "no King, no Lords, no inequalities in the social system; all will be leveled to the plane of petty shopkeepers and small farmers" (Jaher, 1973:206). More recently, Lenski (1966:312-25) has revived the argument, although in a slightly ambiguous fashion. Initially, Lenski makes a purely deductive argument about the redistributive significance of democracy based upon two assumptions. The first assumption is that "those who control the government are able to determine the rules governing the competition for rewards in society, and by virtue of this power are able to influence profoundly the outcome of this competition." The second assumption is that political democracy has shifted significantly the distribution of political power from the rich to the poor. However, in his subsequent discussion of "the outcomes of the struggles for power in the more advanced industrial democracies," Lenski formulates his argument in terms of the relative significance of socialist parties in different countries. This suggests that one may put forward two alternative hypotheses as to the effect of political democracy upon equality: the "social democratic" and the "simple democratic" hypotheses. No assumption is made that these hypotheses are related nor that democracy will lead to socialism.²

sky's, that a small number of accurate and appropriately chosen observations are of more value than a large number of poor ones.

² For the sample of countries analyzed in this study, the correlation between democratic experience and socialist strength is only .30, which is not statistically significant at the .05 level.

Social Democratic Hypothesis

Political democracy is not a sufficient condition for the achievement of a more equal society. The crucial matter is what the mass electorate *does* with the franchise and other democratic procedures. Only if the lower classes use their votes to elect socialist governments will democracy result in more equality, since non-socialist governments will not be concerned with redistribution and social equality (Parkin, 1971:105).

Simple Democratic Hypothesis

Regardless of the strength of socialist parties, the existence of a mass electorate in which a majority of the voters are drawn from the working class will have egalitarian consequences. This will happen because generally there will be a class polarization of voting so that one party will be "the working-class party" in terms of support and will function in the same fashion as an explicitly socialist party. Lipset (1960) provides a much-quoted summary of this view in which elections are seen as "the expression of the democratic class struggle." Ausubel (1960) claims that the New Zealand Labor Party is no more radical than the Democratic Party in the United States. Also, the parties which manifestly represent middle-class interests or non-class interests will have to attract substantial working-class support and will carry out some policies to benefit them. The social policies of conservatives such as Bismarck and Disraeli, for example, are conventionally interpreted as responses to the increase in the franchise and as attempts to reduce the appeal of the growing socialist movement.

The empirical evidence presented by Lenski in support of either hypothesis is inconclusive. He shows that "industrial" societies have a much more equal distribution of income than do "agrarian" societies and also a much greater diffusion of political power. Yet, since the difference in income distribution between the two types of society could be a result of economic differences, the redistributive effects of democracy are unproven. His view that socialist countries are more

equal than non-socialist countries is supported only by impressionistic evidence.³

Those who argue that political democracy has not resulted in a more equal stratification system fall into two schools, the Functionalists and the Marxists. The Functionalists argue that the form of government, whether democratic or non-democratic, socialist or non-socialist, does not affect the stratification system since the "needs" of modern industrial economies will require similar differentials in earnings between occupational groups, similar mobility rates and similar government policies, etc. Any changes in the distributive system are explained as a result of the "logic of industrialism." Industrial and technological changes lead to a convergent pattern of development in the stratification systems of all advanced industrial societies, regardless of politics.⁴

Like the Functionalists, though for different reasons, the Marxists assume that politics, as practiced in non-communist societies at least, has very little impact. Through its economic power, the capitalist class is able to dominate or control the state and, in turn, use their power over the state to maintain their economic position. In consequence, formal democracy is largely a sham. Social democratic parties have used an egalitarian rhetoric, but have rarely translated this into effective policies (Miliband, 1969:22; Parkin, 1971:105).

³ Lenski (1966:308-18) argues that the Swedish Social Democratic Party's policies "have been directed consistently toward the goal of advancing the interests of the working class, and have resulted in the creation of a welfare state in which social inequalities of nearly every kind have been substantially reduced In the United States, the situation is quite different" (Lenski, 1966:320). The only comparison of the actual degree of inequality in Sweden and the United States, however, is to be found in his later book, *Human Societies* (Lenski, 1974:370), where a table showing the distribution of wealth in the two countries is described as showing the "modest success [of the working class] in its efforts to effect a redistribution of income and reduce the economic and political power of the propertied elite."

⁴ The most cited statement of this position can be found in Kerr (1964). Inkeles and Rossi (1956) have shown that the ranking of occupations is very similar in industrial societies, and Mishra (1973) shows that there is a convergence in their social welfare policies.

Existing research on cross-national variations in economic equality generally has concluded that differences in political systems are not associated with the degree of inequality, or that such variations can be adequately explained by economic factors. Cutright (1967a) found that variations in "intersectoral income inequality" among high GNP nations were not related to differences in their "Political Representativeness Index" scores and could be explained instead by four economic factors.⁵ In a second paper, Cutright (1967b) suggested that the variance in the percent of GNP allocated to Social Security expenditures is mainly attributable to variations in the "experience and power of the Social Security bureaucracy." After this is correlated with expenditures, the percent of the voting-age population voting in national elections during the 1950s (his measure of equalitarian pressure in the political system) has no additional effect. Parkin (1971) measures "the impact of socialist governments on the reward structure of capitalist society" by comparing the stratification systems of Western countries which have had significant periods of social democratic government with those which have not. He makes a simple dichotomy between the Scandinavian countries and Britain, on one hand, and the other countries of Western Europe, on the other, and compares the two groups on various measures. The findings are that social mobility and educational opportunity are greater in the social democratic countries, but that there are no differences in either the provision of social services or in income differentials between oc-

⁵ More precisely, since "there is no meaningful difference among these nations in their Political Representativeness Index scores," Cutright omits this variable from his analysis. It should be noted that Cutright does not consider his four variables (proportion of farms rented, military participation ratio, capital formation and proportion of foreign trade) to be purely economic but, rather, indirect indicators of power relations between nations or between groups in a given society. It should be noted also that six of the nations considered in the present study are classified as having middle levels of GNP per capita by Cutright, and that for middle GNP per capita nations, the index is significantly related to the inequality measure.

cupational groups between the two groups of countries.

Wilensky concludes, on the basis of a cross-sectional analysis of sixty-four countries, that economic growth and its demographic and bureaucratic outcomes are the root cause of the general emergence of the welfare state. In any systematic comparison, "alternative explanations collapse under the weight of such heavy, brittle categories as 'socialist' versus 'capitalist' economies, 'collectivistic' versus 'individualistic' ideologies, or even 'democratic' versus 'totalitarian' political systems . . . these categories are almost useless in explaining the origins and general development of the welfare state" (Wilensky, 1975:xiii, 47). However, for the most developed nations, there are "sharp differences in the level of welfare" spending, and Wilensky (1975:50ff.) suggests that socio-political factors may affect such variations.

The most recent research reported is by Jackman (1974; 1975), who concluded that neither political democracy nor socialism have any significant effect on three measures of social equality once the level of economic development is taken into account.

Although some of these findings can be interpreted in ways that allow more significance to political factors, the consensus of these studies is that neither democracy itself nor the use of democracy to elect socialist governments has any important independent effect in reducing inequality. However, such a conclusion requires that one accept the various measures of social equality, democracy and socialism that have been used in these studies. It will be argued below that the measures previously used are not valid indicators of any of these concepts and alternative measures will be proposed.

THE DEPENDENT VARIABLES

Measures of social equality fall into three groups: measures of government redistribution, measures of income inequality and measures of opportunity.

Government Redistribution

Cutright and Parkin both use the proportion of GNP devoted to social service expenditures as a measure of government redistributive efforts. However, social service expenditures are not a good measure of the redistributive effect of government activities. This is because although such expenditures disproportionately benefit lower socioeconomic groups and individuals, the money spent on social services is, in part, raised from taxes upon these same groups and individuals. To estimate the net redistribution carried out by the government, one must simultaneously consider the effect of both the taxation system and social service expenditures. At present, the data for a sophisticated analysis do not exist, but a simplified measure can be calculated which will allow cross-national comparisons. On the assumption that direct taxes on income are generally "progressive" while indirect taxes (sales taxes, etc.) are generally "regressive," it can be argued that *only* that proportion of social service expenditures which is raised from direct taxes should be considered as a measure of net redistribution.⁶

Income Inequality

Cutright (1967a) and Jackman (1974; 1975) both use a rather unsatisfactory measure of income inequality termed "intersectoral income inequality," which measures the disproportion of income and population between industrial sectors. This is viewed by both writers as a substitute measure for *individual* income distribution data which are unavailable for many of the low- and middle-income

⁶ For an analysis of redistribution in Britain under the postwar Labour Government which illustrates well the intellectual issues in the controversy, see Peacock (1954) and Cartter (1955). The figures in the column headed "Redistributive Effect" exaggerate the net effect of government budgets since personal income taxes are calculated as a percentage of personal income rather than GNP, as is the case for indirect taxes. This makes the ratio used higher than the real statistic of direct to total taxes for all countries.

Table 1. Redistributive Effect of Government Budget (1962-1964)*

	Income Tax ^b	Indirect Taxes ^c	Social Service Expenditures ^d	Redistributive Effect ^e
Sweden	19.6	12.6	15.6	9.5
Austria	13.3	15.6	20.2	9.3
Netherlands	13.1	10.1	15.6	8.8
Germany	11.4	14.3	18.6	8.3
Norway	18.8	14.4	12.2	6.9
New Zealand	16.1	10.5	11.3	6.8
Belgium	8.5	12.2	16.2	6.7
Denmark	14.5	13.9	12.7	6.5
United Kingdom	11.3	13.6	13.2	6.0
Finland	11.8	13.2	11.1	5.2
Switzerland	9.1	7.3	9.2	5.1
United States	12.5	9.4	7.9	4.5
Australia	10.1	11.1	8.9	4.2
France	5.5	17.7	17.8	4.2
Canada	7.3	14.1	10.6	3.6
Ireland	4.7	16.5	9.9	2.2
Japan	5.6	8.2	5.5	2.2

* Table includes all non-communist countries listed in Wilensky (1975: Table 2); except Israel and Italy, for which no figures on personal income were available.

^b Column 1 from U.N. (1967: Table 6.3), except non-European countries. Personal income tax expressed as a percentage of personal income (1962-1964 average).

^c Column 2 from U.N. (1967: ch. 6, p. 9), except non-European countries. Indirect taxes expressed as a percentage of GNP (1962-1964 average). Figures for U.S., Canada, New Zealand, Australia and Japan in columns 1 and 2 calculated by author for same years from U.N. Yearbook of National Accounts Statistics (1966).

^d Column 3 from Wilensky (1975: Table 2). Social Security spending expressed as a percentage of GNP in 1964.

^e Column 4 = $\frac{\text{Column 1}}{\text{Column 1} + \text{Column 2}} \times \text{Column 3}$.

countries they analyzed.⁷ This assumption of a congruence between the two measures is not accepted by Taylor and Hudson (1972)⁸ nor supported by the re-

⁷ See Cutright (1967a:564). "Kuznets . . . using limited data and economic theory, argues persuasively that nations ordered by the degree of inequality on intersectoral income will have much the same order as nations ordered by individual earner measures of inequality."

His final comment that "this is powerful validation of Kuznets' theory, and strengthens our use of intersectoral inequality scores in place of (unavailable) inequality data based on individuals" clearly implies that intersectoral inequality is a proxy measure for individual income inequality. Jackman (1975:20) says that "unfortunately, income data collected at the family or consuming-unit level are not available for enough countries for us to be able to test this argument cross-nationally. Such data (if available) would be preferable to the intersectoral data, which are used as an approximation to the disaggregated data."

⁸ Taylor and Hudson (1972:213) say of intersectoral income measures that "the kind of index does make a difference In any event, the series . . . must not be used as if it measured inequality of income among households; it does not."

search of Husbands and Money (1970). Certainly, if a choice were possible, it is hard to see why intersectoral would be chosen over individual income data, since almost all the theoretical literature on income inequality obviously is referring to the latter kind of distribution. A statement that income was becoming more equally distributed in a particular society rarely would be taken to imply that "electricity, gas, water, and sanitary service" workers' incomes were becoming more similar to those of workers in "mining and quarrying" (two of the eight industrial sectors). Instead, it would be assumed that the statement referred to lower-income individuals and households receiving a higher proportion of the national income.

The distributions of intersectoral inequality relate quite well to economic development—the greater the development, the less the inequality among sectors—but for nine countries, Kuznets found no significant relationship between Gini numbers based upon household data and Gini numbers based upon sectoral data" (emphasis in the original).

It could be argued that the accuracy and comparability of household income distribution data leave much to be desired. While this is so, such an objection could be raised against almost all cross-national series. Furthermore, in the present study, individual or household income data are calculated only for industrial societies with well-developed government bureaucracies, while over half the countries in the studies of Cutright or Jackman are non-industrial with small and inefficient statistical reporting systems and large subsistence sectors. Any claim that their sectoral income measures were more accurate than the household income measures used in this study would be hard to support.

Parkin (1971) measures income inequality by the ratios of the average earnings of different occupational groups (e.g., unskilled labor to managerial). Two obvious problems in using these data are that occupational classifications vary between countries and that income differentials *within* occupational categories are ignored (e.g., between high paid and low paid unskilled laborers).

Three measures of income inequality are used in the present study, all of which measure the distribution of income between individuals or households. The first measure is the spread of earned income between labor force participants and is calculated by taking the difference between the earnings of high- and low-paid percentiles of workers and expressing this as a percentage of the median income. The resulting figure yields a measure of the range or spread of earnings. The statistics used are those calculated by Lydall (1968) and Pryor (1971).⁹

The labor force measure ignores one important source of inequality in capitalist society, unearned income accruing to the owners of property. A large proportion of this, however, goes to the highest income

groups and the second and third measures, which are the shares of national income going to the highest twentieth and quintile of income recipients, include the effect of this as well as the effect of high salaries. The most recent and comprehensive set of statistics on the size distribution of income are those of Paukert (1973), which, with minor modifications suggested by Atkinson (1975), are used in the present study.¹⁰

Equal Opportunity

The mobility rates between classes provide one measure of the "openness" of the stratification system. However, mobility studies such as the one by Miller (1960) which Parkin cites are based on competed mobility by mature adults of varying ages and do not provide a measure of *current* mobility chances. Given that this is now largely mediated by the opportunities for education available, it seems reasonable to use access to higher education as a measure of opportunity.

Access to higher education by the lower classes is a result of two factors in combination, the *amount* of higher education and the *proportion* of university students from the lower classes. The index of opportunity is constructed, therefore, by multiplying the proportion of students receiving higher education (as a percentage of the 20–24-year-olds) by the proportion of non-middle-class students in higher education.¹¹

¹⁰ Paukert's figures are derived in large part from a compilation by Adelman and Taft Morris (1973) and include data on the distribution of income by size for 56 countries. Atkinson (1975) has suggested some minor changes in Paukert's figures. The literature on the distribution of income by size is extensive and, in addition to the classic studies by Kuznets (1963) and Kravis (1960), Pen (1971) and Champernowne (1973) should be consulted. Recent analyses by some economists have suggested that statistics on household income exaggerate the degree of inequality in developed societies. Prosperity enables old and young to live in separate households and paradoxically increases the proportion of poor households. In less developed societies with more traditional family systems, young adults and the old live as dependents in extended families whose aggregate family income is higher (Paglin, 1975). This suggests again the advantage of comparing societies at the same general level of development.

¹¹ The statistics are taken from OECD (1971)

⁹ The statistics were first calculated by Lydall and refer to "male adults, in all occupations, in all industries except farming, in all areas, working full-time and for the full period. The income measured should be money wages and salaries only, and before tax" (Lydall, 1968:60). Pryor (1971) further developed the measure and calculated the statistic for some additional countries.

INDEPENDENT VARIABLES

The effect of political democracy or of socialist parties on equality clearly has not been revolutionary. In fact, it has been precisely the belief in the possibilities of gradual incremental reform within a democratic context that has always distinguished social democratic parties from revolutionary Marxist parties, whether Communist or Trotskyite. The implication of this is that it is not the current situation as regards democracy or socialism that must be considered but, rather, the historical experience over the last few decades. Unfortunately, both of Cutright's papers and Jackman's recent paper and book define democracy and socialism in terms of the contemporary situation in the countries they examine while ignoring the historical dimension almost entirely. For example, Jackman's (1975:127) "measure of Socialist Party strength is the proportion of seats in the national legislature held by parties of the non-Communist left . . . taking the average of the number of such seats in both the last legislature created before 1960 as well as the first created after that date." Thus, the country, which, according to Jackman, displays the greatest socialist party strength, is Venezuela, which is scored as 91.5% socialist. However, to expect Betancourt's *Accion Democratica* government to have achieved much in the way of egalitarian reforms is somewhat unfair, since it had only been in power since 1958. Prior to that (except for one election and less

compilations which break down the social background of students into five categories, A, B, C, D and E. The A and B categories are higher and lower white-collar parents, respectively, while the C and D categories include "independent agriculturalists" and "self-employed workers." The E category is referred to as "lower-" class occupations. It was decided to define non-middle class as including all the non-A or B background students, rather than as just the E background students. This is based on two considerations. In most countries (particularly in Western Europe), the C and D categories are more reasonably considered as belonging to the lower strata (in terms of income and education, for example) than they are to the white-collar occupations. Also, since not all the countries report C and D social origins, it seems likely that in such cases they are grouped under the lower class which makes variations in the E category somewhat suspect.

than two years of democratic politics in 1946-1948), Venezuela had been ruled by a variety of military dictators. The figures are similarly misleading for almost all the third world countries included in Jackman's sample, since, if they did have socialist parties in 1960, these were comparatively recent innovations after decades of colonial or dictatorial rule. The same reasons can be given for dismissing Jackman's measure of democratic performance and, to a lesser degree, objecting to Cutright's measures.¹²

Democratic Experience

There are many possible definitions of political democracy. At a minimum, however, three characteristics must be possessed by a state before popular usage would justify the term being used, and which might have egalitarian consequences.

(1) The executive must be elected, or must be responsible to an elected assembly.

Although in some countries, the transition to responsible government can be marked precisely when revolutions overthrew absolutist monarchies or constitutions were granted, in many cases the transition is a gradual process. Hence, the most that can be said about the selection of a particular date is that its arbitrariness is inevitable and, where possible, follows the conventional opinion of most historians.

(2) There should be universal manhood suffrage.

Universal suffrage is considered to exist when virtually all adult males have the vote. Disfranchisement on account of age or sex is ignored, since this study focuses

¹² Cutright's (1967a) Political Representativeness Index refers to the 1945-1954 period and his voting measure refers to the proportion of adults voting in national elections during the 1950s (1967b). Jackman's (1975:64-5) measure of democratic performance appears to relate to a single election or point in time. Largely in consequence of this, there is only a very slight variation in democratic performance scores between the industrialized countries (two score in the forties, one in the seventies, nineteen in the eighties and Trinidad, with a score of ninety, is, surprisingly, the most "democratic" country in his sample).

on the consequences of democracy for the class system.

(3) Elections should be "fair," as indicated by the presence of a secret ballot.

Only one indicator of electoral "fairness" is used, the adoption of the secret ballot, since this prevents or greatly reduces intimidation, bribery, and most other abuses, and is also probably symbolic of a general reform orientation (Rokkan, 1961). Secret ballots were used in the United States and France before the data cited, but were so ineffective that the dates given refer to the introduction of the "Australian ballot."

Democratic scores have been calculated on the assumption that to be democratic a state must *simultaneously* fulfill all three requirements. The scores represent the number of years since the states became fully democratic, less any subsequent period of dictatorship or foreign military occupation.

Currently, almost all the non-communist industrial societies which will

be considered in this paper are democracies as evaluated by the criteria above. However, there are important differences between these societies in terms of how long they have been democratic as can be seen from Table 2.

Experience of Social Democracy

The term "social democratic" is used here to describe those movements and parties which avowedly embody socialist ideals or claim to represent the working class or labor interest, and also are committed to the norms of representative democracy. Specifically, social democracy is defined as including those parties which are affiliated to the Socialist International, thereby guaranteeing a certain ideological and programmatic homogeneity to the grouping.¹³ Alternative definitions are, of

¹³ The People's National Movement of Trinidad had contacts with the Socialist International until 1962 and fought the 1958 federal West Indies elec-

Table 2. Democratic History to 1965

	Universal Adult Male Suffrage	Secret Ballot	Responsible Government	Later Non- Democratic	Years of Full Democracy
Switzerland	1848	1872	1848		93
New Zealand	1879	1870	1856		86
Australia	1858	1859	1892		73
Canada	1898	1874	1867		67
Norway	1898	1884	1884	1940-44	62
United States	1870 ^a	1904	1789		61
Sweden	1908	1866	1917		48
Finland	1907	1907	1917		48
France	1848	1913	1875	1940-44	48
United Kingdom	1918	1872	1832		47
Denmark	1917	1901	1901	1940-44	44
Netherlands	1918	1918	1848	1940-44	43
Luxembourg	1918	1879	1868	1940-44	43
Belgium	1919 ^b	1877	1831	1940-44	42
Ireland	1918	1872	1923		42
Austria	1907 ^b	1907	1918	1934-44	37
Germany	1867	1867	1918	1933-48	32
Italy	1919	1882	1959	1925-44	27
Argentina	1916	1916	1862	1930-55	24
Puerto Rico	1947	1947	1947		18
Israel	1920	1920	1948		17
Japan	1924	1902	1952	1932-51	13
Venezuela	1947	1947	1947	1948-58	9
Trinidad	1946	1959	1946		7
South Africa	1910	1910		0

^a Ignores *de facto* exclusion of blacks after Reconstruction. White males had the franchise by the 1850s.

^b Refers to "one man, one vote" reform.

Main Sources: Seymour and Frary (1918); Rokkan and Meyriat (1969); Mackie and Rose (1974).



course, possible. Jackman takes the term "Socialist" to be synonymous with "non-Communist Left" as used in the *World Handbook of Political and Social Indicators*. The original source of the data is an annual U.S. State Department publication primarily concerned, according to its title, with the *World Strength of Communist Party Organizations*. Incidental to this purpose, the State Department compilers provide a rough classification of other political groupings. There is no description of the rationale behind this classification into "non-Communist Left," "Center" and "Conservative" parties; for example, whether it measures domestic liberalism or foreign policy alignments or both.¹⁴ Nor does the State Department usage appear to be consistent between countries nor even within the same country at different times.¹⁵ The concept of a "Left-Center-Right" spectrum, though of considerable utility in most Western politics, should be used with extreme caution outside this context as almost every student of third world politics stresses.¹⁶ The U.S. State De-

partment category of "non-Communist Left" is a wide and heterogeneous grouping of parties unlikely to play a uniform role. It includes Catholic parties, middle-class radical parties and even groups such as the Peronists of Argentina.¹⁷

The measure of socialist party strength used here is the annual average proportion of seats held by socialist parties in the national legislature, over the first twenty postwar years.¹⁸ This measure takes account of the fact that socialist parties may have an impact even when in opposition as the non-socialist parties try to forestall the appeal of the socialists by adopting some of their policies.

FINDINGS

It has been hypothesized that the experience of either democracy or socialism might have an egalitarian effect upon stratification systems. The most straightforward test of this is to see whether there is any association between the two political history measures and the five measures of equality. Table 4 presents the simple correlations between democracy and socialism and each of the measures of equality. The figures show that at least one political variable is significantly associated with each of the equality measures. The statistically significant associations are as follows. The more democracy or socialism a country has experienced, the lower the share of the national income taken by the highest income categories. The more democratic experience, the narrower the range of earnings between high and low paid work-

tions as a constituent of the Federal Labour Party (Ayeerst, 1960:213). It is, therefore, included as a socialist party, though no longer in contact with the International.

¹⁴ Thus, in Guyana, the People's Progressive Party split into two factions, one led by Burnham and one by Jagan. The polarization was primarily on racial (i.e., Negro versus East Indian) lines, and the two groups had no discernible domestic policy differences. However, Jagan's flirtation with a pro-Soviet/anti-American foreign policy led to his faction being dubbed "Communist" by the U.S. State Department.

¹⁵ An example of this is in the treatment of Trinidad's PNM which, as part of the Federal Labour Party, won four out of ten seats in the 1958 federal elections and twenty out of thirty seats in the 1961 elections. The Democratic Labour Party won the remaining seats in both elections. In 1958, the State Department classified the PNM as "non-Communist left," but as a "center" party in 1961. Therefore, Trinidad's "Socialist" score involves the same party being counted once as Socialist and once as non-Socialist.

¹⁶ Gil (1966:44) says of Chile that it "is the only Latin American country where political forces are clearly and distinctly aligned . . . into three great blocks: Right, Center and Left." The State Department (1968:81) publication itself comments regarding Japan that "political parties are divided into 'ins' and 'outs' and the term 'center' has little meaning" and, in the 1961 edition, classified most Latin American

parties according to whether they were "Ruling" or "Opposition."

¹⁷ In the four third world countries considered here whose parties are classified by the State Department, only four out of twelve "non-Communist left" parties are members of the Socialist International. Furthermore, two parties which were associated with the Socialist International, the Democratic Socialist Party of Japan and the PNM of Trinidad, are classified as "center" parties.

¹⁸ Alternative measures of the strength of socialism such as the proportion of workers unionized, the socialist party vote or the years in which socialist parties were governing had very high intercorrelations and all correlation coefficients were significant at the .05 level.

Table 3. Average Post-War Legislative Strength of Socialist Parties 1945-1965*

Norway	52.0
Israel	50.8
Sweden	48.5
United Kingdom	48.5
Australia	45.0
New Zealand	44.9
Austria	44.7
Denmark	41.8
Belgium	37.4
Luxembourg	32.6
Netherlands	30.8
Venezuela	28.7
Japan	27.5
Germany	27.1
Switzerland	25.8
France	25.1
Finland	24.9
Trinidad	18.8
Italy	17.0
Ireland	10.5
Canada	7.1
Argentina	2.3
South Africa	1.8
Puerto Rico	0
United States	0

* Figures are expressed as a percentage of Socialist Party strength in post-war legislatures, each separate legislature weighted by duration of legislature.

Source: Mackie and Rose (1974). List of Socialist Parties provided by Hans Janitschek, General Secretary of Socialist International.

ers. Strong socialist movements are associated with a greater proportion of the national income being redistributed. All these associations are in the anticipated direction. However, no association is found between democracy and access to higher education by non-middle-class youth and strong socialist movements appear to be *inversely* related to such access. These results clearly contradict Parkins' argument that the socialist countries are not more egalitarian, but are more open societies in which educational opportunities are greater for the lower strata. Table 4 suggests the opposite conclusions; socialist societies are more equal but less educationally open than the less socialist countries. This finding could perhaps be explained by Lipset's argument concerning the inverse relationship between a concern with equality and a concern with achievement (or equality of opportunity). Socialist countries stress

Table 4. Association between Experience of Democracy or Socialism and Equality

	Democracy	Socialism
Redistributive Effect of		
Government Budget	-.05	.58*
Share of Top 5%	-.54*	-.53*
Share of Top 20%	-.57*	-.57*
Labor Earnings Spread	-.55*	-.33
Access to Higher Education		
by Non-Middle Class	-.16	-.46*

* Pearsonian correlation coefficients significant at .05 level.

the former, while the more "liberal" countries emphasize the latter (Lipset 1963: 259-61, 321).

Some other critics of the thesis that political factors, such as democracy or socialism, cause societies to become more egalitarian have taken a different and slightly more complex position. Critics, such as Jackman (1975:85, 151), argue that although democracy (or socialism) may be positively associated with equality, this relationship is seen to be spurious once the level of economic development is taken into account. In other words, economic development is responsible for both the growth of democracy (or socialism) and for the decline in inequality, but there is no direct causal relationship between the political variables and inequality. That there is a significant positive relationship between the level of economic development and income equality appears unquestionable. It was noted in the pioneering work of Kuznets (1955; 1963), Oshima (1962), Kravis (1960) and Lydall (1968), and recent analyses on larger samples of countries by Adelman and Taft Morris (1973) and Paukert (1973) have come to the same conclusions. There are some disagreements as to the form that this relationship takes, but they are not relevant for the present analysis.¹⁹ There appears to be a similar positive rela-

¹⁹ Kuznets (1955) thinks that inequality may have increased in the early phases of economic growth, then stabilized, and finally narrowed. On the other hand, Jackman argues for a curvilinear relationship in which economic development has less effect in reducing inequality at the higher levels of development. However, since the present analysis is concerned with only the most developed countries, the relationship can be assumed to be linear.

tionship between economic development and the growth of the welfare state (Cutright, 1967b; Wilensky, 1975). The fiscal system in developed countries places more reliance upon direct rather than indirect taxes (Musgrave, 1969:142-7), which also seems likely to have egalitarian effects. It has also been suggested that a connection between high rates of economic growth and inequality may exist (Olson, 1963) and Jackman's data (1975:43-7) appear to support this argument.

To see whether the political variables are still related to inequality even after taking into account the two economic variables, multiple regression analysis was used. In order that this test of the effect of the economic variables be a thorough one, both economic development and economic growth were operationalized in two ways. Economic development was measured in terms of GNP/capita and also in terms of energy consumption/capita, while economic growth was measured in terms of the average annual rate of change in both of these measures during the 1950-1965 period (Taylor and Hudson, 1972). Table 5 presents the results of the regression analysis.²⁰

In eleven of the forty multiple regression equations, the economic control proved to have a significant impact upon the equality measure. Economic development was a much better control than the economic growth rate, being significantly related to equality in eight cases of the eleven.

The data do not support the argument that the relationship between socialism and equality is a spurious one when economic factors are taken into account. Socialism is consistently and positively related to government redistribution and to a lowering of the share of the top income groups, regardless of the economic control being considered.

The relationship between democracy

²⁰ Space limitations dictate that only the results of the regressions which operationalize economic development or growth in terms of energy consumption are shown. The energy consumption measures have a slightly stronger relationship to the equality measures than do the GNP measures.

and income equality does appear less obvious when economic variables are considered simultaneously. The previous association between democracy and labor force earnings spread disappears whatever economic control is used. Also, controlling for GNP/capita makes any association between democracy and any measure of equality disappear. It is therefore plausible to assume that the apparent relationship between democracy and equality is indeed a spurious one.²¹

To test the hypotheses with a more fully specified model, all the variables (socialist experience, democratic experience, economic development and economic growth rate) are considered together to see what relative effect they have on the equality measures. For the most part, Table 6 reveals the same pattern found in earlier models. Socialist experience is significantly related in the predicted direction to redistribution through government budgets and to the share of national income going to the top income groups. Democratic experience exerts no effect on the equality measures, with the exception of the share of national income going to the top five percent of income recipients.

DISCUSSION

The preceding analysis suggests that while one may reject the "simple democratic hypothesis," the evidence on cross-national variations in equality is clearly compatible with the "social-democratic hypothesis." Strong socialist parties acting within a democratic framework appear to have reduced inequality in industrial societies. If one accepts this conclusion, it has highly important implications. First, it encourages op-

²¹ The independent variables are associated as follows. Pearsonian correlation coefficients significant at the .05 level are starred.

	2	3	4
1. Democracy	.30	.40*	-.67*
2. Socialism		-.05	-.26
3. Economic Development			-.50*
4. Economic Growth			

It is possible that the insignificant relationship between democracy and equality when we control for economic variables may be due to the collinearity between economic variables and democracy.

Table 5a. Socialism and Equality Controlling for Economic Growth

Dependent Variables	Redistribution	Labor Force Earnings	Educational Opportunity	Share of Top 5%	Share of Top 20%
Intercept	36.774	123.539	113.692	276.519	501.028
Socialism (a)	.082	-.062	-.131	-.205	-.158
(b)	.559	-.306	-.581	-.541	-.550
(c)	6.53*	1.89	5.39*	6.43*	7.03*
Economic (a)	-.154	1.139	-.649	-.149	.187
Growth (b)	-.118	.544	-.371	-.059	.098
(c)	.29	5.96*	2.19	.08	.23
\bar{R}^2	.31	.36	.28	.24	.29
F	3.78*	4.05*	2.97	(3.19)	3.98*

5b. Socialism and Equality Controlling for Economic Development

Intercept	16.959	200.820	31.912	318.520	559.005
Socialism (a)	.097	-.096	-.078	-.210	-.171
(b)	.667	-.475	-.345	-.553	-.598
(c)	8.52*	(3.77)	(3.68)	8.28*	12.61*
Economic (a)	.026	-.080	.118	-.125	-.124
Develop- (b)	.222	-.507	.652	-.360	-.475
ment (c)	.94	4.29*	13.12*	(3.52)	7.95*
\bar{R}^2	.34	.29	.59	.38	.52
F	4.27*	(3.12)	9.88*	5.58*	9.69*

5c. Democracy and Equality Controlling for Economic Growth

Intercept	94.226	145.051	54.020	369.627	553.715
Demo- (a)	-.386	-.576	.133	-2.751	-1.783
cracy (b)	-.335	-.326	.071	-.877	-.752
(c)	1.03	1.12	.04	13.11*	8.43*
Economic (a)	-.571	.706	-.224	-1.321	-.542
Growth (b)	-.437	.337	-.128	-.527	-.286
(c)	1.74	1.20	.11	4.75*	1.22
\bar{R}^2	.05	.32	.04	.42	.33
F	.89	3.49*	.21	6.59*	4.72*

5d. Democracy and Equality Controlling for Economic Development

Intercept	62.920	187.786	6.941	274.240	520.320
Demo- (a)	-.052	-.857	-.104	-1.795	-1.174
cracy (b)	-.045	-.486	-.056	-.572	-.495
(c)	.03	(3.26)	.07	4.16*	(3.30)
Economic (a)	-.003	-.022	.132	.018	-.028
Develop- (b)	-.028	-.140	.728	.052	-.109
ment (c)	.01	.27	11.84*	.03	.16
\bar{R}^2	.06	.27	.47	.25	.29
F	.02	2.82	6.23*	3.28*	3.93*

Notes: (a) Figures refer to unstandardized coefficient.

(b) Figures refer to standardized coefficient.

(c) F Value; starred where significant at the .05 level; parentheses indicate F value almost significant.

timism about the possibilities of political action to reduce inequality. Second, the finding that political factors do affect the stratification system has obvious relevance for the two major orientations in stratification theory, Functionalism and Marxism, both of which deny or ignore

such a possibility. Our findings are even more significant because, almost without exception, previous research has come to the opposite conclusions. It has become a virtual convention to acknowledge the deficiencies of the data which must be used in cross-national studies. It is possible,

Table 6. Association between Political and Economic Variables and Equality

Dependent Variables	Redistribution	Labor Force Earnings	Educational Opportunity	Share of Top 5%	Share of Top 20%
Intercept	38.615	29.075	-14.115	46.382	619.249
Demo- (a)	-.228	-.352	.226	-1.691	-.765
cracy (b)	-.198	-.200	.121	-.539	-.322
(c)	.42	.37	.18	4.50*	1.61
Socialism (a)	.091	-.069	-.049	-.189	-.164
(b)	.623	-.343	-.218	-.497	-.576
(c)	5.02*	1.73	.74	7.62*	10.13*
Economic (a)	.020	-.033	.147	-.126	-.132
Develop- (b)	.175	-.210	.811	-.363	-.502
ment (c)	.32	.45	8.31*	2.32	4.43*
Economic (a)	-.204	.607	.525	-1.538	-.776
Growth (b)	-.156	.290	.300	-.614	-.410
(c)	.17	.73	.56	8.01*	3.57*
\bar{R}^2	.26	.32	.55	.58	.58
F	2.00	2.15	4.51*	6.48*	6.45*

Notes: Although the \bar{R}^2 goes down compared to Table 8, unadjusted \bar{R}^2 increases.
See notes to Table 5.

therefore, that better data might change the relationship that has been reported here between social democracy and equality. However, there is evidence which suggests that if better data allowing more precise measurement could be found, the relationship might appear stronger rather than weaker. For example, the measure of redistribution via government budgets used here involves a simple distinction between direct (progressive) and indirect (regressive) taxes. Some direct taxes are, however, more progressive than others and some indirect taxes may, in fact, be quite progressive. Furthermore, it is, in general, the more social democratic countries that have a high ratio of direct to indirect taxes, and, *within* each category of taxes, adopt procedures that place a greater burden on higher income individuals.²² The small

number of countries which can be examined is obviously a problem. However, the growing interest in public policy and income distribution as well as cooperation between nations in such organizations as the OECD or the EEC is likely to generate measures of equality for those industrial societies which lack such information at present and to lead to a standardization of such measures between countries.

Any conclusions as to the significance of political factors in affecting the class system based on the type of data that must at present be used in cross-national comparisons, should be very tentative. In consequence, this study is not to be seen as definitive but, hopefully it will serve as a stimulus to other researchers to generate new measures of equality and political participation and to examine the relationships between them.²³

²² The U.N. Survey (1967:24) suggested that the progressiveness of income taxes was highest in Sweden, Norway, the UK and Netherlands and lowest in West Germany, Denmark, France and Italy. With regard to indirect taxes, Germany, France, Austria, Belgium, Italy and the Netherlands have a general turnover tax or value-added tax in which the incidence is more or less proportional to household expenditure. In Britain, Denmark and Sweden, indirect taxes are more concentrated on items such as drink, tobacco, automobiles, and petroleum products (U.N. Survey, 1967:8).

²³ For example, longitudinal analysis of individual countries should yield the same results as our cross-national analysis. The Swedish case supports this argument. "A systematic comparison of the pre-tax distribution of income in four European nations and the United States showed that in 1935 Sweden began with 'the most unequal distribution of any we have recorded and wound up in 1954 with one of the least unequal. . . . In 1932, a national crisis brought the Social Democrats and the Farmers' Party to power and cast the die for the accelerated development of the most celebrated welfare state of our time'" (Wilensky, 1975:73).

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THE ETHNIC VICE INDUSTRY, 1880-1944*

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Sociologists have explained the association of ethnic minorities and illegal enterprise in terms of structural blockages and opportunities, emphatically denying any ethnic contribution. A comparison of blacks and Chinese in the vice industry, 1880-1940, confirms the guiding role of American society which rewarded ethnics' participation in prostitution but restricted legal earning opportunities. Nonetheless, divergent demographic and cultural characteristics of Chinese and blacks differentially affected the internal organization of each group's vice industry as well as the process of industrial succession. This finding supports a view of illegal enterprise as a synthesis of illegal work that consumers want to buy and what disadvantaged ethnics have to offer. In general, socio-cultural characteristics of provider subgroups define the manner in which they respond to consumer demand for illegal products and services.

ILLEGAL ENTERPRISE

The major revenues of organized crime derive from the sale of illegal products and services. In this respect, organized crime is a business and has long been so acknowledged (Stone, 1926; Ogburn, 1937:10; Sellin, 1963; Haller, 1970:623; Lasswell, 1972:108; Ianni, 1974:15). The familiar term "organized crime" encompasses, however, some predatory crimes (hijacking, burglary, labor and business racketeering, etc.) as well as the demand-oriented sale of illegal products and services (Landesco, 1968:149-67; Seidman, 1938; MacMichael, 1970; Cressey, 1972:5, 8, 11, 26-7; Schelling, 1971). To designate the demand-oriented, illegal business only, Vold (1958:396-7) and Albin (1971:47) recommended the term "syndicated crime." However, they apply this term only to illegal enterprises

which encompass two or more retail outlets. "Syndicated crime" excludes small, independent, illegal businesses such as "single-action" numbers banking. Smith (1975:335) defines "illicit enterprise" as the "extension of legitimate market activities into areas normally proscribed . . . for the pursuit of profit and in response to latent illicit demand." This definition satisfactorily encompasses every form of demand-oriented, illegal business while excluding predatory crime. Unfortunately, the term "illicit" has the value connotation of impropriety. Therefore, Haller's (1970:623) term "illegal enterprise" is the most neutral designation for every type and size of business enterprise which supplies illegal products or services to willing consumers.

A large and often polemical literature has developed around the putative connection of ethnic groups and syndicated crime. This debate long revolved around the issue of whether syndicated crime is a foreign import or a product of American social conditions. Of these polarities, the

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import theory was the older. The police, Senate investigators, and vocal sectors of the American public have recurrently interpreted syndicated crime here as trans-plantation of secret organizations, cultural forms and even biological strains native to benighted parts of the world (California Department of Justice, 1973; Moore, 1974:viii, 40, 237). The import theory was a convenient vehicle for xenophobia and chauvinism, so racist, nativist and patriotic groups climbed aboard.

In reaction to the import theory, Landesco (1968; cf. Haller, 1968:xiii) and subsequent sociologists have minimized any ethnic contribution to syndicated crime in this country, claiming instead that American social conditions made criminals of oppressed slum dwellers. King (1969:183) calls this issue "the key question in the continuing debate over the nature of organized crime." The sharpest, most influential attack was that of Bell (1960:127, 138) who tagged the rackets "an American Way of Life" and lampooned the "myth of the Mafia." Tyler (1962:xiii) insisted that syndicated crime here is "a product and reflection of our national culture" rather than an alien importation. More recently, Albin (1971:154) concluded that "syndicated crime in the United States has developed from within the American social structure." Albin contrasts this conclusion with "the widely held misconception" that syndicated crime is "a product of foreign evildoers" seeking to "rob the American public of its moral virginity."

Two persuasive types of evidence have supported the long-prevailing sociological view that illegal enterprise is domestic rather than alien in origin. The first depends upon the observation that illegal enterprise satisfies the demand of the American public for illegal products and services (Albin, 1971:47) including gambling, usurious loans, pornography, prostitution, narcotics and, during the Prohibition era (1919-1933), liquor. Americans buy these illegal products and services because they want them, even at the high prices typically charged. Indeed, as Sutherland and Cressey (1970:272) observe, if crime syndicates abruptly disappeared, they would be "sorely missed"

because they perform services "for which there is a great public demand." Since the syndicates cannot exist apart from the public demand they satisfy, the American public's demand for illegal services and products is, in this view, the ultimate cause of the syndicate. Demand begets supply.

The theory of ethnic succession provided a second prop for the assertion that illegal enterprise was domestic in origin rather than imported. This usage paralleled the case for ethnic succession in juvenile delinquency which Shaw and McKay (1942) documented for residential neighborhoods in central Chicago. However, in Bell's (1960) seminal formulation, organized crime offered a "queer ladder of social mobility" along which Irish, Jews and Italians had moved in sequence. Cloward and Ohlin (1960:199-202) and Glazer and Moynihan (1970:210-1) called attention to tensions between established Italian racketeers and black and Puerto Rican newcomers, exclaiming over the retarded pace of ethnic succession. More recently, Ianni (1974) has concluded that black and Spanish-speaking minorities are replacing Italians in some sectors of organized crime. "Ethnic groups move in and out of organized crime," writes Ianni (1974:14) "and their time in control comes and goes." Advancing the same idea, Albin (1971:153; but cf. 1974:119-20) also emphasized the spectrum of ethnic and racial minorities in America who have engaged in syndicated crime at one time or another. In all cases, the ethnic diversity of criminals indicated that syndicated crime was a structurally-engendered response to disadvantage rather than a cultural proclivity. Merton (1957:192-4) and Cloward and Ohlin (1960:150-3, 194-202) advanced the view that blockage of legitimate avenues of social mobility was the structural condition which engendered syndicated crime as one deviant response. Although Smith (1975) has cast the response to illegal enterprise in terms of labeling, the anomie theory is still the orthodox sociological explanation of why ethnic and racial minorities have always been disproportionately involved in illegal enterprise in America (Humphries, 1973). The anomie theory is more complex than

a simple demand theory because it identifies American cultural values (wealth) and blocked opportunities as prerequisites of illegal enterprise. Nonetheless, the anomie theory also concludes that, given these prerequisites, demand does beget supply. Demographic, cultural or social organizational characteristics of minorities play no role. The anomie theory thus rebuts and excludes the import theory, its ideological predecessor and rival.

Although the import and functionalist theories developed as ideological rivals, no necessary repugnance separates them. That is, the public's demand for illegal commodities does not preclude the possibility that cultural, organizational or demographic characteristics of ethnic groups might also affect the timing, duration, price or scale of the supply engendered. In fact, a close reading of leading criminologists reveals a largely implicit convergence around the view that illegal enterprise is neither an import nor a structural function but rather, a synthesis of ethnic characteristics and American society. For example, Cressey (1969) has been cited recurrently as the principal criminological exponent of the view that the *Cosa Nostra* is a confederation of Italian crime families whose origins derive from the Sicilian Mafia. Nonetheless, Cressey (1969:72) had no trouble acknowledging that the Italian crime confederation "thrives because a large minority of citizens demand the illicit goods and services it has for sale."

A critic of Cressey's (1969) interpretation of the history and formal organization of Italo-American crime syndicates in the United States, Ianni (1972) also has developed a position which implicitly reconciles the functionalist and transplantation ideas. Like others, Ianni (1972:61) concluded that organized crime "is that part of the business system operative in the illicit segment of American life." At the same time, Ianni's close observation of an Italo-American crime family led him to emphasize the importance of familism and clanship in its business operations. Rejecting Cressey's view of a bureaucratic conspiracy, Ianni (1972:155) nonetheless concluded that "the origins of this familism are Italian and not American." This con-

clusion swings Ianni into conformity with those who implicitly believe that crime syndicates in the United States are not simple products or "reflections of our national culture" but complex fusions of ethnic heritages and American society.

Why should this convergence be surprising? Insofar as illegal enterprise is really a mode of business enterprise, one expects parallels between legal and illegal enterprise. Recent studies of minority business enterprise in legal industries reveal that ethnic styles of economic organization affect the manner and extent to which minority businessmen respond to American consumers. Comparing Chinese, Japanese, native blacks and West Indian blacks, Light (1972) found that Asian and West Indian business enterprise benefited from forms of cooperation and finance derived from overseas cultural heritages. Lacking these modes of association, Southern-born blacks in the North were less active in business enterprise than other nonwhites, although their disadvantage in the general labor market was no less. Bonacich (1975) and Petersen (1972) also advance the conclusion that Japanese-American solidarity contributed to their success, especially their business success. Wong (1974) found that Chinese-American grocers in South-Central Los Angeles, a black ghetto, were more likely than black grocers in the same neighborhood to be members of a business or trade association and to employ family members in the firm. Loewen (1971) emphasized the cultural bases of Chinese business enterprise in Mississippi. Bonacich et al. (1976) probed the overrepresentation of Koreans in the business population of Los Angeles. Caudill and De Vos (1956), Hsu (1972) and Kitano (1969) also conclude that the cultural background of Asians in the United States encouraged group social mobility in general, and business enterprise in particular. Several overseas studies reach parallel conclusions. Marris and Somerset (1971) compared and contrasted the entrepreneurial styles of Africans and Hindus in Kenya, concluding that the East African family encouraged a more individualistic approach to business enterprise than the Hindu. Stokes (1974) ob-

served that white Afrikaners long eschewed business enterprise, but turned to it abruptly when business was ideologically defined as supportive of nationalistic goals.

Weber (1958) pointed out that the religious beliefs of the Protestant sectarians channeled energies of the faithful into business enterprise. Sombart (1951) claimed that exclusion of Jews from medieval trades encouraged them to turn their rationalist tradition into capitalistic endeavors. Schumpeter (1962) developed his theory of capitalism around the cultural bases of entrepreneurship in the bourgeois family. American literature (McClelland, 1967; McClelland and Winter, 1969; Miller and Swanson, 1958) has been psychological in outlook, quite overlooking structural pressures. Withal, their conclusions support the view that characteristics of groups sometimes endow them for particular modes of success.

Reviewing this vast literature, Smelser (1976:126) concluded that "the market for entrepreneurial services has a demand side and a supply side." The demand side consists of "market opportunities," but socio-cultural characteristics of subgroups govern the supply side. Indeed, the sociologists must insist upon the identity of subgroups or consider whether, as Moore (1974:ix) claims, illegal enterprise be an economic rather than a sociological phenomenon. As matters stand, the outstanding critics of the cultural-structural synthesis are, in fact, economists. Recent tendencies to press the analogies between illegal and legal enterprise (Lasswell and McKenna, 1972:108) encouraged economists to make contributions to this growing literature. Their extensive writings have proceeded on the basis of the usual postulates of utilitarian individualism, producing significant illumination of the differences between illegal enterprise and syndicated crime, business and extortion, and retail and wholesale levels of illegal enterprise (Becker, 1968; Schelling 1967; 1971; Rubin, 1973). This useful literature has developed on the explicit presumption that cultural heritage and ethnic social organization are extraneous to the conduct of illegal enter-

prise. Yet, Anderson's (1974) economic study of a Cosa Nostra family returns to the recognition that its style of operation reflected Italian cultural origins. Thus, even some economists agree that socio-cultural characteristics of subgroups affect the manner in which they conduct illegal enterprises.

Unfortunately, the predominance of Italo-Americans in syndicated crime today has encouraged confusion between the details of Italo-American syndicates and the general problem of illegal enterprise. To escape this confusion, Ianni (1974:15-6) has recommended the comparison of Italian and non-Italian crime. This comparison is "essential to isolate the basic structure of organized crime in American society." Ianni's (1974) comparative research on blacks and Cubans has begun this effort. Helmer (1975) recently has completed a historical comparison of narcotics laws in relation to blacks, Chinese and Mexicans in the United States.

This paper approaches illegal enterprise by means of a comparison between Chinese and blacks in the vice industry between 1880 and 1944. Prostitution was the largest illegal industry of the late nineteenth and early twentieth centuries (Bell, 1960:165). Indeed, until the Mann Act of 1910 put a stop to it, this business involved the international and interstate traffic in women. The contrast of blacks and Chinese in this industry is a contrast of cultural heritages in a similar social setting. Since both Chinese and blacks are visibly nonwhite, and were disadvantaged on that account, the Chinese-black comparison holds constant the otherwise distracting color difference between white and nonwhite. In addition, the black-Chinese comparison offers a sharp test of the fusion hypothesis because, unlike Southern blacks, Chinese immigrants left a society in which secret criminal organizations already existed. These organizations were lodges of the Triad Society (Schlegel, 1866; Blythe, 1969; Elegant, 1976). Therefore, the black-Chinese comparison involves one group which did not (blacks) and one which did (Chinese) have a foreign tradition of illegal enterprise from which to draw in response to the

reward system of American urban society. If ethnic heritage ever makes a difference, black-Chinese comparison ought to discover it, and provide leads to the nature of the difference.

THE ETHNIC VICE INDUSTRY, 1880-1920

Sutherland and Cressey (1970:260) attribute the turn-of-the-century prominence and subsequent decline of prostitution to the normalization of the sex ratio in the U.S. population, and the liberalization of sexual mores. The decrease in the proportion of the population single, and increase in the proportion married should be added (Table 1). Liberalization of sexual mores adversely affected prostitution because promiscuous sexual contacts reduced consumer demand. The incremental normalization of the sex ratio (Table 2) adversely affected prostitution because the surplus of able-bodied men first dwindled, then disappeared. The increasing percentage of married men adversely affected prostitution because the married have less opportunity or motive to frequent prostitutes than the single. As long as it existed, the cadre of supernumerary bachelors represented a sector of the U.S. population in which demand for commercial sex contacts was pronounced (Polsky, 1969:21). The predominance of youthful bachelors was largely a result of the sojourning new immigrants from Southern and Eastern Europe. Foreign white immigrants commonly expected to return to their homeland after a brief, lucrative sojourn in the United States. Indeed, in the period between 1890-1910, approximately

40 percent of foreign whites admitted actually did repatriate (Petersen, 1968:268; Axelrod, 1972:32-49). Those who came to sojourn left their families behind in Europe. When World War I and then the Immigration Act of 1924 curtailed new immigration from Europe, settlers replaced sojourners in the foreign white population and, with natural increase, their numbers of males and females tended toward parity (Table 2).

These changes in the sex ratio of the United States population had special implications for blacks and Chinese in industrial cities. Disadvantaged in the general labor force, blacks and Chinese had a motive to find compensatory livelihoods in illegal industries. In the period of severest imbalance in the American sex ratio, roughly 1880-1924, a large class of white men existed who wanted to purchase sexual contacts (Chicago Vice Commission, 1911:114-6, 228). This demand encouraged blacks and Chinese to enter the vice industry. A white-patronized, ethnic-staffed vice industry thus developed in both communities for the identical reason. Naturally, blacks and Chinese continued to conduct a vice traffic for co-ethnics throughout this period. However, white demand permitted many more blacks and Chinese to find employment in the vice industry than would have been possible on the basis of co-ethnic patronage alone. White patronage probably doubled the volume of prostitution in Chinatowns and tripled it in Harlem and Bronzeville. Even so, these ethnic industries never claimed more than perhaps a third of total white vice traffic in this period.

Table 1. Marital Status of Male Population 15 Years of Age and over by Color for the United States, 1890-1940

	1950	1940	1930	1920	1910	1900	1890
Percentage of Unmarried							
Males 15 Years and over							
All Males	26.4	33.2	34.1	35.1	38.7	40.2	41.7
Black Males							
New York City	28.5		35.5	36.0			
New York State	29.6*	35.2	35.6	36.6	41.4	45.5	
The North			32.7	35.6	39.2	44.4	
United States	28.5*	33.5*	32.2	32.6	35.4	39.2	

* Nonwhite.

Source: U.S. Bureau of the Census, 1918: 247; 1935: 151; 1943a, pt. 1: 16, 17; 1943b: 60; 1953: 202, 205.

Table 2. Males per 100 Females by National Origin and Color for the United States, 1880-1970

	All Persons	Foreign- Born Whites	Chinese	Blacks
1970	94.8		110.6	92.2
1960	97.1	103.5	133.2	93.4
1950	98.7	103.8	161.1	94.3
1940	100.8	111.5	224.4	95.1
1930	102.6	115.7	296.4	97.0
1920	104.1	121.7	465.7	99.2
1910	106.2	129.2	925.7	98.9
1900	104.6	117.3	1385.0	98.6
1890	104.9	118.7	2678.9	99.5
1880	103.5	115.9	2106.8	97.8

Source: U.S. Bureau of the Census, 1960; A 51-8, A 59-70; 1972: T 18; 1935: 78.

The black vice industry and the Chinese vice industry were internally different. The black vice industry consisted of streetwalkers and pimps who settled quarrels with fights. The Chinese vice industry consisted of syndicated brothels which resolved severe business rivalries by gang wars, but adjudicated individual quarrels. The American public's appetite for illegal services does not explain these black-Chinese differences, although it does explain why both ethnic minorities entered the metropolitan vice industry at all. Demographic and cultural characteristics of blacks and of Chinese are needed to explain black-Chinese differences in internal organization of the vice industry.

As normalization of the American sex ratio set in (roughly, after 1914), the public's demand for vice services slackened, and ethnic-staffed vice industries found trade declining. Vice businesses then sought new employments to shore up the declining wages of sin. For the Chinese, the decline of white-patronized vice encouraged the restaurant/tourist industry in Chinatowns. This shift was largely complete by 1940. It represents a case of industrial succession in which an illegal industry disappeared and a legal one replaced it. Among blacks, the decline of white-patronized vice resorts stimulated a parallel search for alternative revenues. In the period between 1920-1944, this redeployment began to take the form of a rudimentary nightclub industry, but race riots and juvenile street crime aborted the

industrial succession after 1944. For both the Chinese and the blacks, therefore, a search for new opportunities arose in response to the declining profits of the white-oriented vice industry. But in the black case, industrial succession fizzled whereas in the Chinese case it succeeded. Cultural and demographic characteristics of the two minorities account for the black-Chinese difference in succession, but the general shift of public demand does not.

The Vice Industry, 1880-1924

Prior to the First World War, brisk public demand for prostitutes, gambling halls and drugs encouraged Chinese and blacks to enter the vice industry. In fact, American Chinatowns and urban black communities in this country were municipal vice centers throughout most of this period (Light, 1974; Frazier, 1949:645; Myrdal, 1944:332; Chicago Vice Commission, 1911:37-8; Hepler, 1972:71; Staples, 1973:81; Katzman, 1973:17; Reckless, 1925:173). Ethnic response to American demand supports the prevailing anomie theory of illegal enterprise because American society was encouraging blacks and Chinese to enter the illegal industry with financial rewards for their participation while their efforts to earn money in legal industries were rewarded much more modestly or not at all.

A massive preponderance of Chinese men existed before Chinatowns turned into municipal vice colonies (Light, 1974). Of course, this preponderance was encouraged and prolonged by the Immigration Act of 1882 and subsequent restrictive legislation. However, the drastic shortage of women in the Chinese population antedated the first restrictive legislation by three decades, thus proving that sex-ratio imbalances among Chinese were not the exclusive result of restrictive U.S. legislation (see Lyman, 1970:18). In 1890 there were 2,678.9 Chinese men for every 100 Chinese women in the United States (Table 2). Although this imbalance was much more extreme than in the case of the foreign-born whites, its origin was also a sojourning attitude among immigrant Chinese men who left their families at home.

Table 3. Males per 100 Females by Nativity and Color for Urban and Rural Areas, 1900-1940

	1940	1930	1920	1910	1900
Urban					
Foreign-Born White	106.8	111.0	115.9	118.9	106.3
Native White	94.5	96.0	96.9	97.3	97.0
Black	90.2*	91.3	95.4	90.8	87.8
Rural					
Foreign-Born White	130.4	134.0	141.8	161.1	143.8
Native White	107.5	107.6	106.7	107.2	106.6
Black	103.0*	101.7	101.2	102.1	102.1

* 1940 figure is for "Nonwhite."

Source: U.S. Bureau of the Census, 1918: 152; 1935: 79; 1943b: 10-4.

In general, a slight surplus of nubile women prevailed among urban blacks (Table 3). This surplus of females among urban blacks was a permanent feature of urban black populations and did not tend toward normalization. On the other hand, large cities had quite different demographic profiles, so the overall surplus of women among black urbanites concealed important city-by-city variation. New York City was an outstanding example of the general surplus of women among urban black populations (Scheiner, 1965:115; DuBois, 1903:2-3), but Chicago began the twentieth century with a surplus of black males and, over three decades, developed a surplus of women. The city-by-city complexity (Table 4) of black demography increases the difficulty of framing reliable generalizations. Coupled with unresolved historical ambiguities, this problem rendered it necessary to restrict archival research to New York City where a stronger than average surplus of black women admittedly existed. However, references to other cities are presented anyway because a surplus of black women was the master trend. Surplus-of-males cities (Detroit, Chicago) are exceptions which one might examine for confirmation of the effect of demography upon the organization of prostitution. In all cases, however, effective public demand for prostitutes was regional, rather than municipal in scope because transients were preponderant consumers of vice services (Chicago Vice Commission, 1911:114-6, 228). Relative to foreign-white or Chinese populations of their regions, even the most pronouncedly male, urban black populations (Table 4) were

generously endowed with needy females.

The contrasting sex ratios of Chinese (surplus of men) and urban blacks (surplus of women) had divergent effects upon the white-patronized, ethnic-staffed vice industries both developed. The shortage of women in Chinatowns encouraged brothel prostitution and eliminated streetwalking. All Chinese prostitutes were full-time professionals and worked in brothels. On the other hand, the abundance of women in most black enclaves encouraged streetwalkers rather than professionally-staffed brothels. Quite unlike Chinatowns, urban black enclaves conducted prostitution in three distinct and mutually competitive modes. The least organized but largest class of black prostitutes was independent streetwalkers. In nearly all cases, these were unemployed women who turned to prostitution until marriage or a regular job

Table 4a. White Males per 100 Females, by Nativity and Region in U.S., 1930

Region	Total	Native	Foreign-Born
United States	102.7	101.1	115.1
South	102.5	102.0	130.2
North	102.1	100.2	112.3

Source: U.S. Bureau of the Census, 1935: 79, 85.

Table 4b. Black Males per 100 Females in U.S. Cities with 10,000 or More Black Population, by Region, 1930, 1920 and 1910

Region	1930	1920	1910
South	86.8	90.5	86.6
North and West	97.8	103.5	96.4

Source: U.S. Bureau of the Census, 1935: 79, 85.

provided alternative support. Their number waxed and waned with the business cycle (Waterman, 1932:118). The next rung up the ladder of organization was the pimp system. Gun-toting black pimps maintained "strings" of 1-15 black and nonblack prostitutes who solicited business on streets and in saloons (Haller, 1970:220-1). In a few cases, pimps claimed a regular territory within which they exercised a monopoly, excluding all competition by threat of violence. However, most pimps were content to let prostitutes fend for themselves, insisting only upon a share of the gain in return for social-emotional support and a modicum of physical-legal protection (Slim, 1967; 1969). Black enclaves also contained syndicated brothels in which 3-100 prostitutes worked and resided. A brothel syndicate consisted of 2-15 brothels, each of which operated under a "franchise" from a common management (Reckless, 1933:70-1). As in Chinatowns, the franchising management provided personnel, corruption, and protection for resorts in its syndicate. Some syndicated brothels were expensive and elegant; others were cheap and dilapidated (Drake and Cayton, 1962:I, 55). In general, however, brothel prostitutes charged higher prices than streetwalking prostitutes and these, in turn, charged higher prices than the unemployed women of the ghetto.

In nearly all cases, syndicated brothels in black enclaves were under the management of white businessmen, although prostitutes were both black and nonblack (Landesco, 1968:27; Katzman, 1973:191).¹ Deliberate racial policy and high prices restricted these brothels to white customers. On the other hand, black men were the only pimps in black neighborhoods. Pimps had both black and nonblack prostitutes (Haller, 1972:221), and many grew wealthy in the business, but they rarely moved beyond pimping to brothel operation or from brothels to syndication. The

three levels of organization (brothels, pimps, streetwalkers) competed with one another for customers. Since white syndicates had political influence, they recurrently employed police to drive away price-cutting streetwalkers, but evicted blacks doggedly returned (Landesco, 1968:34, 42n; Reckless, 1933:69-70). The see-saw battle between streetwalking blacks and white-controlled brothels cast the pimps in the role of champions of their underdog race in competition with faceless white vice-lords (cf. Staples, 1973:92).

Black pimps relied upon their reputation for violent prowess to intimidate workers and rival pimps. Maintaining one's reputation required frequent dueling, and beatings or murders of prostitutes (Light, 1976). However, this ubiquitous violence never triggered wars between rival gangs in local, much less national federations. There is no record of inter-syndicate warfare for control of vice in black enclaves, although brothel syndicates made repeated, but ultimately futile efforts to eliminate price-cutting competitors. Here the situation in black vice districts forms a sharp contrast with the situation in Chinatowns. The black enclaves had high homicide rates, but no gang wars. Streetwalkers and their confederates frequently robbed their customers and, occasionally, robberies eventuated in killings (Asbury, 1940:127-8; Light, 1976). Chinatowns had low homicide rates, rampant gang wars, but no record of street robberies of visiting men. The implication is that the syndication of vice in Chinatowns prevented petty robberies and unregulated conflicts among individuals, but encouraged collective struggles for business advantage. On the other hand, the free market organization of vice in black enclaves permitted individualistic killings and petty crime, but eliminated gang warfare.

Although the surplus of women in the urban black population encouraged streetwalking rather than brothels, the presence of white-owned brothel syndicates in black enclaves prior to World War I proves that the abundance of needy black women did not altogether preclude brothel syndication. However, the strik-

¹ An exception was Mrs. Vina Fields, "a colored woman who has one of the largest houses in the city. During the [Chicago World's] Fair she had over 60 girls in her house, all colored, but all for white men" (Stead, 1894:247). Robert H. Spriggs, a Negro, also operated "several disreputable resorts" in New York's black ghetto in 1906 (Light, 1976).

ing shortage of black-owned brothel syndicates suggests that contrasting cultural heritages contributed to black-Chinese differences in organization of the ethnic vice industry. In the Chinese case, the vice industry consisted entirely of resorts owned by or affiliated with a Chinese secret society. These societies (*tongs*) traced their parentage in ritual to the Triad Society of South China and its Chee Kung Tong affiliate in San Francisco (Light, 1974). These historic origins have occasioned confusion because all Chinese tongs in this country trace an "affiliation" to the Triad Society through the Chee Kung Tong. However, most tongs were business, fraternal or political in character, and only a minority of "fighting tongs" licensed illegal businesses. Since complex alliances knit the nation's fighting tongs together, a purely local dispute could and often did precipitate a fight between affiliates and their allies in every U.S. Chinatown.

Lacking the Triad Society or any functional equivalent, black migrants of the lower class did not arrive with a culturally-provided framework around which to organize a syndicated vice industry. On the contrary, conditions of black life in the South (Jim Crow, free enterprise, police laxity) had encouraged the emergence of the lower-class cultural ideal of the "bad nigger," epitomized by the Stackolee legend (Johnson, 1941:99, 102), 103; Botkin, 1944:122-30). This legendary bully derived his fearsome reputation from violent prowess rather than from organizational affiliations. In the municipal vice industry, this cultural tradition evidently encouraged a free market organization around an internally competitive cadre of pimps. Additionally, the father-absent pattern in the black lower class placed primary earnings responsibilities upon black women heads of households with meager chances in industrial labor (Scheiner, 1965:57-8). Reinforcing the demographic abundance of these women, burdensome earning responsibilities naturally encouraged prostitution as supplementary or interim income. The resulting large class of part-time prostitutes constituted the major organizational barrier to professional monopoly in black-

controlled prostitution. If the Stackolee tradition offered scant support for monopolization, the ubiquity of inexpensive prostitutes additionally impeded it.

The intriguing problem of the demographic versus cultural origins of black-Chinese differences in the organization of the ethnic vice industry is inconsequential for the restricted purposes of this paper. Whether demographic or cultural in origin, or some combination of the two, the black-Chinese contrast in organization of the vice industry always reflected ethnic differences. Admittedly, American society encouraged both groups to sell vice services, but the demand of American buyers does not account for the contrasting organization of the supply. Therefore, a structural explanation of black and Chinese participation in the vice industry cannot fully account for the phenomena and requires supplementation by close review of contributory characteristics, demographic and socio-cultural, of sub-groups.

Industrial Succession, 1915-1940

The erosion of public demand for vice occasioned a search for alternative earning opportunities in Chinatowns as well as in urban black enclaves. However, cultural and demographic differences in black and Chinese populations also affected the outcome of this redirection, which was not simply a mechanical readjustment to market forces. For the Chinese, a restaurant-based tourist industry slowly supplanted the older vice industry (Light, 1974). Like its predecessor, the tourist industry depended upon patronage by non-Chinese. However, the tourist industry catered to women as well as to men, and even permitted the accommodation of children. In these respects, the developing tourist industry reflected the normalization of the sex ratio in American society, the growth of husband-wife families, and the increase in children. Although the transition from vice to tourism provided the occasion for a flurry of conflict at the turn of the century between opposing industrial interests within the Chinese communities, the basis for the conflict evaporated when fighting tongs

discovered the profitability of tourist enterprises. Fighting tongs actually opened restaurants on premises previously occupied by brothels and gambling halls.

In Northern black communities, the interwar years (1919-1939) also saw the development of an incipient tourist-entertainment industry catering to white outsiders (Reckless, 1925:278; Chicago Commission on Race Relations, 1922:202, 323-5; Light, 1976). These began as speakeasies in the Prohibition period and, like the brothels of the pre-Prohibition decades, were usually owned by white racketeers affiliated with outside crime syndicates (Waterman, 1932:128-9; McKay, 1940:200-1; Landesco, 1968:33). Speakeasies attracted white couples rather than single men; their entertainment consisted of suggestive revues and jazz music (Hobson, 1939: 214) rather than gambling or prostitution. Numerous restaurants purveying "soul" cuisine also opened to accommodate the gustatory curiosity of whites. These restaurants attracted favorable attention in the general news media (Light, 1976; James, 1931:248; Osofsky, 1963:32-3). Naturally, the interwar development of black communities as legitimate but risqué musical, entertainment, and restaurant attractions for affluent whites did not supplant the black neighborhoods' older attraction—prostitution (Waterman, 1932:128; Myrdal, 1944:977-8). This illicit business continued to attract white customers although the syndicated brothels disappeared, their white proprietors often shifting capital into cabarets and speakeasies. What is crucial in this interwar period, however, is the incipient separation of entertainment and vice, each industry attracting a different clientele (Guilds' Committee, 1939:257-63). The vice industry attracted unmarried men alone or in groups (Committee of Fourteen, 1928:22). They came to patronize vice resorts. The tourist industry attracted both men and women as couples, married as well as unmarried, and those who came were typically of middle or upper social status (Strong, 1940:100-1; Committee of Fourteen, 1930:21; Committee of Fourteen, 1927). The purpose of the tourists was to thrill to the depravity of nearby vice resorts, to

experience a "Negro" atmosphere, and to patronize jazz cabarets and "soul" restaurants (Osofsky, 1963:151; Reckless, 1925:88, 278). However contrived and superficial, the emergent tourist industry in Harlem, Chicago's South Side and most other metropolitan black enclaves authentically reflected a new public appreciation of black cultural forms, especially jazz music (Huggins, 1971:118). Indeed, jazz music was originally called "whorehouse" music in recognition of the setting in which its originators performed. The acceptance of jazz by respectable whites was a reflection of the underlying change in economic function of the black neighborhoods in the intervening half-century (Leonard, 1962:36, 50-1).

Yet, despite this substantial beginning, white tourism collapsed after 1944 and never recovered. Why did this industry collapse? One contributor was the Great Depression. The Depression had a deflationary impact upon the nightclub industry in general, and the repeal of Prohibition in 1933 was a specific shock for black belt speakeasies which could no longer rely upon police corruption to protect them from outside competition (Guilds' Committee, 1939:262). However, black belt clubs were among the most resilient of any in the nightclub industry, many reports indicating that they continued to operate "at full blast" throughout the Depression (Light, 1975; 1976).

Street robberies by juvenile criminals ("muggers")—not the Great Depression—finally killed the incipient tourist-entertainment industry in Harlem and other black neighborhoods. Of course, these robberies had always occurred in metropolitan vice districts (Scheiner, 1965:120; DuBois, 1899:238, 249, 252; Reckless, 1925:71). Riis (1892:221), Thrasher (1933:138) and Shaw (1930) all reported that "jackrolling" drunks and bawdy-house patrons was a typical apprenticeship in crime for adult offenders, and black men and boys were pronouncedly active in this form of robbery—an association which is hardly surprising in view of their residential proximity to centers of prostitution. Naturally, robberies and beatings reduced the volume of trade in a red-light district. In-

deed, a major recommendation of the more expensive, syndicated brothels was the customers' assurance of personal safety in them.

However, a tourist district required higher standards of street safety than a red-light district (Walker, 1933:252), and the proprietors of cabarets, dance palaces and other tourist attractions did, in fact, make strenuous efforts to protect tourists against thugs (Light, 1976). However, these efforts proved insufficient because of a sharp increase in the volume of juvenile delinquency in black neighborhoods beginning in the mid-1930s. The contemporary black and white press was acutely aware of this increase (Light, 1975, 1976) attributing it to Depression-spawned unemployment, and the constantly growing number and proportion of young blacks among the urban population (Table 5). Also, during the Second World War, when military personnel on liberty attempted to find "girls" in urban black neighborhoods, they came into conflict with local youths. The ensuing fights and race riots aggravated an already delicate situation, and wartime black neighborhoods acquired a reputation for danger which kept tourists at home (Lait and Mortimer, 1950:44, 46-7).

The mugging problem in black neighborhoods requires additional explanation because Chinatowns did not have this problem. It is a temptation to credit the Confucian family for suppressing juvenile delinquency, thus encouraging the tourist

industry in Chinatowns (President's Commission, 1967:74). However, this popular view assigns credit to the Chinese family when, as Lyman (1974:113) has observed, a safer explanation lies with the absence of families (no women) and, thus, of juveniles in Chinatown. In addition, Light and Wong (1975) have called attention to the growth in juvenile delinquency in American Chinatowns since the resumption of mass immigration following the Naturalization and Immigration Act of 1965. This abrupt increase raises additional doubt about the allegedly delinquency-resistant Confucian family.

Unlike Chinatowns, of course, urban black enclaves had always had an abundance of women of childbearing age. Largely because of rapid natural increase, the proportion of black juveniles to the urban juvenile population continuously increased, variously exceeding the general proportion of blacks between 1930 and 1950 (Table 5). This demographic process alone was enough to increase the incidence of juvenile crime in black neighborhoods. In contrast, the protracted shortage of women in the Chinese population retarded natural increase in population and, therewith, the delinquency problem in Chinatowns. Insofar as the father-absent cultural pattern in lower-class black families exacerbated delinquency and the Confucian family minimized it, black-Chinese cultural differences accentuated discrepancies in rates of delinquency which black-Chinese demo-

Table 5. Black Population by Age for the United States, Chicago and New York City, 1910-1950

	Percent Black				
	1950	1940	1930	1920	1910
Urban, U.S. ^a					
Age 10-19	11.78	9.17	7.14	6.60
All urban	10.41	8.60	7.53	6.55	
Chicago ^b					
Age 10-19	15.63	8.22	5.07	3.08	1.20
City	14.06	7.70	6.92	4.05	2.01
New York City ^c					
Age 10-19	10.29	6.18	3.38	1.85	1.26
City	9.49	6.40	4.72	2.71	1.92

^a U.S. Bureau of the Census, 1953:1-91; 1935: 93.

^b U.S. Bureau of the Census, 1952a: 80; 1943a, Pt. 2: 610; 1935: 127; 1922: 248; 1913: 439.

^c U.S. Bureau of the Census, 1952b: 195; 1943a, Pt. 3: 663; 1935: 133; 1922: 681; 1913: 141.

graphic contrasts engendered (McCord, 1968:91; Short, 1968:77). Withal, pronounced black-Chinese differences in rates of delinquency had a severe impact upon the developing tourist industry, for a tourist industry could not exist where delinquency was uncontrollable. Therefore, industrial succession (vice-tourism) took place in one case, but not in the other; and cultural and demographic characteristics of subgroups are required to explain the divergence.

CONCLUSION: DEMAND DOES NOT EXPLAIN SUPPLY

This historical comparison of blacks and Chinese confirms the claim that American society channelled disadvantaged minorities into illegal industries. But a close review also shows that no purely demand theory can account entirely for style, organization or succession in illegal enterprise because socio-cultural and demographic characteristics of minorities also affected these outcomes. Indeed, a shift in public demand (less vice, more tourism) had sharply contrasting consequences in Harlem and in Chinatowns because of contrasting demographic and sociocultural characteristics of inhabitants. So powerful were these demographic and sociocultural characteristics that a lucrative demand (tourism) had to abandon Harlem as well as other metropolitan black enclaves.

These anomalies point up a weakness of prevailing sociological analysis of illegal enterprise: the assumption that, given a demand schedule, disadvantage in the labor market is the only or, at least, the only substantial determinant of supply. In fact, even given a public demand and a high level of disadvantage, the quantity and quality of the supply of vice engendered still depended upon demographic, cultural and social organizational characteristics of blacks and Chinese. Indeed, some economic demands (tourism) produced no supply because of street crime engendered by adverse demographic and socio-cultural characteristics among potential suppliers.

Neither the old-fashioned import theory

nor the anomie theory alone can explain these results. This finding strengthens the hypothesis that illegal enterprise represents a fusion between what the American public wanted to buy (demand) and what, and how much disadvantaged subgroups (supply) have been willing or able to sell. True, providers can only sell what customers will buy. But, given a consumer demand for illegal products or services, the manner in which providers respond depends upon socio-cultural and demographic characteristics closely linked to ethnicity. Demand does not, therefore, explain supply because provider culture, social organization and demography intervene.

Although this conceptual adjustment is simple, its implications are uncharted. For example, sociologists acknowledge the importance of entrepreneurship in business so why not criminal entrepreneurship? The concept of criminal entrepreneurship enormously complicates the analysis of illegal enterprise because it implies that endowed groups can exploit niggling opportunities whereas others may flub Heaven-sent opportunities for illegal gain. Breaking the one-to-one correspondence of structural disadvantage and illegal enterprise, the concept of criminal entrepreneurship compels an analyst to inquire how groups are exploiting the illegal opportunities around them. Successful exploitation of illegal opportunities raises the cost of redirecting providers into legal occupations because only highly remunerated occupations can match or equal the livelihood illegal enterprise offers.

But what constitutes criminal entrepreneurship? Ordinary business skills contribute, but presumably do not exhaust the role. For example, Quakers are well endowed for business and have prospered in trade, but their religion precludes violence. Hence Quakers are unendowed for illegal enterprise even though highly endowed for business. In most cases, also, syndication in illegal enterprise requires the exclusion of small fry in the interest of maximum profit for the excluding in-group. Thus, when entrepreneurship is naively identified with scale of operation, the most entrepreneurial groups become

those who restrict participation the most—an obvious irony.

These ruminations merely illustrate the problems which the new direction implies. Admittedly, this horizon is stormy, but what is the alternative? Clinging to the time-honored theory of structural disadvantage is impossible because empirical objections are too easy to substantiate. A more mature view begins with the insistence that American society thrusts opportunities for lucrative deviance upon the disadvantaged, but acknowledges that socio-cultural and demographic characteristics determine the manner in which providers respond. The sociology of the problem then revolves around framing empirically-grounded generalizations to fit the diverse articulations between illegal industries and provider subgroups.

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THE EXPANSION OF NATIONAL EDUCATIONAL SYSTEMS: TESTS OF A POPULATION ECOLOGY MODEL *

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This paper investigates the expansion of enrollments in national systems of education during the 1950-1970 period, from the point of view of the population ecology of organizations. A simplified dynamic model of the growth of a population of educational organizations is estimated using various techniques for pooling time series of cross-sectional data. Results lend support to a meta-model of growth in which the dynamic parameters of the process are the dependent variables. Structural inertia, the effects of resources and available candidates are found to depend in a systematic way on structural properties of the educational organizations (cost intensity and the complexity of the division of labor) and characteristics of the national environment (abundance of resources). The findings indicate the usefulness of the ecological approach for the analysis of organizational change.

Most research on organization-environment relations concerns itself with the effects of environmental variations on well-established organizations. However, Stinchcombe (1965) has argued persuasively that environmental impacts are strongest at the time of founding. If so, the impacts of environments on organizations will be seen most clearly on new or emergent organizations. Following Stinchcombe, we propose to study environmental impacts by examining the societal conditions for organizing. The creation and expansion of formal organization

demands both the existence of certain social organizational features (e.g., market economy, bureaucratic state administration, etc.) and the mobilization of personnel and other resources. Resources can be mobilized only at the expense of other existing or potential forms of organization. That is, if certain material and human resources are to be devoted to a certain form of organized activity, they are not available to other forms of activity. Consequently, forms of organization compete for resources. Insofar as forms of organizations both compete for resources and differ in their demands on the environment, the theoretical strategy of population ecology can be employed to model the differential expansion of the various forms of organization (Hannan and Freeman, 1974; 1977).

We focus on the expansion of one par-

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ticular class of organization: educational organizations. Formal schooling has expanded enormously in every nation in the world over the past twenty-five years (Meyer et al., 1976). However, the rate of expansion varies greatly from nation to nation. We propose to use these variations to test an elementary population ecology model.

The expansion of education is most commonly explained by reference to the demands of modern industrial and bureaucratic production systems. Education viewed from the perspective of the individual consumer is an asset in competition for scarce rewards. Viewed from the perspective of the labor market or the firm, education is either a productive asset or a signal of some learning or productive ability (Machlup, 1970). Expansion of those forms of activity that demand skills in processing information will tend to increase the demand for education. Even if schooling does not affect productivity, its use as a signal for productive skills will tend to activate a competition for education that will lead to increasing levels of investment by individuals in education (Collins, 1971; Boudon, 1974). In either case, education is treated as a property of individuals that affects their relationship to the production system.

The expansion of education also has strategic value to political elites committed to changing the relationship of the individual to the state or polity. There is a good deal of evidence that education is used to define or redefine status categories in the population and thereby regulate or alter the position of those obtaining education in the polity (Rubinson, 1973; Ramirez, 1973; Meyer, 1971). Insofar as there are advantages to state controllers in mobilizing the populations politically and as long as expanding educational systems is a convenient or effective way to this end, education will be expanded. Although this argument is quite different from productivity and signaling arguments, it too treats education as a property of individuals that affects their relationship to the larger system.

We do not dispute either explanation. The trend toward greatly increased schooling undoubtedly reflects increased demands for knowledge, increased reliance on formal certification, status and class competition for jobs and other scarce rewards, and changes in the relationship of the state to its members. Yet we are unwilling to presume that any social system obtains any outcome it needs—in this case, increased schooling—simply by virtue of needing it. Nor do individuals automatically find schools or places in schools by virtue of desiring schooling. Schooling is an organized activity and the expansion of schooling represents an organizational achievement. Schooling expands in a system only when there is demand (from either the controllers of the system or the members) for additional schooling *and* an organizational capacity to incorporate more students. Previous research has emphasized the former at the expense of the latter. We propose that the expansion of formal educational organizations in any society be analyzed in specifically organizational terms.

Educational organizations differ on at least two variables of organizational significance. The first, *complexity*, particularly complexity of the division of labor, increases regularly from primary schools to universities. The second dimension is *unit-cost*, the amount of resources needed to process one student. Differences in costs for staff and equipment and the increasing burden of the indirect cost entailed by the nonproductivity of a more valuable fraction of the potential labor force means that unit-cost also increases from primary schools to universities.

Thus, we can define three *forms* of educational organizations: primary, secondary and tertiary, distinguished by their relative positions on the scales of complexity and unit-cost. If the environments of educational organizations are delimited by national boundaries, the collection of schools at a given level within a particular country constitutes a *population* of organizations. Finally, the three populations of organizations (primary, secondary and

tertiary schools), interacting within the same national environment, constitute a *community* of organizations.

Organizational forms, populations and communities of organizations constitute three basic ingredients of a population-ecological analysis of organizations, as explicated by Hannan and Freeman (1974; 1977). In this paper, we focus on the *aggregate size* of populations of organizations, measured as the national enrollment in a given educational level.

1. An Elementary Process Model of Educational Expansion

We discuss first the simple demographic and resources processes introduced above. This simple model then is embedded in a dynamic model that characterizes the manner in which enrollments grow. Finally, we employ organizational dimensions of the three forms of educational organizations to derive a series of inequalities among the parameters of the models applied to each level (section II).

a. Demographic and resources effects. To a large extent, the educational career of an individual takes the form of a rigid processing, with a fixed sequence of steps. From an aggregate perspective, therefore, the total student enrollment at one level will set an upper limit to the number of suitable candidates for the next level and, thus, on the enrollment at that level at some later time point. For primary schooling, the upper limit is given by the size of the primary school-age population of the country.

The supply of qualified candidates is only one of the factors that are likely to determine enrollment in a given educational level. As we noted above, all the ingredients of education represent a cost of some kind. The price has to be paid by the community in one way or another, so that we expect that the total amount of wealth in the environment will also constrain enrollment at all three levels: the wealthier the country, the larger enrollments it can afford.

The two arguments can be summarized as follows. The joint action of the avail-

ability of candidates, or demand, and resource availability determines a ceiling on the expansion of each level of education. From an ecological point of view, the ceiling may be thought of as the *carrying capacity* of a population of organizations in the national environment. The candidates available, and the amount of resources in the system correspond, then, to the *parameters of the niche* of an organizational form. We do not assume that the size of a population of organizations at a given point in time always coincides with its carrying capacity. The actual degree of expansion of a population depends on dynamic considerations that are further discussed below.

If we let E_t^* denote the carrying capacity of some organizational form and C_t and R_t , the cohort and resource conditions, respectively, the model proposes that:

$$E_t^* = f(C_t, R_t). \quad (1)$$

We do not have any *a priori* knowledge of the functional form of (1). So for the present, we adopt the simplest possible form, a linear relationship:¹

$$E_{it}^* = \alpha'_1 + \gamma' C_{it} + \delta' R_{it}. \quad (2)$$

The parameter α'_1 summarizes the cultural, political and infrastructural conditions that affect the carrying capacity and vary from nation to nation, but are approximately constant over time for each nation.

b. Dynamic considerations. The creation and expansion of formal organizations requires the mobilization of resources. Such an effort requires time. Consequently, organizations cannot react instantly to

¹ The linear approximation has the defect that it implies non-zero enrollments when either C or R are zero. However, as we are considering the behavior of these systems far away from zero, we do not think this is a serious problem. Alternatively, we can specify that the relation in (2) is a power function and estimate the model under a logarithmic transformation. Our experience is that a log-log specification gives qualitatively similar results. We retain the linear specification since it leads to the simplest dynamic structure.

variations in their carrying capacities. Moreover, the speed of adjustment to a new level of demand/affordability is likely to depend on the structure of the organization, on characteristics of the environment, and on the magnitude of the required change. We can capture the most obvious features of these processes in a simple dynamic model.

As before, let E_t^* denote the carrying capacity of the environment for some form of organization and let E_t denote the existing degree of expansion. Then the growth of a system that responds to demand will depend on a comparison of E_t^* and E_t . In particular, we argue that:

$$\Delta E = \beta' (E_t^* - E_{t-\Delta t}) \quad (3)$$

where $\Delta E = E_t - E_{t-\Delta t}$, and Δt represents some time interval to be determined.²

According to (3), not only does the system expand when $E_{t-\Delta t}$ falls below E_t^* and decline when $E_{t-\Delta t}$ rises above E_t^* , but the speed of response is proportional to the distance from the *carrying capacity*. The speed of response parameter β' describes the characteristics of the organizations and the texture of the environment. When the individual members of a population of organizations are relatively adaptable and the environment offers many resources and little resistance (competition by other forms of organizations), β' will be close to one: the actual size E_t of the population fully adjusts to its carrying capacity E_t^* during the time interval Δt .³

² Models similar to equation (3) are often written as:

$$\Delta E = \beta'' \Delta t (E_t^* - E_{t-\Delta t}),$$

where β'' represents the response of the system during a unit time interval $\Delta t = 1$ (see, for example, Hummon et al., 1975). In the present context, we are not attempting to estimate the underlying continuous process. We choose to define $\beta' = \beta'' \Delta t$ and to focus on the coefficient β' , which permits us to formulate the dynamic properties of the model (e.g., the conditions of stability) in a simpler way. As Samuelson (1970: Appendix B) points out, the time variable in a difference equation can always be redefined so as to make the time interval Δt equal to unity.

³ The assumption that enrollment adjusts itself

Organizational inertia or environmental resistance will decrease β' .⁴ Therefore, the *responsiveness* parameter β' may be quite informative about the ecology of a population of organizations.

Finally, we make the obvious substitution of (2) into (3) to yield the full model:

$$E_t = (1 - \beta') E_{t-\Delta t} + \beta' a' + \beta' \gamma' C_t + \beta' \delta' R_t \quad (4)$$

Equation (4), a finite difference equation, can be rewritten as:

$$E_t = a + \beta E_{t-\Delta t} + \gamma C_t + \delta R_t \quad (5)$$

where $\beta = 1 - \beta'$, $a = \beta' a'$, $\gamma = \beta' \gamma'$, and $\delta = \beta' \delta'$.

In equation (5), a , γ and δ are compounded coefficients that incorporate the effects of demand or resources on equilibrium enrollment multiplied by β' , the measure of organizational responsiveness to the exogenously determined carrying capacity. Keeping γ' and δ' constant, the to E_t^* places constraints on the acceptable range of values of the coefficient β' , which has to be between 0 and 2. If β' is negative, the size of the population of organizations and its equilibrium value diverge over time. A β' greater than 2 implies that E_t exhibits oscillations of increasing amplitude over time. In the stable case, $1 < \beta' < 2$ entails oscillations of decreasing amplitude until $E_t = E_t^*$, while $0 < \beta' < 1$ corresponds to a monotonic convergence of E_t to E_t^* (Bialock, 1969: ch. 5; May, 1973: Appendix III).

⁴ One major mechanism of environmental resistance is competition with other forms of organizations. Competition between forms of organizations occurs when their respective carrying capacities depend on the same environmental variables, so that the expansion of one form decreases the carrying capacity, or maximum size, of others. Competition depends, therefore, on the relevant niche parameters. For example, the three levels of education do not compete for candidates, since their prospective students are likely to belong to very different groups, so an increase in enrollment at one level does not threaten the expansion of other levels. By contrast, they are likely to compete for *resources* (i.e., money), since the allocation of environmental resources for the expansion of one level of education are unavailable for other levels. In our simplified model of educational expansion, the net effect of competition is forced into the dynamic coefficient β , which is treated here as a purely phenomenological, descriptive parameter. We argue that competition decreases the speed of response of the populations involved.

higher β' , the higher the net effects of C_t and R_t on enrollment. Since we are mostly concerned in what follows with deriving and testing hypotheses on the value of γ and δ , which represent the net effects of demand and resources on *enrollment at time t*, rather than with γ' and δ' , the effects of these two variables on the carrying capacity E^* , we choose to reserve the expressions *cohort* and *resource effects* to refer to γ and δ , respectively. The coefficient β will be called the *inertia* parameter since it is a decreasing function of the responsiveness coefficient β' and indicates, therefore, the degree of organizational resistance to change. The larger β , the slower the adjustment of an educational level to its carrying capacity.

c. Empirical specification of the model. Equation (5) has the familiar form of a linear equation with lagged dependent variable. We require data at two or more time periods to estimate the model. In fact, we have observations on national enrollments at five-year intervals from 1950–1970 (UNESCO, 1971). We measure resources as gross national product in 1964 U.S. dollars (I.B.R.D., 1971). Following the earlier discussion, the size of the cohort, C_t , for each level of education is measured as the enrollment in the next lower level of education five years earlier. For primary schooling, the relevant cohort is the school-age population at that time. For a number of reasons, the measurement of cohorts for secondary and tertiary education are not ideal. The typical time spent in a level of education is usually different from five years, students may reenter the educational system after some interruption, etc. To attempt to correct for any resulting distortions, we add total national population to the models for secondary and tertiary education and primary enrollments to the model for tertiary.

The three empirical specifications of the general model of educational growth may be represented as follows:

$$\begin{aligned} \text{PRI}_t &= a_p + \beta_p \text{PRI}_{t-5} + \gamma_p \text{PRIPOP}_t + \delta_p \\ &\quad \text{GNP}_{t-5} + \epsilon_{pt} \end{aligned} \quad (6.1)$$

$$\begin{aligned} \text{SEC}_t &= a_s + \beta_s \text{SEC}_{t-5} + \gamma_s \text{PRI}_{t-5} + \delta_s \\ &\quad \text{GNP}_{t-5} + \xi_s \text{POP}_t + \epsilon_{st} \end{aligned} \quad (6.2)$$

$$\begin{aligned} \text{TER}_t &= a_T + \beta_T \text{TER}_{t-5} + \gamma_T \text{SEC}_{t-5} + \delta_T \\ &\quad \text{GNP}_{t-5} + \xi_T \text{POP}_t + \zeta_T \text{PRI}_t + \epsilon_{Tt} \end{aligned} \quad (6.3)$$

where PRI_t , SEC_t and TER_t denote the enrollments at time t in primary, secondary and tertiary education, respectively, POP_t (Taylor and Hudson, 1971) denotes national population at time t and PRIPOP_t denotes the size of the primary school-aged population at time t .

In equation (6), ϵ_p , ϵ_s and ϵ_T represent the combined effect on enrollment of variables omitted from the model. Alternative specifications of ϵ and their implications for estimation are discussed in section IV. An interesting theoretical feature of (6) is that the three equations contain *comparable* parameters of organizational significance: the inertia, cohort and resource coefficients (β , γ and δ , respectively). Since the model is defined on three populations of organizations that are assumed to differ systematically on the scales of complexity and unit-cost, we can compare these coefficients across populations and generate hypotheses relating their respective values with the assumed positions of the populations with respect to these structural criteria. In other words, from the organizational point of view, *the coefficients of the general dynamic model are the variables of substantive interest*. The next section is devoted to an elaboration of this theme and to the derivation of specific hypotheses.

II. Hypotheses

The general model described in section I reduces the growth of national enrollments to three main mechanisms. We first attempt to elucidate how the parameters that characterize these mechanisms would be expected to vary across populations of organizations that differ with respect to their degree of complexity and the amount of resources they require.

a. The inertia parameter. In each equation in (6), β is an *inverse* measure of the speed of adjustment to the carrying capacity. Therefore, β characterizes the degree of structural inertia of a population. A larger β means that population size is more dependent on the past, that it exhibits

a higher degree of rigidity with respect to exogenously induced pressures for a change in scale.

We argue that both a high degree of complexity and a high cost per unit lower the speed of adjustment of organizations and, therefore, increase the value of β . A highly complex and differentiated structure entails the need for a wide variety of specialized skills, roles and elements of material infrastructure. Not only does the organization compete more with others to mobilize choice ingredients in shorter supply and, thus, is less likely to fulfill its needs quickly, but the efficient coordination of the interdependent whole following growth is also more problematical. Conversely, once the organizational structure is created, the specialized components (people or things) are difficult to reallocate for some alternative use, and one would expect a high degree of resistance of the organization to decline as well as to growth.⁵ To take specific examples, such prerequisites of the educational process as libraries, schedules, laboratories, classrooms and adequate teaching and administrative bodies cannot be created and put together without a series of complex decisions and adjustments. The more differentiated the organization, the more complex the series of decisions involved in growth and the longer the adjustment time.

On the other hand, a high cost per unit limits the rate of organizational expansion in an obvious way. For the same level of success of an organization competing with others for environmental resources, a low-cost organization will expand more (in terms of units) than a high-cost one. To summarize both arguments, the speed of adjustment of the size of an organization is expected to be higher when the cost of an additional unit is low and when the organization can expand by the mere addition of relatively unspecialized segments

of human and material infrastructure. Thus we hypothesize: $\beta_P < \beta_S < \beta_T$.

b. The cohort effect. According to the general model embodied in equation (5), the coefficient γ associated with C_t is the product of γ' , the effect of the cohort of qualified candidates on E^* and β' , the coefficient of organizational responsiveness. If one assumes for a moment that γ' is the same for all three levels, it follows from the preceding discussion that the composite coefficient γ decreases in magnitude from PRI to TER. This is because the responsiveness coefficient β' is a decreasing function of the inertia parameter β , so that the discussion of paragraph (a) implies: $\beta'_P > \beta'_S > \beta'_T$.

It seems likely, however, that γ' itself is dependent on structural characteristics of the organization. Thompson (1967) argues that, in their efforts to approach bounded rationality, organizations attempt to seal off their core technology from environmental fluctuations. This protective function can be carried out by specialized boundary-spanning components through the buffering, leveling, forecasting and rationing of environmental demands. Thompson's argument can be extended, at the *population* level,⁶ by assuming that the protection requirements of organizations are some increasing function of the complexity and intensity (cost) of the core

⁵ For all the nations we are studying, enrollments increased over the period. Therefore, we do not address the likelihood that decline processes differ from growth processes for these forms of organizations (Freeman and Hannan, 1975).

⁶ This extension of Thompson's argument is one instance of an hypothesis that is more readily justified at the population level rather than for individual organizations. Considered separately, individual members of a population of organizations have available a variety of strategies, ranging from extreme exclusiveness to minimal boundary-spanning intensity. Whatever strategy is chosen presumably will depend in part on mechanisms of intrapopulation competition between organizations for membership and resources, so that one would expect the individual coefficients γ' to vary widely within the population. At the aggregate level of a population, by contrast, general constraints on the functioning of a given organizational form are likely to appear in a sharper light. From a deeper point of view, the issue is intimately connected with the problem of collective rationality (Hannan and Freeman, 1977): there is no guarantee that a course of action that is adaptive for a single organization will be optimal when many competing organizations adopt a similar strategy.

technology: *the more complex the core technology of an organization, and the more efforts and resources invested in each unit of input, the more selective the boundary-spanning components in charge of input.* In the educational context, such boundary-spanning activities manifest themselves in various guises: admission committees, entrance examinations, massive elimination after a short trial period within the organization, etc. In view of equation (5), it means that the carrying capacity of more complex and resource-intensive organizations will be less dependent on cohort size, so that $\gamma'_P > \gamma'_S > \gamma'_T$. Putting together the expected behaviors of the two component parts of the coefficient γ , $\beta'_P > \beta'_S > \beta'_T$ and $\gamma'_P > \gamma'_S > \gamma'_T$, with $\gamma = \beta' \gamma'$, we hypothesize that the estimated γ will decrease from PRI to TER: $\gamma_P > \gamma_S > \gamma_T$.

c. *The effect of resources.* The effect of environmental wealth is less easy to cast in terms of straightforward inequalities. δ , the coefficient to be estimated, is the product of the responsiveness, β' , and of δ' , the effect of resources on E^* . If one reduces equation (2) to its simplest form $E_t^* = \delta R_t$, the meaning of δ' can be expressed as: the average increase in the carrying capacity for enrollments due to an increase of one unit of resources in the system. However, we expect both the strength of the resource effect and the unit-cost to increase from PRI to TER. With the present model we cannot distinguish the two effects.

d. *Community of organizations and environmental resources.* Our analysis to this point involves comparison of three *populations* of organizations that differ on structural criteria. However, the three populations of organizations within a bounded system are clearly in a relationship of *sequential dependence* (Thompson, 1967): each level of education produces the input for the next. To that extent, the three populations interacting within the national boundaries constitute a *community* of organizations. We proceed to investigate the impact on the community of organizations of a major environmental constraint: the relative abundance of re-

sources. To this end, we introduce resources in a nonadditive way by estimating the equations (6) separately for poor and rich countries.⁷ This procedure leads to a series of additional hypotheses that take the form of pairwise comparisons of coefficients.

First, resource levels in the environment are likely to improve the capability of the three populations composing the community to adjust quickly to their respective carrying capacities: more resources in the system mean less competition between organizations for essential ingredients. Thus, for each level of education, the inertia coefficient β will be lower in rich countries (R) than in poor ones (P):

$$\beta_P^R < \beta_P^P; \beta_S^R < \beta_S^P; \beta_T^R < \beta_T^P.$$

Second, in an abundant environment, input fluctuations are likely to constitute a lesser threat for the core technology of an organization, since a temporary disequilibrium is more easily corrected by a reallocation of resources. Within a level of education, γ' is expected to be larger in rich countries. Since in addition the organizational responsiveness β' is also increased by wealth, the composite coefficient γ is expected to be larger in rich countries than in poor ones:

$$\gamma_P^R < \gamma_P^P; \gamma_S^R < \gamma_S^P; \gamma_T^R < \gamma_T^P.$$

This hypothesis can be readily translated into a property of the educational community as a whole. Since γ represents the effect of enrollment at one level on the size of the next, it provides a measure of the degree of sequential interdependence between the component parts of the national system of education. We argue that environmental wealth increases the interdependence of the three levels within the educational community.

Finally, analysis of the resource effect is somewhat more tractable within the

⁷ Nations are assigned to the poor or rich sample according to their position below or above the median GNP per capita measured in 1950, the earliest time point in the data. We exclude from the sample countries that were not independent in 1950.

community context. There are two reasons for expecting that δ' will be larger in poor nations. The first is a marginal return argument. When few resources are available, the major constraints on organizational growth are resource constraints. Thus, an increase in resources should have maximum impact in this situation. In richer environments, the bulk of causality presumably shifts to other potentially limiting factors. Second, unlike the case in the population level analysis, the unit-cost effect is in the same direction: a dollar of expenditures can purchase more educational services in a poor nation.

Notice that the coefficients δ , that are products of resource and responsiveness effects, are still indeterminate in sign. We argue that the resource effect is larger in poor nations but that responsiveness is higher in rich nations. Thus we must compute δ' explicitly.

III. Methodology

a. Pooled cross-section and time series analysis. Rather than estimate (6) separately for each of the four 5-year lags, we pool all of the data into a single model. This design, discussed extensively by Nerlove (1971) and Hannan and Young (1977), permits us to deal satisfactorily with an estimation problem that plagues almost all sociological panel analyses: autocorrelation of disturbances. In motivating (2), we pointed to the relatively constant features of national cultural, social and political organization that affect educational expansion. Since these features are not measured here, they are forced into the disturbances in (6). Since they presumably operate in every period, the disturbances are correlated over time and with the lagged dependent variables. Consequently, ordinary least-squares estimators applied to (6) are biased and inconsistent. If the autocorrelation of disturbances is due only to the constant nation-specific effects (i.e., $\epsilon_{it} = \mu_{it} + v_{it}$, where the v_{it} are well-behaved random disturbances), two strategies of estimation applied to the pooled model yield consistent and asymptotically efficient estimators. The first, LSDV (least-squares with dummy vari-

ables) treats the μ_i as fixed effects and calculates pooled within-nation regressions. The second, MGLS (modified generalized least-squares), treats the μ_i as random effects. Their variance contributions are to be estimated under the assumption that the μ_i have constant variance. When the constant variance assumption holds, MGLS has better small sample properties (Nerlove, 1971; Hannan and Young, 1977). However, since we are uncertain that the constant variance assumption is appropriate to these data, we report results from both approaches.

b. Heteroscedasticity. When the units of observation differ greatly in size, the random portion of the disturbances are unlikely to have constant variances (see the discussion in Freeman and Hannan, 1975). In fact, preliminary analysis of these data indicated that the absolute values of disturbances increased sharply with size. Therefore, we introduce a generalized least-squares correction, namely, weighted least-squares. However, implementing weighting in the pooled model is complicated. We have shifted from the Nerlove-type estimators most usually used to those proposed by Henderson, Sr. (1952; 1963; see also Henderson, Jr., 1971) which permit us to construct two estimators that simultaneously control for both nation-specific factors and heteroscedasticity.⁸ They parallel the two approaches introduced above. We label the fixed effects estimator WLSDV (weighted least-squares with dummy variables) and the random effects estimator WGLS (weighted generalized least-squares). Both are consistent and asymptotically efficient under the stated assumptions. A justification for this procedure and an algorithm are presented in Nielsen (1974).

IV. Results

We begin by presenting estimates of (6) for the population and community level hypotheses. Then we calculate estimates of the underlying dynamic model.

⁸ The weighting factors are PRIPOP_i in the PRI_i equation and POP_i in equations for SEC_i and TER_i.

a. Cross-population analysis. We start by investigating the hypothesized ordering of inertia and cohort effects in (6) across the three populations of organizations. Table 1 gives the relevant results for the two estimators and two samples (rich and poor nations). Consider first the inertia effect. For both estimators and both samples, we find the hypothesized effect: the estimated inertia parameter increases from primary to secondary to tertiary. For example, the WLSDV estimates are .169, .694 and .872 for primary, secondary and tertiary in rich nations and .552, .747 and .935 in poor ones. This result lends support to our theoretical expectation: at the population level, more complex organizations, with a higher unit-cost, adjust more slowly to their carrying capacity.

The hypothesis concerning the cohort parameter is that it should decrease from primary to tertiary. This hypothesis also fits the data perfectly. Therefore, more complex and costly educational organizations tend to incorporate a smaller fraction of the supply of qualified candidates.

We argued that the dimensionality of the linear resource parameter made its meaning ambiguous. Nevertheless, it is interesting to examine the empirical results to see if the coefficients exhibit some systematic pattern across educational levels. The resource coefficient for SEC enrollments is the highest for all four sample-estimator combinations. This seems to suggest that the two effects in opposite directions of complexity and cost on the resource parameter indeed take place in reality. The low coefficient of primary would then be explained by a low "dependence" on resources, the low tertiary coefficient by a high "cost" effect in the opposite direction while the higher resource parameter of secondary would result from a combination of more dependence on resources not yet offset by a parallel increase in cost. This interpretation is, of course, ad hoc, but the empirical results strongly suggest the existence of a curvilinear relationship.

b. Cross-community hypotheses. To test the effect of the abundance of the environment on the inertia, cohort and resource parameters, we compare estimates for rich

and poor countries. For example, by the WLSDV method the inertia parameter of primary is estimated as .169 for rich countries and .552 for poor ones; the inertia coefficients are .694 and .747 for secondary and .872 and .935 for tertiary. The same pattern holds for the WGLS estimates. Thus, our first community hypothesis is confirmed: the inertia parameter tends to be lower in wealthier environments.

The cohort coefficients are analyzed by similar techniques. For both estimators, the postulated inequality holds: for each educational level the cohort effect is higher in rich environments than in poor ones. That is, in wealthier countries the educational system is more responsive to the demographic pressure of the pool of qualified candidates.

The last set of hypotheses concerns the effect of the abundance of the environment on the resource parameter of the educational system. Recall that the resource effect is defined on δ , the effect of R_t on E^* , and that δ is a composite coefficient, the product of parameters δ' and β' that are expected to be found in reverse orderings when compared across environments. The situation, however, can be approached through the following *a fortiori* argument: if δ is larger for poor countries, then δ' must be larger also, since the smaller responsiveness coefficient β' would tend to diminish the value of δ and obliterate the difference between poor and rich countries. Keeping this possibility in mind, we compare the estimates of δ across the two samples. For the first time, we encounter substantial disagreement between estimators. For WLSDV, the resource effect is larger for poor nations for each level of education. However, exactly the reverse pattern holds with the WGLS estimates. We have not been able to determine why this difference arises. It may have something to do with small samples properties of the WGLS method in that unweighted GLS estimates (not reported here) agree with those of WLSDV. Clearly, this matter deserves deeper study.

c. The determination of carrying capacities. We have managed, so far, to

Table 1. Estimates of Coefficients of Model (6) *

Dependent Variable	Sample	Method	Independent Variables							R ²	N
			PRI _{t-5}	SEC _{t-5}	TER _{t-5}	GNP _{t-5}	POP _t	PRIOPOP _{t-5}	CONSTANT		
PRIMARY Enrollment:	Rich Nations	WLSDV	.169 (.052)			-.106 (.025)		1.073 (.063)	0	.893	28
		WGLS	.473 (.054)			.015 (.022)		.598 (.051)	-186.96 (183.01)	.987	28
	Poor Nations	WLSDV	.552 (.087)			.145 (.177)		.584 (.082)	0	.982	28
		WGLS	1.015 (.046)			-.275 (.141)		.165 (.028)	92.17 (181.40)	.982	28
SECONDARY Enrollment:	Rich Nations	WLSDV	.275 (.062)	.694 (.084)		.190 (.049)	-.385 (.017)		0	.877	29
		WGLS	.077 (.031)	.962 (.052)		.054 (.019)	-.015 (.041)		15.82 (33.26)	.977	29
	Poor Nations	WLSDV	.106 (.047)	.747 (.110)		.235 (.093)	.042 (.085)		0	.855	29
		WGLS	.036 (.020)	1.119 (.082)		.029 (.062)	.023 (.015)		-23.57 (48.76)	.948	29
TERTIARY Enrollment:	Rich Nations	WLSDV	.018 (.015)	.035 (.019)	.872 (.112)	.035 (.014)	-.016 (.045)		0	.899	29
		WGLS	.017 (.008)	.013 (.011)	1.160 (.068)	.013 (.005)	-.023 (.010)		-.96 (4.72)	.972	29
	Poor Nations	WLSDV	-.023 (.009)	.002 (.024)	.935 (.024)	.040 (.094)	.068 (.019)	.017	0	.832	29
		WGLS	.005 (.004)	.012 (.008)	1.165 (.061)	.005 (.011)	-.000 (.003)		-4.42 (6.57)	.958	29

* Standard errors in parentheses.

verify most of the propositions of section II without explicitly computing the parameters of equation (2) for the carrying capacity: γ' and δ' . These coefficients, however, can be easily recovered from equation (5): β' is first computed as $1-\beta$, and then γ' and δ' as γ/β' and δ/β' , respectively. Given our results, these calculations are meaningful only for the WLSDV estimates for reasons we discuss below. The calculations are reported in Table 2. Across the levels of education, both β' and γ' exhibit the expected decrease from PRI to TER and the behavior of δ' remains curvilinear with the highest resource effect in the SEC equations. The rich-poor comparisons are also all in the expected direction (the one exception is γ' for PRI, where the effect for poor nations is slightly larger). More important, we finally get a clean test of the community-level resource hypothesis. As we had predicted, the resource effect for each level of education is larger for poor nations.

We could not make similar calculations from the WGLS estimates because the estimated β exceed unity in four of the six equations. This implies that the responsiveness parameter is negative. A negative responsiveness parameter implies substantively meaningless dynamics. This problem

likely stems from any combination of three complications: (1) the functional form of the dynamics may not be appropriate, especially for more complex organizations (the problem seems worst for tertiary education); (2) the hypothesis that the transitory portion of the disturbances of (6) are not autocorrelated may be wrong; (3) the constant portion of the disturbances in (6) may not have constant variance. Each of these issues demands further study. It is particularly important that the dynamic structure be clarified further. In the meantime, our results must be considered a first approximation.

V. Discussion and Conclusion

This paper focuses on the aggregate size of populations of organizations. This point of view enabled us to propose a simple dynamic model of educational growth, independent from individual variations in organizational strategies due to mechanisms of intra-population competition (see footnote 6). Since the model could be specified for three populations of organizations (the primary, secondary and tertiary levels) within two types of environments (rich and poor), we could meaningfully compare general mechanisms of change (inertia, cohort effect and dependence on resources) across populations and environments.

The results confirm, on the whole, our population ecology model of organizational growth. The maximum size of a population of organizations within a bounded environment is determined by the *carrying capacity* of the environment for a particular organizational form. In our simplified model of educational expansion, we specified the carrying capacity as a function of two *niche parameters*: the supply of qualified candidates and the amount of resources in the environment. An immediate goal for further research consists in identifying other parameters of the niche of an organizational form (e.g., political and cultural factors). A population of organizations, however, cannot react instantly to an expansion or contraction of the carrying capacity of the environment

Table 2. Estimates of Parameters of the Dynamic Model (WLSDV)*

		Rich Nations	Poor Nations
Responsiveness:			
$\hat{\beta}' (=1-\hat{\beta})$	PRI	.831	.448
	SEC	.306	.253
	TER	.128	.065
Effect of Cohort on Equilibrium:			
$\hat{\gamma}' (= \hat{\gamma}/\hat{\beta})$	PRI	1.291	1.304
	SEC	.899	.419
	TER	.273	.031
Effect of Resources on Equilibrium:			
$\hat{\delta}' (= \hat{\delta}/\hat{\beta})$	PRI	-.128	.324
	SEC	.621	.929
	TER	.273	.615

* Calculated from estimates in Table 1. See text for explanation.

because of both the inherent structural inertia of individual organizations and competition between populations for resources and membership. In our model, structural inertia and the effect of competition are lumped together in the inertia parameter β . We showed that this coefficient, and the dynamic mechanisms it represents, could be meaningfully compared across populations and environments. Further work should attempt to analyze the process of growth by modeling explicitly the mechanisms of competition.

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SCIENTIFIC PRODUCTIVITY AND THE REWARD STRUCTURE OF SCIENCE*

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Regression analyses of longitudinal data for a probability sample of chemists provide estimates of the causal links between chemists' predoctoral training, early productivity, recognition and organizational context and productivity at the end of the first postdoctoral decade. After identifying certain methodological problems involved in using Science Citation Index citation counts to assess the effects of collegial recognition on later productivity, I report tests of several hypotheses proposed to account for scientists' conformity to productivity norms. Apart from the calibre of the Ph.D. department, measures of socialization have no direct effect on decade productivity. Early productivity and collegial recognition do contribute to decade productivity, but the strength of their effects varies by the research orientation of the first employer: early productivity is more important for those employed in universities, whereas collegial recognition is particularly important for chemists in contexts that do not stress scholarly publication.

Scientific research is fraught with difficulties which are not characteristic of most professional work. Scientists must choose research questions that are solvable yet nontrivial, must apply disciplined and creative efforts to intractable problems, must convince others that their results are valid and valuable and must tolerate high levels of competition that may deprive them of the ultimate reward of recognition from the scientific community (Merton, 1957; Hagstrom, 1974). Furthermore, scientists must work without the structural supports for role performance that many other professionals take for granted. For example, scientists' services are not subject to the high public demand that structures the work of free professionals; they must schedule their own work and that of their assistants. Rarely subject to regular supervision, sci-

entists also lack the short-run incentives that rewards for daily performance provide. While they are powerful sanctions in the long run, promotion, tenure and salary raises are probably not especially effective in maintaining day-to-day conformity to productivity norms. Moreover, many scientists have nonresearch obligations (teaching, administration) that do have deadlines, are subject to positive and negative sanctions and, thus, can divert them from their less structured research work.¹

Several social-control mechanisms ensure that some scientists undertake the uncertain task of original research. These include selective recruitment of highly-talented individuals to scientific careers (Harmon, 1965), intensive research training (Ben-David, 1971:141-2), socialization to norms emphasizing research and publication (Hagstrom, 1965:10) and rewards for productivity (Merton, 1957; Hagstrom, 1965). Some observers of science have stressed the intrinsic psychological rewards of scientific achievement, but the former are probably

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¹ Stinchcombe (1966:30) suggested strategies for overcoming these obstacles, but their general effectiveness is still unproved (Hargens, 1977).

not sufficient for most scientists without the professional rewards that usually accompany them. Extrinsic rewards—formal and informal recognition (Hagstrom, 1965); material rewards, such as positions and research resources; and, less frequently, prizes and honors (Zuckerman, 1970:237)—appear to be the major source of social control in science. Yet, in spite of these rewards and their effectiveness for some, many scientists do no original research after leaving graduate school and rarely or never publish (Price, 1963:47). Apparently, the scientific reward-structure, while perhaps sufficient to maintain scientific progress (Cole and Cole, 1972), is not particularly effective in maintaining conformity to professional norms at the individual level. Several hypotheses (reviewed below) have been proposed to account for this variation in scientists' productivity. This paper reports tests of some of them, using data for scientists from a single discipline, chemistry.

DETERMINANTS OF SCIENTIFIC PRODUCTIVITY

Until recently, most studies of scientific productivity were done by psychologists interested in either creativity (Eiduson, 1962) or organizational characteristics that maximize productivity (Taylor and Barron, 1963; Pelz and Andrews, 1966). The former studies, which emphasized the role of personal characteristics such as intelligence, motivation or other personality traits (Roe, 1952), have added little to our understanding of the determinants of productivity. Despite its intuitive appeal, measured ability has yet to distinguish between more and less productive scientists (Taylor and Barron, 1963; Bayer and Folger, 1966:389–9; Cole, 1974:40). Nor have hypotheses about the impact of motivation on productivity been supported (Pelz and Andrews, 1966:190–1). For example, the "sacred-spark" hypothesis (Cole and Cole, 1973:114), which holds that an inner drive compels some scientists to do research even in the absence of rewards, has been untested, largely because of measurement difficul-

ties. Nevertheless, it has been proposed as an alternative explanation for productivity differences unexplained by reinforcement (see, for example, Allison and Stewart, 1974). The Coles (1973:71) have also suggested that sheer stamina may play an important role in scientific productivity, but the relationship between one measure of stamina—number of hours worked per week—and scientific output is weak (Fulton and Trow, 1974:62; Hargens, 1977).²

Socialization

Because some aspects of professional socialization are public, certain facets of socialization are more amenable to measurement than the professional commitment it is thought to engender. Thus, while we lack data that permit exploring the links between socialization, scientific commitment and conformity to professional norms, researchers have studied the relationship between scientists' socialization and their productivity. Aran and Ben-David (1968), for example, found that "resocialization" into the international scientific community was associated with both increased and "more specific" publications among Israeli medical researchers. Reported links between scientists' later success and their sponsors' eminence (Crane, 1965; Zuckerman, 1967) or the calibre of their Ph.D. departments (Bayer and Folger, 1966:388; Hargens and Hagstrom, 1967; Cole, 1974) provide indirect evidence for the effect of socialization, but these associations may also reflect a selection process wherein graduate students' talents are roughly matched with the calibre of their doctoral sponsors or the quality of their Ph.D. departments. Measures of scientists' predoctoral ability are necessary to sort out these two processes.

² The empirical support for relationships between other research practices and productivity, while mixed, has been generally negative (Gaston, 1970; Simon, 1974). Variation in four research practices (patterns of collaboration, deadlines, number of projects simultaneously pursued, and number of hours spent on research) accounted for only 5% of the variation in university scientists' output (Hargens, 1977).

Reinforcement and the Accumulation of Advantages

While socialization and professional commitment undoubtedly influence conformity to productivity norms, neither are sufficient to maintain conformity without a reward structure that reinforces appropriate role behavior (Hagstrom, 1965:12, 21). Institutional features of science provide a variety of reinforcers such as the personal satisfaction derived from completing creative work (or, in Guston's [1973] terms, from "partaking of the charisma of science"), collegial recognition expressed through the acceptance of research papers for publication or citations to published work, material rewards such as research grants and the conferral of formal honors (Hagstrom, 1965:28). Given its public nature, formal collegial recognition should be a particularly effective reinforcer. Using citations as a measure of collegial recognition, the Coles (1973:110-5) found some support for the reinforcement hypothesis among university physicists, thereby simultaneously demonstrating the utility of a reinforcement approach and the potential value of more fully specified reinforcement models. Their accumulative-advantage hypothesis (Cole and Cole, 1973:119-22) elaborates the reinforcement hypothesis to take into account the feedback relationship between productivity, recognition and resources. Briefly, collegial recognition of published work demonstrates a scientist's merit to those who distribute professional resources (appointments, promotions, grants, research assistance), which not only reinforce past productivity but also facilitate future productivity. Through this process, successful scientists accumulate rewards that lead to even greater productivity. The "Matthew effect" (Merton, 1968) refers to such disproportionate recognition of the contributions of well-known scientists. Allison and Stewart (1974) hypothesized that the process of accumulative advantage would give rise to an improved fit between resources, productivity and recognition with scientists' increasing age. Their findings for academic scientists were consis-

tent with predictions derived from the reinforcement and accumulative advantage hypotheses. While these studies are highly suggestive, they did not separate the effects of ability and socialization from those of reinforcement. Moreover, they were limited to academic scientists. But reward systems are not uniform throughout the scientific community and their effects may depend on scientists' location. Thus, in order to understand the determinants of conformity to productivity norms, we must consider organizational contexts with varying reward structures.

Organizational Contexts and Rewards

Theoretically, organizational context should directly affect scientists' productivity and specify the effects of other variables. Organizations that value the production of knowledge should reinforce such behavior with professional rewards that will facilitate future research (Hagstrom, 1965:39). Organizations committed to other goals will rely on organizational rather than professional incentives, such as promotion to administrative jobs, to maintain performance (Kornhauser, 1962:131; Hagstrom, 1965:37).

The relationship between setting and productivity is well established (Pelz and Andrews, 1966:6; Fulton and Trow, 1974:46; Hagstrom, 1968) and, as expected, publication is less important for promotion in nonresearch-oriented organizations (Meltzer, 1956). Of course, the association between context and output also might be due to selective recruitment, and this alternative must be considered. In either case, the hypothesized impact of collegial recognition on scientific productivity may vary depending on the values of the employing organization. While it may be attenuated among scientists working in contexts not oriented toward the production of scientific knowledge, scientists in such settings who do publish may be even more responsive to mechanisms that symbolize their connection to the larger scientific community.

THE PRESENT STUDY

The research reported here extends previous work in several ways. First, unlike most studies which used cross-sectional data, this analysis employs longitudinal data for the sample members' first postdoctoral decade, so the time order of important events is clear. Second, previous tests of the reinforcement hypothesis were restricted to that minority of scientists employed in universities (Office of Scientific Personnel, 1967:115).³ While the latter do write most scholarly articles (Krause, cited in Hagstrom, 1965:38), by sampling all scientists who are *qualified to do research* (i.e., Ph.D.s) variation in productivity is increased and the impact of organizational context can be assessed.⁴ Third, data on predoctoral ability and educational background provide controls for some aspects of selection and socialization.

DATA AND MEASURES

This research is based on the careers of a systematic random sample of 238 doctoral chemists who obtained their Ph.D.s from U.S. universities between 1955 and 1961. The sample was drawn from the American Chemical Society's biennial *Directory of Graduate Research (DGR)*, which also provided the names of the chemists' doctoral sponsors. Sample members' educational and career histories were obtained from the 11th and 12th editions of *American Men and Women of Science (AMWS)* and from mailed questionnaires for chemists not included in *AMWS*. These sources ultimately yielded professional histories for 95% of the sample,⁵ including data on length of doctoral

study, and employment setting at the beginning and end of the first postdoctoral decade. In order to examine the impact of context on productivity, I classified organizations in terms of research orientation with two dummy variables: university employment versus all other contexts,⁶ and employment in a research-oriented context (university, government laboratory or nonprofit research institute) versus all other contexts.

Productivity and Recognition Measures

For this study, unpublished work, books and patents were excluded from the productivity measure.⁷ Data on productivity came from *Chemical Abstracts (CA)*, American Chemical Society) and *Science Citation Index (SCI)*, Institute for Scientific Information).⁸ Both sources list articles for all authors of multiple-authored papers. Productivity was observed for three time-periods: prior to the Ph.D. ("predoctoral productivity"), during the three-year period following the Ph.D. beginning with the Ph.D. year ("early productivity") and during the chemists' ninth and tenth postdoctoral years ("decade productivity").

Collegial recognition is usually measured with citation counts (Bayer and Folger, 1966; Cole and Cole, 1971), available in the *SCI* since 1961. *SCI* attributes citations to first authors only. While this procedure apparently does not bias estimates of recognition for established scientists (Hagstrom, 1968; Cole and Cole, 1971), counts for scientists who are disproportionately junior authors will underestimate the total citations to their published work and, presumably, the recognition they experience (assuming, of course, that junior authors feel as rewarded by citations or receive as many rewards for

³ For example, in chemistry, only one-seventh of the Ph.D.s in the late 1950s and early 1960s began their careers in university positions.

⁴ This design also avoids the problem that inter-organizational mobility poses for studies restricted to university scientists.

⁵ Nonrespondents (i.e., those for whom career data were unavailable) and respondents did not differ significantly on either early productivity or their number of early citations, but nonrespondents had significantly fewer decade publications. This association is partly an artifact of the way chemists not included in *AMWS* were located (see Reskin, 1973: Appendix 1). Given the high response rate, this difference should have little effect on the results.

⁶ Academic institutions shown in the *DGR* as granting advanced degrees in chemistry are coded as universities; all others are coded as colleges.

⁷ Books represent a very small part of sample members' publications. Excluding patents, however, does underestimate an important form of the productivity of industrial chemists.

⁸ Article counts came from *CA* through 1964; after 1964, they were taken from *SCI*. I did not use available *SCI* data for 1961 or 1964 because these first two volumes underreport sample members' publications.

them as do senior authors). New Ph.D.s are probably more likely than mature scientists to coauthor papers and more likely to be junior authors.⁹ Thus, citation counts may systematically underestimate the amount of recognition young scientists receive.¹⁰ Ideally, one would identify the first authors of chemists' junior-authored papers and add their citations to the junior authors' totals, but this was not feasible. As an alternative, I took authorship order into account in the analyses reported below. The number of citations to the chemists' early work that were shown in the 1961 or 1964 volumes of *SCI* indicate *early recognition*.¹¹ All productivity and recognition measures were standardized by the chemists' Ph.D. year to adjust for variation in *CA* and *SCI* coverage over time.¹²

Ability and Socialization Measures

Controls for predoctoral ability are desirable both to rule out selection as an alternative explanation for apparent socialization or context effects and to specify the regression equations as fully as possible. Since intelligence-test scores are not related to scientific productivity, they are not a desirable measure of predoctoral ability. But scientists do vary in their propensity to do publishable research; and this trait, which should appear rather early in chemists' training, is fairly stable over time (Hargens et al., 1976). Thus, measures of chemists' predoctoral research output should reflect this propensity. Predoctoral publication has been shown to be related to later productivity among sociologists (Clemente, 1973:415).

⁹ Sample members were junior authors in 44% of their pre-Ph.D. publications and in 35% of the articles published during their Ph.D. year and the two following years. They collaborated with their sponsors on about half of the latter articles.

¹⁰ Previous studies (e.g., Cole and Cole, 1973) have failed to take this problem into account.

¹¹ *SCI* did not publish citation data before 1961 or in 1962 and 1963; thus, I used 1961 citations for 1955 to 1958 Ph.D.s, 1964 citations for 1959 to 1961 Ph.D.s.

¹² Since this procedure precludes interpreting the regression coefficients in terms of the increment in later articles associated with each additional early article or citation, only standardized regression coefficients are reported.

However, simple pre-Ph.D. article counts may be biased in two ways. First, coauthored publications may reflect substantial contributions from more experienced faculty members, so I counted the number of single-authored predoctoral publications. Second, the number of predoctoral publications should reflect the length of time in which predoctoral publication could occur, so that the duration of doctoral study (a second indicator of predoctoral ability discussed below) should be positively related to the number of predoctoral publications. To control for this, I regressed publication counts on the length of doctoral study and used the residuals as a measure of predoctoral ability.

An additional indicator of predoctoral performance is the duration of doctoral study. The negative relationship between time taken to complete the Ph.D. and later professional success observed in several disciplines (Hagstrom, 1971:386; Clemente, 1973:415; Reskin, 1973) has been interpreted as the effect of ability or motivation (Folger et al., 1970:265).

I measured three aspects of socialization. The productivity of the chemists' doctoral sponsors during the period the chemists were graduate students should reflect the socialization that sponsors provide through direct tuition and as role models. Actual student-sponsor collaboration should reflect additional professional guidance beyond that involved in routine supervision. Although faculty differ in their propensity to share responsibility for their students' published research, presumably they have a greater degree of involvement in work which bears their names. Finally, a postdoctoral fellowship may provide additional socialization. Fellowship recipients often study at institutions more prestigious than their Ph.D. departments (Berelson, 1962:127) and with more eminent sponsors (N.A.S., 1969:71), often with the goal of acquiring additional skills (N.A.S., 1969:64-5). And the experience pays off: former fellows are more productive than are nonrecipients (Folger et al., 1970:267; N.A.S., 1974; Reskin, 1976). Sponsors' productivity is the number of articles attributed to the sponsors during the four-year

period ending with the chemists' Ph.D. years minus the number of student-sponsor coauthored papers (the second socialization measure). Postdoctoral fellowships (coded 0 for none, 1 for any) were reported in *AMWS* or the biographical questionnaire.

The calibre of the Ph.D. department (measured by rankings in Cartter, 1966) in part measures the quality of graduate training and is associated with productivity. But this association may also reflect the ability of better-quality departments to recruit the most talented students. Studies that simultaneously considered the impacts of ability and the calibre of the Ph.D. department support interpreting the latter as an indicator of ability (see Bayer and Folger, 1966:388-9; Folger et al., 1970:264; Cole, 1974:70); but without sound measures of ability, the implications of the department-calibre/productivity relationship cannot be resolved.

Finally, productivity varies by specialty within chemistry (with organic chemists publishing less; Blume and Sinclair, 1973:133). Specialty was controlled with a dummy variable with organic chemists coded 0, all others coded 1.

RESULTS

Distribution of Publications

The chemists' distribution of publications is consistent with our knowledge that a small proportion of scientists contribute most publications (Price, 1963:45): 7.5% of the sample members did not publish during their first postdoctoral decade, and an additional 11% published only one article. Only 15% of the sample members authored half of the almost 2,000 articles these scientists published,¹³ and, with one exception, no more than 40% published an article in any year. The Gini index provides a measure of the inequality in the distribution of publications. For the eleven-year period beginning with the Ph.D. year, the Gini indices are remark-

ably stable, about .76, indicating pronounced inequality in article productivity. These values are substantially higher than those Allison and Stewart (1974:600) report, reflecting the greater heterogeneity of my sample.¹⁴

These results lead us to the question addressed in the remainder of this paper: what accounts for the differential productivity of the chemists surveyed? Before reporting analyses for the chemists' productivity at the end of their first professional decade, I present the results of multiple-regression analyses for early postdoctoral productivity and early recognition.

Early Productivity

Regression results in equation (1) of Table 1 show some of the expected effects of predoctoral ability and professional socialization on early postdoctoral productivity. While length of doctoral study did not affect early productivity, the number of single-authored predoctoral publications showed a significant independent effect. The direction of its impact varied with the calibre of the Ph.D. department: for chemists from low-calibre departments (those unranked by Cartter or classified as "not sufficient" or "marginal"), single-authored predoctoral publications were positively related to early postdoctoral output; for chemists from more highly ranked departments, the relationship was negative.¹⁵ This specification by the calibre of the Ph.D. department is reflected by the significant positive main effect of predoctoral publications and the negative effect of the interaction term. Students and faculty collaborated more often in better departments than in low-prestige departments, and those students from high-calibre departments who publish alone may do so for want of faculty coauthors.

¹⁴ The indices for my sample show neither increasing nor decreasing inequality over time. This discrepancy with Allison and Stewart's (1974:600) results is due to the substantially longer period their synthetic cohorts cover.

¹⁵ These findings are based on separate regression analyses for each of Cartter's major department-prestige categories, not presented here.

¹³ Because of my restricted period of observation, this figure is higher than Price's (1963:45-6) estimate of 6-10%; some of the chemists in my sample will never publish after their first postdoctoral decade.

Table 1. Regression Equations for Early Productivity, Early Recognition, and Decade Productivity *

Independent Variables	Standardized Regression Coefficients			
	Early Productivity	Early Recognition	Decade Productivity	
	(Equation 1)	(Equation 2)	(Equation 3a)	(Equation 3b)
Prior to Early Productivity				
Single-authored pre-PhD pub.	.251*	.051	.028	.013
PhD duration	.011	-.143*		
PhD calibre	-.028	.112*	.134*	.136*
Sponsors' pub.	-.561*	-.050		
PrePhD collab.	.327*			
Organic specialty	-.164	-.058		
Postdoc. fellow.		.140*	.027	.025
First job univ.	.093		.293*	.288*
PhD dept x spons. pub.	.751*			
PhD dept x prePhD pub.	-.222*			
Prior to Early Recognition				
Early pub.		†	.049	.075
% 1st-authored pub.		†		.120*
1st-authored pub.		.403*	†	†
Pub. x univ. first job		.227*	.446*	.455*
Prior to Decade Productivity				
Early recognition			.330*	.308*
Recog. x univ. first job			-.216*	-.218*
Early pub. x recog.			-.241*	-.245*
Decade job univ.			.291*	.280*
Always univ.			-.228*	-.215*
R ²	.281	.367	.386	.399

* Except for a single indicator of each theoretically relevant control variable, variables that were not statistically significant at the .05 level were excluded.

* Coefficient at least twice its standard error.

† Omitted in favor of an alternative measure. See text for bases for selecting alternative measures for particular equations.

Two of the socialization indicators significantly affected early productivity, but the effect of one—sponsors' productivity—also depended on the calibre of the sample members' Ph.D. departments. In low-calibre departments, working with a productive sponsor *reduced* the chemists' own early productivity, whereas in "distinguished" departments, having a prolific sponsor increased early postdoctoral output (see footnote 15). This interaction is reflected in the significant negative main effect of sponsors' productivity and the significant positive product of the former and Ph.D.-department calibre shown in equation (1). Assuming a moderately good fit between faculty talents and departmental calibre, faculty at top departments would be better researchers and have more to offer their students than equally productive staff at

low-prestige departments. Productive faculty at lower-quality institutions might also be skeptical about the value of investing time in training students whom they do not expect to pursue research careers.

As predicted, the chemists benefited from predoctoral collaboration with their sponsors. While this result may reflect socialization, an alternative explanation is possible. Collaborations among chemists during graduate school may lead to postdoctoral coauthored articles. To test this alternative, I included the number of articles the chemists coauthored with their sponsors during the first two post-Ph.D. years in the regression equation. While the standardized coefficient for predoctoral collaboration dropped slightly (from .33 to .29), it remained an important predictor of early productivity.

Any socialization resulting from a post-

doctoral fellowship should not be reflected in early productivity because the interval between completing the postdoctoral fellowship and the time remaining in the early-productivity measure was too short for any effect of an award to be reflected.¹⁶

Apart from the interactions described above, the calibre of the Ph.D. department was unrelated to early productivity. (The negative sign for its regression coefficient results from including the two Ph.D.-calibre interaction terms in the equation and has no substantive meaning.) More surprising was the failure of either of the measures of first employment setting to show the expected positive effects.¹⁷ The zero-order associations between setting and productivity, although small ($r = .11$ for universities; $.16$ for all research-oriented organizations), were significant, but their partial association disappears because the same background variables that affect early productivity are important determinants of working in these contexts.

Early Recognition

The analysis of early recognition provides a partial test of the accumulative-advantage hypothesis. Scientists may accumulate advantages by acquiring training or credentials that enhance either the quality or the visibility of their work. The advantages may elicit greater collegial recognition (in itself reinforcing) and may also attract additional resources that will facilitate future performance. For example, a purported benefit of the "Matthew effect" (Merton, 1968) for young scientists is the increased visibility resulting from collaborating with established scientists. Even without actually collaborating, students of well-known scientists may receive disproportionate recognition.¹⁸

¹⁶ In addition, for 29% of the postdoctoral fellows the award was subsequent to the period covered by the early-productivity measure.

¹⁷ The results for employment in a research-oriented organization are similar to those shown in equation (1) for university employment.

¹⁸ This assumes that this ascribed status is known to potential citers; acknowledgements sometimes reveal such information. Of course, such a pattern would also occur if they did better work.

Young chemists who come from prestigious Ph.D. departments or who obtain desirable university jobs may also accumulate advantages if their institutional affiliation listed on their articles elicits increased citations. I tested for such advantages by examining the effects on early citations of the interaction of early productivity and (a) collaborating with one's sponsor, (b) collaborating with a particularly productive sponsor, (c) coming from a prestigious Ph.D. department and (d) working in a university.

The regression equation for early citations is shown in equation (2). Although predoctoral publication was not related to early recognition, the more quickly the chemists completed their training, the more often they were cited early in their careers. The calibre of their Ph.D. department and a postdoctoral fellowship were also positively related to early citations. Their sponsors' productivity did not affect the frequency with which the chemists were cited, and the significant zero-order effect of coauthoring papers with their sponsors during graduate school ($r = .18$) was mediated entirely by its impact on early postdoctoral productivity. However, before concluding that collaboration had no direct effect on recognition, we must ask whether order of authorship specified the relationship. Given the underestimation of citations to junior authors, the expected collaboration effect should hold only for chemists who were primarily senior authors. In fact, results of an analysis of covariance not shown here revealed no significant difference between chemists who were exclusively junior and exclusively senior authors; in neither of these subgroups did collaboration contribute significantly to recognition.

Since cited scientists first-author at least one article (except for a few citations to unpublished work), the strong effect of the number of first-authored articles published between the Ph.D. year and the second postdoctoral year ($b^* = .403$) is expected.

Although employment context had no simple effect on citations, the return in citations to early publications was significantly greater for chemists whose first

jobs were in universities. Their work may have been more useful than that produced by nonuniversity chemists, or their institutional affiliation may have influenced readers' judgments. None of the other product terms included to test accumulative advantage was statistically significant, and they are not included in equation (2). While the significant effect for the employment-context/early-productivity interaction term is consistent with the accumulation of advantages to scientists who were initially well placed, it would also follow from a selection process in which potentially better researchers who produced work deserving greater recognition were hired by universities. Lacking an independent measure of the quality of publications (citations themselves are usually used for this purpose), I can not choose between these interpretations. Still, several measures of ability were controlled in the regression procedure. At the very least, these results do not rule out the possibility of accumulative advantage with respect to the first-job setting.

Decade Productivity

By the end of their first postdoctoral decade, sample members had had ample time to establish research programs, publish results and build at least minor reputations. Thus, I observed their productivity at this point, seven years after early productivity was observed and three to six years after early citations were counted. The lag is small enough for any effects of early recognition to persist, but enough time elapsed so that any independent effect of early productivity would constitute support for the sacred-spark hypothesis. Mobility during the first postdoctoral decade should lead to an improved fit between the chemists' productivity and employers' requirements, so organizational context at the end of the decade should mediate the effects of early productivity on later productivity. The decade-productivity regression results (see equations [3a] and [3b]) provide some support for the sacred-spark, reinforcement, and accumulative-advantage hypotheses.

Predocutorial performance and socialization. The calibre of the Ph.D. department

exhibited a significant independent effect on the chemists' productivity at the end of their first professional decade. Its impact is noteworthy, particularly because this variable did not significantly affect early productivity. The calibre of the Ph.D. department might reflect the enduring, albeit slight, impact of access of effective socialization on subsequent productivity. Institutions differ in the quality of instruction they provide, and these differences presumably affect their graduates' later performance. But department calibre may also have ascriptive effects on productivity (Crane, 1965; Hargens and Hagstrom, 1967) through its impact on the allocation of certain unmeasured resources to young scientists. For example, established scientists more often may invite young colleagues from prestigious Ph.D. departments to collaborate, either out of paternalistic feelings or from a desire to exploit the latter's familiarity with the newest techniques.

The importance of these "school" effects is highlighted by the absence of any independent effects of sponsorship. Collaborating with one's sponsor during graduate school led to increased early postdoctoral productivity, but any direct effect was lost by the end of the decade. Similarly, studying under a highly prolific sponsor increased recognition for early work, but had no direct effect on later productivity. This does not mean that the quality of sponsorship is unimportant for scientists' careers, but any measurable effects on productivity may be short-run and primarily ascriptive.

I argued that by providing additional socialization a postdoctoral fellowship should increase conformity to productivity norms and, hence, output; but, contrary to previous findings, the analyses revealed neither short- nor long-run effects. Apparently the effects of the precursors and consequences of a fellowship account for most of its zero-order association ($r = .17$) with decade productivity.

Organizational context. Beginning one's career in a university was a far more important determinant of decade productivity ($b^* = .29$) than one's graduate school credentials, and this effect was independent of university employment at

the end of the decade. Initial university employment could increase the decade productivity of scientists who subsequently left the university for several reasons, the most obvious being the continuation of collaborative work with former university colleagues. Former colleagues may also control the allocation of ongoing opportunities as conference organizers or journal editors, for example, or they may continue to serve unwittingly as a reference group. Some scientists who leave the university for less prestigious jobs may try to show former colleagues that they could have succeeded by university standards. Exposure to professional values regarding the primacy of research on one's first job may also facilitate their internalization (see, e.g., Cotgrove and Box, 1970: 127); chemists whose first jobs are in nonresearch-oriented contexts may view such values as purely academic. Finally, the enduring effect of early university employment may actually reflect universities' ability to recruit (if not always to retain) potentially productive chemists. While graduate students prefer university careers (Davis, 1962:158), in the face of growing economic pressures some of these more able scientists may be seduced by the financial prospects of industry. Controlling for predoctoral ability reduces the plausibility of this alternative, but does not exclude it.

Early productivity and the sacred-spark hypothesis. The data do not permit a direct test of the sacred-spark hypothesis. However, such a drive to publish should be fairly stable, leading to a strong partial association between early and later productivity. We see in equation (3a) that early productivity did not significantly affect decade output, but the absence of a direct effect does not necessarily disconfirm the hypothesis. While the failure to publish early in one's career may signal the absence of a spark, publication does not necessarily signal its presence, because a chemist's early publication rate may reflect the efforts of more experienced or motivated collaborators primarily responsible for joint efforts. If we assume that the order of authorship on articles reflects the authors' relative contributions, the partial association between

early first-authored articles and decade productivity provides a more realistic test of the sacred-spark hypothesis.

When both early productivity and the proportion of early first-authored publications were included in the regression, the latter did positively affect decade productivity (see equation [3b]). Apparently, early-publication rate, when measured to exclude publications for which the chemists may not have been primarily responsible, is associated with subsequent productivity. These results provide *very limited* support for the sacred-spark hypothesis: while their slopes are statistically significant, less than one percent of the variance in decade productivity among the sample as a whole can be attributed to the operation of a sacred spark.

While the sacred-spark hypothesis implies that strongly committed scientists will publish without social support, it does not necessarily follow that social mechanisms and social context have no effect on the productivity of individuals either blessed with or lacking a spark. Among chemists whose first jobs were in universities, early productivity significantly affected decade output. Since the emphasis on and rewards for productivity are generally greater in universities than in other sectors, a drive to publish should flourish in such settings. The significant-setting/early-productivity interaction is also consistent with the accumulative-advantage hypothesis. Universities allocate a variety of unmeasured rewards for early productivity (desirable teaching assignments, increased research facilities, etc.) that presumably foster continued productivity (Hagstrom, 1968). For nonuniversity chemists, other signs of successful performance (teaching evaluations, development of industrial applications, patents) bring similar organizational rewards, while publication is presumably incidental to career advancement.

Collegial recognition and the reinforcement hypothesis. Collegial recognition in the form of citations has a strong impact on decade productivity ($b^* = .308$). Since citation counts underestimate the number of citations for junior authors, I tried to assess the effect of this bias by

examining the relationships among the 98 chemists who had no junior-authored early publications. Among this subsample, the independent effect of early citations was 40% stronger than it was for the entire sample ($b^* = .456$ and $.308$, respectively; the equation for the subsample is not shown). Thus, of the variables considered here, early recognition is the most important determinant of decade productivity, accounting for almost 10% of its variance. However, this probably overestimates the effect of collegial recognition, which is certainly correlated with other, unmeasured structural rewards for early productivity that also facilitate productivity.¹⁹

Hagstrom's (1965:37) discussion of organizational incentives implies that the effect of recognition should vary by organizational context, with recognition from the scientific community being less important to scientists in contexts that do not emphasize research. To test this, I introduced into the regression equation a multiplicative term for first-job context and early citations. While significant, the effect of recognition on later productivity was *weaker* for chemists whose first jobs were in universities or other research-oriented organizations. Apparently in organizations that emphasize and immediately reward research and publication, formal recognition from the scientific community is less important in maintaining conformity to productivity norms than in nonresearch-oriented contexts in which rewards for productivity may be uncertain or scarce.²⁰

The impact of recognition on chemists' later productivity also varies with their

early productivity level. The greater one's early productivity, the weaker the effect of recognition on later output. Citations may be more reinforcing to late bloomers than to chemists whose early publication records are strong. The latter may have a strong drive to publish and need less social support to maintain their output or, perhaps, the intrinsic rewards of their early successes render productive researchers less vulnerable to the reactions of the scientific community.

Accumulative advantage. Without data on the chemists' research resources, I cannot directly test the accumulative-advantage hypothesis, which predicts both a direct reinforcement effect of citations and indirect effects from early productivity and early recognition mediated by the resources both can attract. Since research resources vary considerably by organizational context, a crude test of the hypothesis might use decade context as a proxy for resources. University employment at the end of the decade significantly increased decade productivity ($b^* = .280$), which is consistent with the accumulation of advantages. However, the selection hypothesis remains a viable alternative.

Chemists who began and ended their first decade in universities were significantly less productive than those who moved into universities from other contexts. While this result is consistent with a beneficial effect of mobility, it is as likely that universities require faculty they recruit from other contexts to be superior to their current faculty (see Cole and Cole, 1973:118).

SUMMARY AND CONCLUSIONS

Regression analyses of longitudinal data have provided estimates of the causal links between chemists' predoctoral training, early productivity, collegial recognition, organizational context, and productivity at the end of the first postdoctoral decade. After identifying methodological problems involved in using *SCI* citation counts to assess the effect of collegial recognition, I report tests of several hypotheses that have been proposed to account for variation in scientists' productivity.

¹⁹ The coefficient for citations will better estimate their true effects (in similarly specified equations) in fields where research work is less routine, since in routine fields such as chemistry (Hargens, 1975) the unmeasured resources that should accompany recognition are more likely to lead to increased productivity.

²⁰ A larger proportion of chemistry Ph.D.s work in nonacademic settings than in any other natural science, so nonacademic chemists may feel less isolated from colleagues doing basic research than similarly situated scientists in other disciplines. Insofar as this is true, the impact of formal recognition on nonacademic scientists' productivity should be even stronger in fields in which most practitioners are concentrated in university research.

Apart from the enduring effect of the calibre of the Ph.D. department, the effects of predoctoral socialization are slight and short run. Sponsor's productivity affects only early recognition; and predoctoral collaboration with one's sponsor increases early productivity, but does not affect decade productivity directly. Although sponsorship may ascribe to new Ph.D.s a professional status that affects their job placement and, indirectly, their productivity, educators apparently overestimate the direct effects of graduate training on scientific achievement.

The hypothesis that collegial recognition in the form of citations influences later productivity was supported, but to a smaller extent than past research on university scientists would suggest. This is not due to the heterogeneity of the sample: the effect of citations on decade productivity was *weaker* for those in contexts that emphasize research. In such contexts, immediate, informal recognition from research-oriented colleagues may be more important in maintaining productivity than the formal, but delayed recognition that citations provide. This is consistent with the greater direct effect of early productivity on decade productivity among university chemists than those employed in other contexts. Given the reward structure in most university departments, the act of publishing—signalling both successful professional performance and a variety of forthcoming rewards—may be especially reinforcing to university scientists. On the other hand, scientists in contexts that do not stress research and who thus lack informal or material rewards for publishing, may depend more on the formal response of the scientific community. Such recognition may symbolize their ties to the larger scientific enterprise and thereby encourage continued conformity to productivity norms.

The effect of organizational context points to the role of organization-specific reward structures and is consistent with the accumulation of advantages among scientists whose jobs provide access to resources that facilitate productivity. However, if context only affected performance in this way, its impact should dis-

appear when scientists change employment settings. The enduring effect of beginning one's career in a university may reflect on-the-first-job continuation of predoctoral socialization.

Although the analyses provide no direct evidence for the sacred-spark hypothesis, neither do they permit its disconfirmation. Chemists' predoctoral and early postdoctoral productivity had very small but significant independent effects on decade productivity. Moreover, the greater one's early productivity, the less the importance of recognition for later productivity; thus, highly motivated scientists—or those who quickly developed effective work habits—need less recognition to continue publishing. The expression "sacred spark" may be an unfortunate choice, since it suggests an innate drive uninfluenced by social factors. Any relatively stable propensity to publish probably depends on scientists' achievement needs, socialization and a constellation of competing interests and activities. The effect of such a latent propensity on actual performance would depend on a scientist's circumstances, including access to resources and rewards. The interaction effects on decade productivity of early productivity with both employment context and recognition should remind us that individuals' scientific drives operate in social contexts.

These results bear on the general question of maintaining normative conformity. Theories of normative conformity stress the importance of sanctions to maintain commitment induced by socialization (Goode, 1960: 254). While my results confirm the importance of *positive* reinforcement among professionals, they provide an incomplete picture of the role of sanctions in maintaining role performance. Anticipated or actual *negative* sanctions are also powerful motivators for scientists. Theories of scientific competition, for example, emphasize the consequences of fear of anticipation on research and publication (Collins, 1968; Gaston, 1971; Hagstrom, 1974). Thus, the extent and severity of competition in a scientific specialty may increase scientific productivity; and those factors that affect competition, such as specialty size, normative

consensus and functional integration (Hagstrom, 1974; Hargens, 1975), should, in turn, affect productivity indirectly. A single control for specialty, as used here, cannot capture such differences. Similarly, the publish-or-perish injunction expresses an important potential sanction for academic scientists. The actual motivating capacity of job insecurity should vary with conditions in the job market (which also vary by specialty). Finally, scientist may fear that past or present associates will view them as failures if their productivity declines (Fulton and Trow, 1974:54; Zuckerman, 1967: 402). If scientists publish in part to avoid losing face with significant others, poorly integrated scientists would have less to lose from low performance and, consequently, should exhibit lower productivity (see Homans, 1974, regarding the relationship between social status and conformity). Studies of scientists' actual and perceived positions in collegial networks are necessary to test these conjectures. I have shown that the same rewards vary in their importance with scientists' employment locations and exposure to formal reward structures. Surely integration into informal networks or invisible colleges is as important.

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A COMPUTER SIMULATION OF THE EMERGENCE OF CONSENSUS IN CROWDS *

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A simple model of crowds seeking consensus for action is posited. Individual crowd members seek support from a sample of crowd members; successful ones attempt to influence the crowd toward a particular course of action. Consensus is achieved with a sufficiently reduced variability of opinions in the crowd. Independent variables are the initial distribution of opinions, crowd suggestibility and the average position of individuals seeking to influence the crowd. Outcomes of interest are whether consensus is achieved within a given time limit, the rapidity with which consensus is achieved, and the extremity of the consensus position reached. Most of the simulated crowds reach consensus within a given time limit; rapidity and extremity of consensus are considerably affected by interaction among the manipulated variables. A partial mathematical description of the model suggests that the results are nontrivial.

Many recent conceptualizations of crowd behavior emphasize the interaction within an initially heterogeneous collectivity which leads to the behavior characteristic of crowds. Although these recent approaches differ in emphasis, they share the view that crowd behavior occurs when a heterogeneous plurality of individuals, faced with an ambiguous or unstructured situation, reach a consensus for behavior through a process of interaction or milling. The basis for the consensus is variously explained by the theorists we discuss; however, all reject notions of contagion or convergence (see Turner, 1964a), which emphasize the distinctive aspects of crowd behavior, and focus in-

stead on its similarities with the action of more structured groups.

Our purpose in this paper is to draw from these recent discussions in order to synthesize some propositions about crowd processes, to describe a model of the internal dynamics of crowd processes based on these propositions, to specify more clearly the variables involved, and to test some implications through computer simulation.¹ Our synthesis initially will be limited to crowds rather than encompassing collective behavior in general.

Assuming that a heterogeneous collectivity is aggregated in a potential crowd situation and ignoring the basis for, and process of assembling (McPhail and Miller, 1973), there are at least three important questions that must be addressed in explaining the resultant concerted behavior. One must consider the effect of the initial distribution of individual characteristics, the function of influence or leadership on the individuals; and the change in behavioral intentions of the individuals as a reaction to the influence.

*A much different version of this paper, which emphasized the technical aspects of the computer simulation, was read at the Sixth Annual Pittsburgh Conference on Modeling and Simulation, School of Engineering, University of Pittsburgh, Pittsburgh, Pennsylvania, April 24-25, 1975. We wish to thank our colleagues, David C. Lundgren and Gregory J. Moschetti, for their perceptive and constructive suggestions. Our high suggestibility in relation to their comments, of course, makes them responsible for any inadequacies in this paper. We are also grateful to a colleague in the Mathematics Department, H. David Lipsich, who reviewed our mathematical calculations, and to anonymous reviewers who made valuable comments on an earlier version of this paper.

¹ We will not discuss the more macroscopic problems addressed by many theorists such as Smelser (1962) and recently advocated for study by Weller and Quarantelli (1973).

Individual Dispositions

Most sociologists reject explanations of collective behavior based on individual characteristics. Brown and Goldin (1973:173) argue that "neither the entrance of individuals into collective behavior nor their behavior during participation in collectivities can be explained by their underlying beliefs, attitudes, intentions, or states of motivation." Turner (1964a), in his elaboration of the emergent norm approach to collective behavior, McPhail (1971), in his analysis of research on civil disorders, and Weller and Quarantelli (1973), who are critical of both individualistic and interactionist approaches all concur. Research on crowd behavior is equally unsupportive of an individualistic interpretation. Studies of crowd members from the French Revolution (Rude, 1964) to recent student disorders (Foster and Long, 1970) find that participants in crowd behavior do not share distinctive psychological attributes.

Although no particular personality traits are characteristic of crowd participants, the *distribution* of individual characteristics, viewed as a property of an aggregate, is an important sociological variable to be considered in explanations of crowd behavior. In a crowd, the distribution of individual characteristics such as beliefs, motives, attitudes, behavioral dispositions or social statuses is frequently considered to affect either the probability of crowd behavior occurring or the form of the behavior that occurs. Smelser's (1962) "generalized belief" can be reduced to the beliefs of participants, as has been observed by Weller and Quarantelli (1973). Turner and Killian (1972:27) note the impact on the crowd of different types of individuals and suggest that consensus in an incipient crowd changes as the individuals who constitute the crowd change (Turner and Killian, 1957:138).

Berk (1972; 1974) and Johnson (1974) more explicitly discuss the effect of the distribution of individual dispositions on the crowd's behavior. Berk's (1974) observations of crowds suggest that the emergent action in the crowd is a function of what people bring to the scene as well as

the crowd situation. He contends that individuals in crowds bring a variety of motivations "ranging from passionate commitment . . . to simple curiosity" (Berk, 1972:312) out of which a consensus emerges through negotiation and compromise; if most crowd members are moderates, the modal action of the crowd will be moderate unless someone more militant persuades the moderates to act. Berk (1974:364) also notes that crowds in which there is little initial variation will reach consensus more readily. This suggests that if all were equally moderate, agreement could be readily reached on some moderate action; if all were militant, the rapid consensus would be on some militant activity. This implies an effect of the distribution of individual dispositions on both the time required for consensus and the consensus itself.

Johnson (1974) argues that the emergence of consensus in a crowd can be viewed as analogous with the "risky shift" which occurs in group decision making in the experimental laboratory. Concurring with Berk, he suggests that individual crowd members arrive on the scene willing to engage in a variety of different actions, from extreme to moderate. But where Turner and Killian refer only to a variety of motivations and Berk refers to moderates and militants, Johnson suggests that the variety of acts suggested or supported by different individuals might be arrayed on an extremity dimension similar to the degrees of risk in the risky shift experimentation. Through discussion, a shift to a position more extreme than the mean position of the individuals in the group is likely to occur.²

Thus, these three (Turner and Killian, 1972; Berk, 1974; Johnson, 1974) all argue that initially heterogeneous, incipient crowds reach consensus through an interaction process. Although none argues that persons involved in crowds have distinctive personality traits, each suggests that the *distribution* of behavioral intentions or dispositions in the aggregate affects the crowd activity—the probability

² A shift to a less extreme position is likely if the positions of the individuals are generally less extreme; thus, an incipient crowd might disperse and no crowd behavior emerge (see Johnson, 1974).

of crowd formation, the extremity of action, or the milling time required for consensus.

This argument, and the model developed subsequently in this paper, must assume that all members of the incipient crowd are oriented in the same direction, i.e., they all share a common focus. It is not applicable without modification to the possibility of a crowd which includes two sets of individuals with antagonistic objectives. For instance, a crowd involving militant blacks confronting white racists, e.g., a race riot, would have to be treated as two separate but interacting crowds. One could postulate that the dispositions among blacks ranged from passive opposition to the whites to a willingness to take violent action against them, and vice versa for the white racists. One would then examine the emergence of consensus within each group separately.³

Thus, following Johnson (1974), we believe that the individual dispositions in a crowd (whose members have a common focus) can be arrayed on some dimension of extremity or willingness to engage in non-normative actions. Persons might be willing, for instance, to engage in a peaceful march, but not in a sit-in. Others might be willing to engage in some violent demonstration. One can characterize a crowd on the basis of the shape of the curve which describes the initial distribution of individual participants.⁴ The curve might take any form, e.g., normal, U-shaped or skewed, depending on a variety of situational factors, but the form then has an independent effect on the crowd activity.

Influence on the Crowd

Unless one assumes that external factors are the sole determinants of crowd behavior or that each crowd member is

equally influential, one must assess the impact of influence or leadership on a crowd. Turner and Killian's (1972) emergent norm approach draws on Sherif and Harvey's (1952) experimental work with the auto-kinetic effect, which suggests that an emerging group standard will approximate the mean of the individual judgments, an expected outcome when all members are equally influential. With their *post hoc* notion of the "keynoter," however, Turner and Killian (1972:47) introduce differential influence into their interpretation. They suggest that, in ambiguous situations, the first forceful proposal of definite action will be followed. Although it might appear that a first forceful suggestion to disperse might be as readily followed as a suggestion to "storm the Bastille," Turner and Killian suggest that the speaker must receive support or he does not keynote the crowd. Thus, for a speaker to determine the course of crowd behavior, latent support for the position of the speaker must exist in the crowd. This implies an interaction effect of the speaker with the crowd's distribution of dispositions to behave.

Turner and Killian do not address the question of what types of suggestions are most likely to be *made*. If we assume a heterogeneous grouping with a variety of individual dispositions in the initial stages of crowd formation, it still appears that a first strong suggestion to "go home" might be as likely to be made as a suggestion to "storm the Bastille." Which suggestion was followed would then depend only on temporal primacy. Thus, the important and unanswered question here is whether the *occurrence* of a suggestion varies according to the extremity of the action involved. Given a wide range of individual dispositions, is a suggestion to go home as likely to occur as a suggestion to take some other, more extreme action?

Berk (1974) begins to resolve this issue with ideas from game theory. He argues that the ultimate crowd consensus is based on a rational calculation of potential rewards and costs to individuals. The perception of the possible payoff is a function of perceived support from crowd members and the potential rewards for action as opposed to inaction. Thus, Berk im-

³ A similar problem is mentioned by McPhail and Miller (1973:733) who discuss the possibility of some persons receiving instructions to assemble for "an anti-war demonstration, while others may be instructed to assemble at the same time and place to 'bug the peaceniks'" It is this kind of situation which requires the slight modification of our model.

⁴ We are not suggesting that the dispositions of crowd members can be arrayed on only one dimension. Willingness to engage in non-normative actions, however, seems to be an important dimension which could encompass a variety of motivations.

plies that all members of the crowd *seek* support, and those most likely to propose a course of action are the ones who perceive that their proposals will *receive* support. Where Turner and Killian see that support affects whether a speaker will be followed, Berk's reasoning leads to the suggestion that the perception of support affects whether an individual will *attempt* to influence the crowd. Since the individual in a crowd is not always able to judge accurately the crowd mood and calculate the probability of support, the crowd reaction to the speaker might or might not be supportive.

Although Berk addresses the question of who becomes a speaker, he does not ask who is most likely to seek support. If all seek support in a heterogeneous grouping in which all positions are represented, it seems likely that all are equally likely to find minimal support. Johnson (1974) addresses the latter issue by suggesting that persons at more extreme, or risky, positions are more likely to seek support than are more moderate individuals. He suggests that the correlation that exists between intensity and extremity of opinion (cf. Suchman, 1950) might account for the greater probability of the potential speaker coming from the extreme end of the distribution. More extreme persons, who hold their opinions more intensely, would be more concerned with influencing the course of action. A similar explanation for the shift to risk in experimental groups has been advanced by Burnstein (1971), who argues that persons supportive of risky positions are more confident, thus more assertive and influential.

Together these arguments support a position concerning the emergence of leadership that is more complex than Turner and Killian's notion of the keynoter, which implies that the first strong suggestion for action is followed by the crowd. It is congruent, however, with their observation that there may be a succession of potential keynoters who offer a variety of alternatives from which the crowd selects (Turner and Killian, 1972:89). This leads to a view of leadership as a function of both a group factor—the crowd distribution—and individual factors—the intensity with which individuals hold their

opinions. Following Johnson (1974), we believe that the extreme individual who holds his position intensely will take the first step toward influencing the crowd. That step is, according to Berk (1974), to sample the crowd in order to assess the degree of support for his position. If the individual perceives that sufficient crowd support does exist, he then attempts to influence the crowd by verbalizing his suggestion for action. Rather than uncritically following that suggestion, the crowd merely assesses it and reacts. The reaction involves the possibility of some shifting of individuals toward the speaker's position, which begins the process of consensus emergence. If consensus is achieved, the crowd is ready to act on the suggestion; if not, other individuals with other preferred courses of action seek support, make suggestions and get reactions from the crowd until consensus emerges or the crowd disperses.

Suggestibility

Whether it is labeled contagion (LeBon, 1960), social facilitation (Allport, 1924) or circular reaction (Blumer, 1939), nearly all students of collective behavior refer to a notion that individuals in the crowd situation are especially susceptible to the influence of others. The idea persists in the work of contemporary theorists, such as Turner and Killian (1972:32), who refer to the "heightened responsiveness of the individual to cues provided by others when situational anchorages are inadequate"; Lang and Lang (1961:221) and Smelser (1962:11) make similar arguments.

Even if the notion of heightened suggestibility is rejected (Couch, 1968), for consensus to arise in an initially heterogeneous grouping some individuals must change their opinions or behavioral intentions.⁵ Although consensus is generally not defined and is rarely thought of as unanimity (Berk, 1974), a shifting toward

⁵ This assumes that the individuals making up the group do not change. A group might also reach consensus through selective withdrawal and/or selective recruitment of individuals. The model developed in this paper assumes, however, that the group membership remains constant.

some basis for concerted action is typically thought to occur in crowds.

An important question is whether crowds vary in this tendency. Variation can be examined from two perspectives. One can look at individual differences in suggestibility, an early interest in communication research (Hovland et al., 1953) or situational variables that are conducive to suggestibility. The major emphasis by theorists of collective behavior is on the latter, which affects all members of crowds similarly. The situation which produces heightened suggestibility is variously referred to as ambiguous, problematical, unstructured or characterized by strain. Common to these terms and their definitions is some idea that collective behavior grows from situations for which the culture or social structure provides insufficient guidelines for behavior.⁶ The assumption is that social behavior occurs within a cultural and structural framework which provides definition for, and prescribed appropriate behavior in a variety of situations; however, not all situations are clearly defined. We will use the term "ambiguous situation" to refer to those situations which lack *sufficient* cultural definition for clearly determining appropriate behavior, and the term "suggestibility" to refer to a willingness to change particular dispositions in response to cues from others. Regardless of the particular terms used, the condition indicated by ambiguous situation is generally perceived as increasing "the suggestibility of individuals to the actions of others" (Turner and Killian, 1972:95).

⁶ Blumer (1939) says that collective behavior involves group activity "that comes into being and develops along lines that are not laid out by preestablished social definitions"; Turner (1964b:132) suggests that collective behavior occurs "when the conventional normative structure is internally conflicting or inapplicable to the situation at hand"; Shibutani (1966:172) refers to crises in which "the previously established social machinery breaks down"; Smelser (1962:47, 51-4) refers to structural strain as "inadequate functioning of the components of (social) action" and suggests that ambiguity is a principal kind of strain; Lang and Lang (1961:16-9) refer to "gaps" in the social structure; and Turner and Killian (1972:64) say that collective behavior occurs in situations where "cultural and social organization do not provide adequate directives for coping with the situation."

Although this suggestibility in crowds is contrasted with more deliberate behavior in structured groupings, the question of varying degrees of suggestibility in crowds is little discussed. If suggestibility is due to ambiguity, it seems that greater ambiguity would lead to greater suggestibility. This can be inferred from Turner and Killian's reliance on the experimental work of Sherif and Harvey (1952), which indicates a greater tendency for convergence on a group standard in more ambiguous situations. A similar inference can be drawn from the discussion of rumor by Allport and Postman (1947) and Shibutani (1966), both of which indicate that the magnitude of rumor spreading is a function of the ambiguity in the situation.

Given that incipient crowds may vary in suggestibility, whether the ultimate crowd action varies with suggestibility is unclear. It seems evident that the rapidity with which convergence on a group consensus occurs is a function of suggestibility, but its effect on *which* action of a range of possible actions emerges as the crowd consensus is less clear. Couch (1968) and Berk (1974), for instance, argue that crowd action is no less deliberate nor more emotional than other group decisions, implying that the suggestibility has no unusual effect on the outcome. Sherif and Harvey's (1952) work does not indicate that the group standard which emerges in more ambiguous situations differs from the standard emerging in situations of lesser ambiguity. Only the rapidity with which it emerges is affected. Shibutani's (1966) work, however, does indicate that rumors in situations of greater ambiguity are less congruent with general cultural standards, and Smelser (1962:72) adds that the generalized beliefs on which collective behavior is based are "short circuited," producing the "excessive" or "irresponsible" aspects of crowd behavior.

Thus, it might be argued that the heightened suggestibility in crowds varies with the degree of ambiguity in the situation, and the degree of suggestibility then affects the behavior that takes place in the crowd situation. It might affect either the rapidity with which convergence on some course of action occurs, or the course of

action itself. Since the idea of suggestibility assumes a "tendency to respond uncritically to suggestions" (Turner and Kilian, 1957:84), it must imply that individuals are shifting in the direction of some person or persons making a suggestion. If one could vary the degree of suggestibility of crowd members, by inference as a result of varying ambiguity in the situation, one might expect different outcomes from the crowd.

Summary

This discussion has identified three variables related to incipient crowds which might influence the outcome of crowd interaction, the dependent variables. These variables are operationally defined in our computer simulation model. We hypothesize that the distribution of individual dispositions in an incipient crowd, the type of influence efforts that are made and the suggestibility of the crowd (which we infer is due to the ambiguity of the situation) together determine the outcome of the interaction. Outcome variables include whether consensus for crowd action occurs, the rapidity with which it emerges and the form of the consensus—i.e., its location on a dimension of extremity.

Description of the Model

We begin by assuming that a large number (100 in our experiments) of individuals are gathered within some space without physical characteristics of consequence to the communication (and its informational content) and movement of the individuals. Individuals constituting the incipient crowd cannot leave the area nor can individuals enter the space after the process begins. Thus, there is a constant number of individuals with perfect communication among them. Ignoring why the individuals are gathered and the specific content of the issue which might incite the crowd into action, we assume that each individual has an initial disposition toward a particular course of action with respect to the issue, and all are oriented in the same direction as discussed earlier. The different courses of action are assumed to

fall along a discrete ten-point scale: position 1 is the least extreme course of action, and position 10 the most extreme.⁷ Finally, we assume that a finite amount of time is available for the individuals to agree on a course of collective action. Thus, there is the possibility that the individuals would not agree on a course of collective action within the time allowed.

The activity of the crowd in trying to reach consensus on some course of action is seen to involve three *recurring* processes: milling, influence attempts through speechmaking and crowd reaction (see Figure 1).

In the milling process, one *potential speaker* appears. This potential speaker, having a particular disposition to act, is chosen by matching a random number against a set of probabilities generated by a binomial distribution (see Appendix). Following Johnson's (1974) argument, the binomial distribution which generates the set of probabilities is set up to give greater likelihood of the chosen potential speaker having a more extreme disposition to act (i.e., six or more on the ten-point scale).

Each recurrence of the milling process, and thus the choice of a new potential speaker, is counted as the beginning of a new cycle in the model. The cycles represent time, and the crowd is restricted to an arbitrarily fixed number of cycles (50) within which it is possible to reach consensus on a course of collective action.

The milling process is one in which certain individuals survey the disposition of other crowd members, partly to discover whether there seems to be sufficient support for their own views. More specifically, the model has the potential speaker seeking support for his own disposition to action by sampling a small number (five) of crowd members and discovering the dispositions (on the ten-point scale) of the sample members. Because we have assumed that communication is perfect and

⁷ This ten-point scale follows Johnson's (1974) suggestion that the variety of dispositions in a crowd can be arrayed on an extremity dimension similar to the dimension of risk manipulated in the risky shift experimentation. The selection of ten as the number of possible dispositions is arbitrary, but it corresponds with the ten-point scale used in risky shift research.

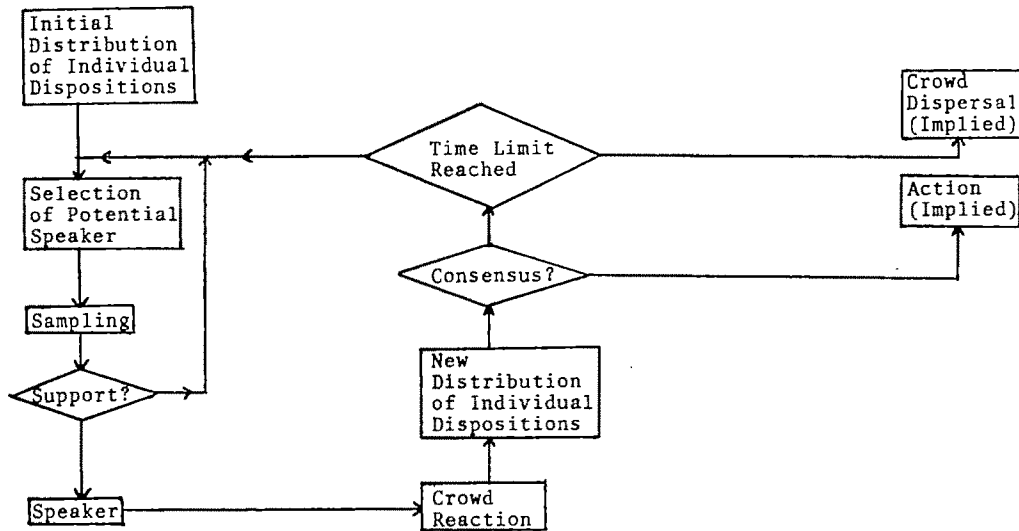


Figure 1. Schematic of Crowd Consensus Model

that physical movement is such that all crowd members are equally accessible, the sampling procedure means that all crowd members have an equal likelihood of being sampled.

The sampling is performed in the model by matching, for each sample member, a random number against a probability distribution obtained from the distribution of crowd members on the disposition scale. What is actually sampled are dispositions; as long as there are individuals having a particular disposition, there is a non-zero probability that that particular disposition will be included in the potential speaker's sample. In addition, sampling is with replacement.⁸ By sampling individual opinions, the potential speaker is testing whether his idea might be well received by the crowd as a whole.⁹

⁸ This raises the possibility that a sample will include a particular disposition more than once, even when that disposition is held by only one individual in the crowd; more so, the possibility exists in the model that the potential speaker, perhaps the only individual holding a particular disposition, might include that disposition (i.e., include himself) in his sample. For those who see this sampling with replacement as unrealistic or even absurd, we offer a rationale: first, the opinion of a particular crowd member might be accorded more weight than that of some other member; and second, a potential speaker might be sufficiently sure of his own position to accord it extra weight in deciding whether the sample is providing support for his position.

⁹ This is in accordance with Berk's (1974) notion that the appearance of support reduces the degree of

The model specifies three criteria for judging whether the sample provides sufficient support for the position of the potential speaker. Support is present if at least one criterion is fulfilled. They are: (1) at least one member of the sample has the same disposition as the potential speaker;¹⁰ (2) at least two members of the sample are not more than one scale position away from the potential speaker; (3) at least three in the sample are not more than two positions away from the disposition of the potential speaker. These criteria are somewhat arbitrary, but some support for these, or similar criteria, can be inferred from the small groups literature.

If the potential speaker's sample does not meet any one of the three criteria, a new potential speaker and new sample are chosen, and the number of cycles is increased by one, so long as the maximum number of cycles (the time limit) has not been reached. If the sample does meet one of the criteria for support of the potential speaker's position, the potential speaker becomes a *speaker*, and the second pro-

risk involved for the individual and increases the likelihood of a favorable outcome. This idea is similar to the diffusion of responsibility explanation of the risky shift. See Pruitt (1971) for a discussion.

¹⁰ Asch's (1951) experimentation indicates that the existence of one true partner permits one to act on his own beliefs and that increasing the size of the dissenting majority has little effect.

cess in a cycle is begun. Through means unspecified in the model, the speaker is able to make his views known to the other members of the crowd and seeks to influence others concerning the correctness or desirability of his position as a *collective* course of action. Because of our initial assumptions, the speaker can communicate his position equally well to all members of the crowd.

Members of the crowd then react individually to the speaker's message. Individuals whose positions are different from that of the speaker either maintain their position or shift their position toward the speaker's position on the ten-point scale. The model does not allow shifting away from the speaker's position.

We assume that the probability of shifting one position on the ten-point scale is constant, i.e., not dependent upon one's position on the scale or on individual suggestibility.¹¹ The number of positions shifted by an individual in response to the speaker's statement is found by use of the binomial distribution and the generation of random numbers. The maximum amount of shift by an individual is equal to the absolute difference between the individual and speaker positions, and the minimum shift is zero. Thus, the binomial distribution appropriate to finding the amount of an individual shift is determined by the maximum possible shift (which determines the number of binomial trials) and the constant probability of shifting one position.

¹¹ We are aware that certain attitude change theories would predict that subjects who hold more extreme positions are less likely to change their opinions in response to a communication. Social judgment theory (see, e.g., Sherif and Sherif, 1967) suggests that not only would a person in an extreme position be unlikely to change his opinion in the direction of a widely disparate suggestion, but might in fact shift *away* from the communication. For purposes of simplifying the model, because alternative explanations have been proposed for data relating extremity of opinion with resistance to change (see Keisler et al., 1969:238-301 for a critique of social judgment theory), and because even social judgment theory suggests that unstructured situations, as in crowds, widen the range of communication which can produce attitude change, we chose neither to permit the probability of shifting to vary with position on the scale of opinions, nor to permit shifting away from the speaker's position. These may be considered in a subsequent model.

The cumulative result of individual shifting in response to a speaker is a new frequency distribution of the crowd with respect to the ten-point scale of individual dispositions. We wish to know whether this new frequency distribution indicates that the individual crowd members have reached a degree of consensus concerning some course of collective action. If consensus has not yet been reached by the crowd, the model repeats the cycle of finding a potential speaker, potential speaker sampling, speechmaking and individual shifting, as long as the time (number of cycles) allowed for the overall process has not already been reached.

In the model we have arbitrarily stipulated two criteria, with at least one to be satisfied, for determining whether consensus has been achieved.¹² Both criteria involve the crude mode of the frequency distribution of individual dispositions. The first requires that the modal position on the ten-point scale contain at least 75 percent of the crowd members. The second is concerned with the amount of variability about the modal position. If at least 95 percent of the crowd is contained in the three scale positions (mode - 1, mode and mode + 1), this second criterion is met. With satisfaction of either criterion, the modal position is considered the consensus position reached by the crowd.¹³

Consensus for a course of collective action for the crowd is at least the reduction in variability of opinion about what is appropriate to the situation. This does not require total agreement among all members. While referring to consensus in crowds we recognize, as argued and demonstrated by Stark et al. (1974), that

¹² Consensus is rarely defined in a precise manner. Berk (1974:364n) says consensus "does not mean that everyone will engage in exactly the same actions. It means that a certain range of activities will be defined as acceptable and other activities defined as unacceptable." Our operational definition is consistent with this.

¹³ In those situations where the distributions of crowd member positions or opinions have more than one mode of equal frequency, consensus has not emerged even if the distribution otherwise satisfies the second criterion. Such situations (at least equally bimodal) indicate conflict and a lack of a clear-cut consensus and, therefore, require further "deliberation" by the crowd.

crowds involve "interactive, multiple and differentiated behaviors" (Stark et al., 1974:866). Berk and Aldrich (1972) also have clearly shown the heterogeneity of motives and behaviors present in the crowd. We suggest, however, that the range of behaviors has been narrowed through the process of interaction. In fact, if behavior is outside this narrowed range, it is not considered a part of crowd behavior. The criteria reflect this argument and, therefore, are not totally arbitrary. Each criterion operationally defines consensus as reduction in variability, the first in terms of the degree of modality and the second in terms of grouping of individual positions in the vicinity of the modal position.

Parameter Values

In the conduct of the experiment with our computer simulation model, we have allowed three parameters to vary from experiment to experiment: initial distribution of dispositions to action, the probability (PSPK) governing the set of binomial probabilities used to choose potential speakers, and the probability (PSHFT) governing the shifting of individual dispositions toward the speaker's position.

Five initial distributions were used: rectangular, U-shaped, "normal," skewed left and skewed right. Each distribution is a relative frequency distribution, based on the incipient crowd having 100 members. We required that no position in any of the distributions be initially empty (i.e., less than .01); thus the normal distribution is a slight modification of the binomial probability distribution for nine trials with $p = .5$. The U-shaped distribution is the normal distribution split in half and reversed. The two skewed distributions are reversals of each other; their basis was arbitrary. The rectangular distribution is self-explanatory.

The probability governing the choice of the potential speaker had one of four possible values: .6, .7, .8 and .9. The higher this probability, the higher the probability of the potential speaker coming from an extreme position.

The potential speaker who gained sufficient support from his sample of five indi-

viduals became a speaker, and the other individuals in the crowd react to his suggestion by shifting their positions. Individual shifting was governed by a binomial process in which the probability of shifting one position on the ten-point scale was invariant among the members of the crowd. This probability took values of .05, .10, .20 and .25. Increased shifting probability indicated an increased suggestibility for crowd members to the message of the speaker.

Thus, our experiment involved 80 sets of conditions of the model: five distributions \times four potential speaker probabilities \times four shifting probabilities. Within each set of conditions, 20 runs were performed. The 20 runs constitute a random sampling of results for the given parameters.

Finally, the maximum time allowed in each run for the crowd to reach consensus is 50 cycles. As indicated, a new cycle begins with the appearance of a new potential speaker. Given this time limit, some crowds would not reach consensus, while those that did would require varying numbers of potential speakers.

Results

Three independent variables related to incipient crowds were allowed to vary in the model as discussed above. Our results are presented in relation to the variation in initial distribution, value of PSPK, and value of PSHFT. No statistical tests are presented.¹⁴

Under each experimental condition, 20 independent runs of the simulation model were performed; therefore, a maximum of 20 crowds could achieve the stipulated conditions for consensus within the time limit of 50 cycles. Table 1 gives the results in which consensus is reached in the allotted time.

It appears that the least suggestible crowds (PSHFT = .05) were least likely to reach consensus within the allotted time, but the difficulty varied with the combination of initial distribution and extremity of potential speaker (PSPK). In general, as

¹⁴ We did perform some statistical tests which confirmed the significant interactions among the independent variables which seem apparent upon examination.

Table 1. Number of Runs (Maximum=20) in Which Consensus Is Reached within 50 Cycles ^a

Distribution	PSPK ^b	PSHFT ^b			
		.05	.10	.20	.25
Normal (N)	.6	20	20	20	20
	.7	16	20	20	20
	.8	18	20	20	20
	.9	11	20	20	20
U-shaped (U)	.6	0	20	20	20
	.7	2	20	20	20
	.8	6	20	20	20
	.9	2	20	20	20
Rectangular (R)	.6	9	20	20	20
	.7	9	20	20	20
	.8	11	20	20	20
	.9	10	20	20	20
Skewed Left (SL)	.6	19	20	20	20
	.7	19	20	20	20
	.8	20	20	20	20
	.9	20	20	20	20
Skewed Right (SR)	.6	10	20	20	20
	.7	5	19	20	20
	.8	0	18	20	20
	.9	0	10	20	20

^a One cycle is equivalent to one potential speaker.

^b PSPK refers to the probability governing choice of the potential speaker. PSHFT refers to the probability of shifting one position on the ten-point scale.

the average potential speaker diverged from the majority of the crowd, it became more difficult for the crowd members to achieve consensus within the time limit. This interaction notion is strengthened by the decreasing number reaching consensus when the crowd is characterized by a skewed right distribution (66 percent of the crowd in positions 2, 3 and 4) and suggestibility is increased (PSHFT = .10). On the other hand, suggestibility when characterized by PSHFT greater than .05 (except with the skewed right distribution and PSHFT = .10) is always sufficient to overcome the inhibiting factors of initial distribution and average position of the potential speaker indicated by PSPK. That is, when the crowd is more suggestible, it will react more favorably to each speaker and thus be more likely to reach consensus and reach it more rapidly.

There are two ways in which we can measure the time required to reach consensus: the number of potential speakers and the number of speakers. Since the effects of the two are so similar, only data relating to the latter are reported. Table 2

shows the mean numbers of speakers for those runs in which consensus was reached. Holding initial distribution and PSPK constant (reading across), it is clear that increasing suggestibility, as measured by PSHFT, does decrease markedly the time necessary for consensus to be achieved in every case, although the amount of decrease varies among combinations of initial distribution and PSPK.

When we hold constant the initial distribution and shifting probability (reading down within blocks), the effect of increasing extremity of the average speaker (measured by PSPK) on rapidity of reaching a consensus is problematical. No single pattern is evident from column to column and from block to block. What seems apparent is that the effect of increasing PSPK on the mean number of speakers depends upon the initial distribution of the crowd. Leaving the exceptions unnoted, the effect of increasing PSPK in conjunction with either the normal or skewed right distributions is an increasing number of speakers; in conjunction with the U-shaped distribution, the result is a

Table 2. Mean Number of Speakers

Distribution	PSPK	PSHFT			
		.05	.10	.20	.25
Normal (N)	.6	28.00	14.45	7.25	6.25
	.7	30.62	15.50	7.80	5.80
	.8	33.67	15.95	7.85	6.00
	.9	29.82	16.00	8.80	6.75
U-shaped (U)	.6	25.30	11.65	9.65
	.7	43.00	24.70	12.75	9.60
	.8	41.17	23.90	11.70	9.40
	.9	48.00	23.50	12.05	8.85
Rectangular (R)	.6	40.33	21.80	10.45	8.20
	.7	41.00	22.65	10.44	8.00
	.8	39.64	22.40	10.65	8.25
	.9	41.00	22.15	11.20	8.00
Skewed Left (SL)	.6	34.26	20.10	8.25	6.90
	.7	33.68	17.25	8.35	6.15
	.8	30.55	14.75	7.40	5.90
	.9	31.70	17.00	7.40	6.60
Skewed Right (SR)	.6	34.40	21.40	9.10	7.10
	.7	34.00	20.05	9.65	7.95
	.8	21.67	9.55	8.00
	.9	20.90	10.80	7.90

decrease in mean number of speakers. The pattern in the relationship between PSPK and the number of speakers is U-shaped when the initial distribution is skewed left. With the initial distribution rectangular, no relationship between PSPK and number of speakers is evident.

In order to examine the effect of the initial distribution on time required to reach consensus when controlling PSPK and PSHFT, we ranked from lowest to highest the mean numbers of speakers under constant conditions and indicated these sets of rankings in Table 3. For example, from Table 2 we get the following values when PSPK = .6 and PSHFT = .25: 6.25 for the normal distribution (N); 9.65 for U; 8.20 for R; 6.90 for SL; and 7.10 for SR. Ranking these from lowest to highest, we get the following rank set designated A in Table 3: N, SL, SR, R, U. Under three sets of conditions, none of the twenty runs reached consensus; a rank of 5 was assigned to these non-consensus conditions.

Table 3 shows only four different sets of ranks characterizing the data, with two rank sets unique to particular conditions. While there is little difference between

rank set A and rank set D, the presence of sets B and C suggests an interaction among the conditions more complex than if only sets A and D appeared in their respective lines of the table.

Table 4 presents the mean consensus values of those runs reaching consensus under each of the experimental conditions. The modal position when consensus was achieved was regarded as the consen-

Table 3. Effect of Initial Distribution on Mean Number of Speakers Needed to Reach Consensus

PSPK	PSHFT			
	.05	.10	.20	.25
.6	A	A	A	A
.7	A	A	A	A
.8	C	D	D	D
.9	B	A	D	D

Rank Set	Order from Lowest to Highest*
A	N, SL, SR, R, U
B	N, SL, R, U, SR
C	SL, N, R, U, SR
D	SL, N, SR, R, U

* In those cases where no consensus was reached, the rank was assumed to be highest (see text).

Table 4. Mean Consensus Values

Distribution	PSPK	PSHFT			
		.05	.10	.20	.25
Normal (N)	.6	6.05	6.00	6.15	6.15
	.7	6.88	6.75	6.75	6.70
	.8	7.44	7.55	7.15	7.20
	.9	8.00	8.05	8.05	8.00
U-shaped (U)	.6	6.40	6.40	6.50
	.7	7.00	7.05	7.25	7.35
	.8	8.00	7.95	8.15	7.90
	.9	9.00	9.00	8.90	8.95
Rectangular (R)	.6	6.11	6.30	6.15	6.35
	.7	6.89	7.00	6.95	6.85
	.8	7.91	7.80	7.70	7.85
	.9	8.80	8.90	8.85	8.65
Skewed Left (SL)	.6	6.95	6.80	6.80	6.60
	.7	7.21	7.45	7.30	7.40
	.8	8.00	8.00	8.10	7.95
	.9	9.00	8.90	8.75	8.85
Skewed Right (SR)	.6	5.40	5.75	5.55	5.35
	.7	6.40	6.47	6.35	6.40
	.8	7.17	6.95	7.35
	.9	8.10	8.90	7.95

sus position, and the entries of Table 4 are the mean values of these positions.¹⁵

When initial distribution and suggestibility (PSHFT) are held constant, increasing values of PSPK yield a more extreme average consensus position. This seems intuitively obvious, since increasing values ultimately yield more extreme speakers to whose message the crowd is reacting by shifting their individual positions toward his position.

Suggestibility, as measured by PSHFT values, does not have any particular effect on the consensus reached, holding PSPK and initial distribution constant; however, if we consider only the differences between consensus means for PSHFT = .05 (or .10 when .05 yields no consensus runs) and for PSHFT = .25; i.e., between lowest and highest suggestibility, twelve differences are negative, six are positive and two differences are zero. This means that a crowd with low suggestibility is twice as likely as a crowd with high suggestibility, all else being equal; to arrive at a more extreme consensus position.

¹⁵ None of the model's crowds achieved consensus by fulfilling the first criterion of having at least 75 percent of the members of the modal position although, in a small number of runs, they came close.

For study of the effects of initial distribution on the consensus value reached, we employed a ranking procedure similar to that used in Table 3. These results are shown in Table 5. We were not able in this tabulation to assume anything about the ranks when no consensus was reached (unlike the procedure of Table 3), but we have made use of the similarities of rank sets.

We note two things about the information in Table 5. First is the great similarity between rank sets *a* and *b* (and therefore including *a'* and *b'*); and second is the considerable degree of consistency in rank sets among the varying conditions, since rank set *c* is also quite similar to *a* and *b* except for the absence of consensus runs with an initial skewed right distribution. Altogether, the results seem to indicate that the initial distributions, as portrayed by their means and standard deviations, exert a fairly consistent influence on the consensus position reached.

Some of our results may appear to be obvious consequences of the assumptions and variables included in the model. We certainly should have expected that the shifting probability indicating suggestibility would be inversely related to the rapid-

Table 5. Effect of Initial Distribution on Mean Consensus Reached

PSPK	PSHFT			
	.05	.10	.20	.25
.6	a'	b	b'	b
.7	b	b	b	b
.8	c	b	a	d
.9	c	c	a	a

Rank Set	Order from Least to Most Extreme Consensus
a	SR, N, R, SL, U
a'	Same as a, except U unranked (no consensus)
b	SR, N, R, U, SL
b'	Same as b, except N and R tied
c	N, R, tie between U and SL; SR unranked
d	N, SR, R, U, SL
e	N, SR, R and SL tied, U

ity with which consensus is reached. The resultant direct relationship between average speaker position (indexed by the value of PSPK) and extremity of consensus reached also is not surprising and might be considered obvious from the definition of the model.

The effects of the independent variables in combination generally are not obvious. The results clearly point to interaction among the independent variables. A partial mathematical description of the model provides a means for testing some of the results. Specifically, if the mathematical results match results from the model, we would concede that the outcomes are strictly determined by the structure of the model and, therefore, somewhat trivial. On the other hand, a lack of correspondence between the mathematical results and those of the model would indi-

cate a more interesting situation and confirm the value of this approach.

The partial mathematical description¹⁶ involves two of the independent variables—initial distribution of dispositions in the crowd and the value of PSPK used in determining the positions of potential speakers. The value of PSPK, in conjunction with the binomial distribution, allows the calculation of the probabilities of potential speakers having each of the dispositions on the ten-point scale. Given the rules by which a potential speaker gains support for his position from a random sample of five members of the crowd, thereby becoming a speaker, and the initial distribution of dispositions in the crowd, it is possible to calculate, for potential speakers having each of the ten possible dispositions, the probabilities of gaining support and becoming speakers. The probabilities of potential speakers occurring with particular dispositions, varying with the value of PSPK, and the probabilities of potential speakers with particular dispositions gaining support, varying according to the initial distribution, can then be combined. This produces a set of probabilities, with each the probability of any potential speaker gaining support and becoming a speaker, for the specific PSPK and initial distribution conditions considered together. These probabilities are shown in Table 6.

The higher the probability value, the more likely that a potential speaker will gain support and become a speaker. In this mathematical formulation, we cannot

¹⁶ The procedure for the partial mathematical description and the distributions which constitute the basis for that description, are available from the authors on request.

Table 6. Probability of Any Potential Speaker Gaining Support from a Sample *

PSPK	Initial Distribution				
	Normal (N)	U-shaped (U)	Rectangular (R)	Skewed Left (SL)	Skewed Right (SR)
.6	.8521	.2797	.6869	.8066	.4847
.7	.7231	.4183	.6740	.9010	.3231
.8	.5160	.6032	.6440	.9226	.1975
.9	.2652	.7658	.5857	.8626	.1124

* From the partial mathematical descriptions; see footnote 16.

include the effects of the shifting probabilities; once a potential speaker becomes a speaker, the crowd members react to the speaker's message and the distribution of dispositions in the crowd changes, with our calculated probabilities then not strictly applicable to the new distribution of the crowd. It seems reasonable to expect, however, that crowds which are more likely *a priori* to provide support for potential speakers would, over the long run, reach consensus more rapidly than those crowds less likely to provide support. Therefore, we expect a strong direct relationship between the probabilities of Table 6 and the rapidity of reaching consensus (from Table 2), at least on some relative basis and controlling for the value of the shifting probability PSHFT.

Accordingly, we ranked the probabilities associated with the twenty conditions of Table 6 in descending order. Ranks also were assigned to the mean number of speakers under the same twenty conditions in each column of Table 2. This yielded four different rank orderings for the rapidity of reaching consensus under the PSPK-initial distribution conditions, controlling for the PSHFT value. The degree of relationship between the theoretical ranking (from Table 6) with each of the sets of ranks from Table 2 was then measured with Kendall's τ (τ). The rank-order correlations are all positive but are decidedly weak, ranging from $\tau = .258$ when PSHFT is .10 to $\tau = .383$ for a PSHFT of .05 (with a PSHFT of .20, $\tau = .338$ and $\tau = .318$ with a PSHFT of .25).

We conclude from these comparisons that the results of the model are not trivial outcomes of what was built in. This is not to suggest reification of the model, but that the model provides a useful starting point in a different approach to the study of a difficult phenomenon.

Discussion

We recognize the tentative nature of any conclusions about aggregates of real people in real situations; however if one accepts for the moment the assumptions of the simulation model, which were generally derived from theoretical statements

about real crowds, one can observe the interrelationships among the variables.

Although the distribution of individual dispositions in the aggregate has some effect on the outcome, one can argue from our simulation data that given the "right" conditions crowd behavior will emerge from any aggregate. The initial individual dispositions have no effect on whether consensus for action emerges under conditions of high suggestibility (Table 1). Inferring, as we have, that suggestibility is related to situational ambiguity, then we can argue that when the situation is sufficiently ambiguous, any aggregate, given sufficient milling time, will reach a consensus for crowd action. Under conditions of lower ambiguity, however, the distribution of individual dispositions does have an effect. Aggregates made up of individuals initially disposed toward some relatively extreme action (the skewed left distribution) will almost invariably take action under these conditions, but aggregates with other distributions (e.g., U-shaped and skewed right) rarely reach consensus under conditions of low suggestibility, assuming that leadership tends to be extreme.

A normally distributed grouping is also highly likely to take action regardless of the suggestibility involved. Many aggregates would constitute a normal distribution of those oriented in a particular direction. McPhail and Miller (1973) suggest that the prime determinant of whether individuals assemble at some location where collective behavior takes place is their access to communications about the event. Access to communications is based to some extent on an individual's predispositions; however, to the extent that communication is independent of such predispositions, a likely assemblage would be characterized by a normal distribution of dispositions to behave. These data indicate that this kind of aggregate could readily take on the characteristics of a crowd.

Whether an aggregate develops the consensus necessary for crowd action and the rapidity with which the consensus is developed are determined, according to the output from our model, primarily by the degree of suggestibility in the crowd.

Assuming again that suggestibility is a function of ambiguity, our results lend support to the notion that in highly ambiguous situations groups tend to reach consensus more rapidly than in less ambiguous situations. The degree of suggestibility, however, has little effect on the extremity of the consensus that emerges, a result that diverges from the notions of Shibutani (1966) concerning the content of rumor. The notion of suggestibility alone implies nothing about the content of the suggestion, although Turner and Killian (1972) and others argue that suggestions must be congruent with the "mood" of the crowd to be effective. Equating our distribution of individual dispositions with crowd "mood," evidence from Table 1 supports this notion when suggestibility is low. Under this condition, when the crowd mood is moderate (normal and skewed right distributions), the crowd is less likely to reach consensus when suggestions come from militant positions ($PSPK = .8, .9$) than when they come from less militant positions ($PSPK = .6, .7$).

Shibutani (1966) implies, however, that the content of rumor also is affected by suggestibility, i.e., content is less plausible and less congruent with general cultural norms under conditions of extreme ambiguity. If we assume that there is some relationship between his notion of plausibility and our notion of extremity, then our results are contradictory to Shibutani's idea. As indicated in the previous section, there is some evidence that consensus is less extreme under conditions of high suggestibility. Obviously this requires further research.

Modifications of the Model

We argued in the formulation of our model that suggestibility varies with the ambiguity of the situation and that individual differences in the willingness to accept normally unacceptable information could be ignored. We propose to examine whether individual differences in suggestibility within a crowd produce markedly different results from those presented here. This requires that individuals in the crowd be randomly assigned values for

PSHFT, the probability of shifting one position, with the means for the individuals equal to the respective constant values of PSHFT used in the present case.

This will aid us in formulating a more complex model, and we wish to present a bare outline of that model. An important aspect of the more complex model will be that physical space will be explicit, with the space (and distance) affecting communication and the physical movement of individuals. In our present model, the structures and processes through which individuals are influenced to change their preferred course of action are largely implicit. We believe, along with Berk (1974), that small groups are formed within the crowd for purposes of discussing the alternative possible actions and that consensus will emerge (a) if the groups achieve some consensus internally and (b) if the groups can reduce the variability between them. One means by which small groups can achieve consensus internally is by reduction of size; individuals who find their group's position incompatible with their own may move physically to find a more compatible group or leave the field of action altogether. Moreover, the group in which consensus is achieved may find it easier to change its position, as a group, when attempting to settle differences in position with other groups. This more complex model allows the occurrence of more than one potential speaker (and possible speaker) at a given time; this is not possible in the present model.

While the explicit inclusion of physical space and small group processes may not appear to portend a drastic change in the model, it does require that a considerable number of additional assumptions be made about connections among elements in the model. An arithmetic increase in the number of concepts to be dealt with creates a geometric, or even exponential increase in the number of assumptions and interconnections in the model. Making these assumptions and interconnections explicit is one of the great advantages of computer simulation, since this increases the chances of developing a coherent and viable theory.

Our reasons for discussing possible modifications of the present model have

been two-fold. The first is to emphasize that our present model is simple, the result of considerable abstraction from real situations. Second, we wish to indicate to those interested in such an approach one possible direction in which the approach might lead in the study of collective behavior.

APPENDIX

We start with N individuals distributed on a ten-point discrete scale ($D:d=1, 2, \dots, 10$) according to their dispositions to act, with 1 the least extreme and 10 the most extreme position.

Choosing the Potential Speaker

Using a parameter PSPK and the binomial distribution for nine trials, each position on the ten-point scale is assigned a probability of the potential speaker coming from that position d . The probability of the potential speaker having disposition d is

$$\Pr(D=d) = \binom{9}{d-1} (\text{PSPK})^{d-1} (1-\text{PSPK})^{9-(d-1)}$$

for all d . A cumulative distribution of the ordered probabilities is then found. This is all done once at the beginning of the run of the model. A random number R ($0 \leq R \leq 1$) is generated and matched against the cumulative probability distribution, with the position d^* of the potential speaker determined by where the random number falls on the cumulative distribution. If the position d^* is unoccupied by any of the N individuals, a search for a new potential speaker is begun by generating a different R .

Choosing the Potential Speaker's Sample

Given a potential speaker having disposition d^* , that potential speaker randomly samples (with replacement) n individuals from the total incipient crowd of N individuals. Since the N persons are classified according to dispositions d , the sampling is of dispositions.

The distribution of the N individuals (including the potential speaker) on the

ten-point scale is converted to a cumulated relative frequency distribution. A random number for each of the n sample members is then generated and matched against the cumulated distribution. Each of the n sample members is thus an individual disposition from the group of N individual dispositions.

Individual Shifting

Given that we have a speaker having disposition d^* , individuals in the crowd respond to the speaker's message by variably shifting their positions on the ten-point scale. The amount of shift for each individual is governed by a binomial distribution.

The probability of shifting one place on the ten-point scale is given by PSHFT and is constant for all N individuals. Since all shifting is assumed to be toward the speaker's position, the maximum number of scale places that might be shifted from position d to position d^* is $d - d^* = S$. For example, if the speaker is at position 8, an individual at position 3 can shift 0, 1, 2, 3, 4 or 5 positions, ending up at positions 3, 4, 5, 6, 7 or 8, respectively.

The probability of shifting s number of positions is then

$$\Pr(s) = \binom{S}{s} (\text{PSHFT})^s (1-\text{PSHFT})^{S-s}$$

For each value of S a different probability distribution is generated and cumulated. Each individual is assigned a random number each time a speaker appears; comparison between the random number and the appropriate cumulated probability distribution yields the appropriate amount of shift s and thus the individual's new position $d + s$.

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SOCIAL EXCHANGE: PREDICTING TRANSACTIONAL OUTCOMES IN FIVE-EVENT, FOUR-PERSON SYSTEMS *

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This paper reports an experimental test of Coleman's theory of social exchange. The theory utilizes information on interest (X) and initial control (C) to make predictions concerning the value of events (V), the resources of persons (R), and the pattern of control at equilibrium (C). The experimental paradigm, which incorporates three distinct five-event, four-person exchange systems, shows that the theory's predictions are highly accurate for V, R and C*. Competitive goodness-of-fit tests demonstrate that Coleman's theory surpasses alternative theories based on less input information. The findings substantiate previous tests of Coleman's theory conducted on smaller exchange systems.*

In an earlier paper, Michener, Cohen and Sørensen (1975) reported the first experimental test of Coleman's (1972) theory of social exchange. That test, while limited to small exchange systems involving four events and three persons, offered very encouraging results and showed that Coleman's theory was more accurate than several competing models.

Coleman's theory is intended to be highly general (i.e., to apply to any pure exchange system of m events and n persons). The theory's adequacy in this respect cannot be accepted on assumption, however, because social systems grow in complexity as they increase in size—a fact which makes the task of prediction more formidable. The present paper, therefore, reports a further experimental test of Coleman's theory, utilizing five-event, four-person exchange systems. Since these systems are virtually twice the scale and complexity

of those in the earlier test, data from the present study constitute evidence on the generality of Coleman's framework. Moreover, the data provide a basis for additional tests against alternative models.

Before reviewing Coleman's theory *per se*, it is useful to consider the general issues posed by exchange systems. In his classic statement on bilateral trade nearly a century ago, Edgeworth (1881) identified the key concepts for any exchange system and expressed the problem in formal terms. Any market system includes *persons*, who conduct transactions with one another, and it also includes *events*, which are commodities or services traded by the persons. Edgeworth noted that prior to the occurrence of any transactions, each person *controls* some portion of the tradeable events; additionally, each person has an *interest* in some or all of the events in the system.

To understand the issues posed by such a system, consider a concrete example. Assume there are four persons (i.e., Alex, Bernard, George and Dan) participating in an agricultural market consisting of five tradeable events (i.e., tomatoes, onions, plums, lemons and avocados). Assume that the system includes a total of 1,000 tomatoes, 300 onions, 200 plums, 800 lemons, and 400 avocados, and that control of these events is distributed as depicted in Table 1.

The control matrix (C matrix) in Table

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Table 1. A Simple Agricultural Market

Interest Matrix (X)						Control Matrix (C)				
	tomatoes	onions	plums	lemons	avocados		Alex	Bernard	George	Dan
Alex	0	.3	.1	0	.6	tomatoes	.6	.2	0	.2
Bernard	.8	.1	.1	0	0	onions	.1	.7	.1	.1
George	.3	0	0	.4	.3	plums	.3	0	.4	.3
Dan	.3	0	0	.5	.2	lemons	.5	.5	0	0
						avocados	0	.6	.2	.2

1 indicates that, before any trades occur, Alex controls .6 (i.e., 600) of the tomatoes, .1 (30) of the onions, .3 (60) of the plums, .5 (400) of the lemons and none of the avocados. Bernard controls .2 (200) of the tomatoes, .7 (210) of the onions, none of the plums, .5 (400) of the lemons and .6 (240) of the avocados. George controls no tomatoes, .1 (30) onions, .4 (80) plums, no lemons, and .2 (80) avocados. Dan controls the rest. Note that control of any event sums to 1.0; thus, each person's control over an event represents that proportion of the commodity he owns (ranging from 0.0 to 1.0).

In addition to exercising control over the various events, persons also have interest in these events. The interest matrix (X matrix) in Table 1 details the extent to which each person has some interest in each of the four events. Interest is expressed in terms of the proportion of a person's total interest ranging from 0.0 to 1.0; that is, a given person's interest sums to 1.0 across all events. This conception of interest is analogous to a budget, in which a person allocates his purchasing power across various events in accordance with their importance to him. The more interest a person has in a given event, the more willing he is to commit his resources to obtain control over the event.

As shown in Table 1, Alex has no interest in obtaining tomatoes or lemons, but he does have a .3 interest in onions, a .1 interest in plums and a .6 interest in avocados. In other words, Alex has a large interest in avocados and a lesser interest in onions and plums. Bernard has a strong interest in tomatoes (.8) and a lesser interest in onions (.1) and plums (.1). George has approximately equal interest in tomatoes (.3), lemons (.4) and avocados (.3).

Dan has a prime interest in lemons (.5) and a minor interest in tomatoes (.3) and avocados (.2).

One might expect this situation to foster a series of trades, because the four persons don't have what they want and don't want what they have. The market system is in disequilibrium, and all persons can improve their positions by exchanging commodities with one another.

This simple example poses an important research question: *After all trades have occurred, who will control what?* That is, how much of the various events will be owned by each of the traders after all transactions? As emphasized by several writers (Edgeworth, 1881; Homans, 1961; Newman, 1965; Rapoport, 1970), this is the fundamental question to be answered by any exchange theory.

A second question posed by the example is: *What is the value of each of the events in the exchange system?* In other words, what is the relative "price" of each event? Since the traders have greater interest in some events than in others, one may ask which of the various commodities will command the highest price. Although of lesser importance than the issue of control after trading, this question has interested some theorists (Newman, 1965; Rapoport, 1970) and must be addressed by any general theory of exchange.

A third question is: *What is the relative power of each of the persons in the exchange system?* Since the traders begin by controlling different amounts of the various events and since these events differ in value, it follows that some traders will enjoy a more advantageous position than others. Various theorists (Thibaut and Kelley, 1959; Tannenbaum, 1962; Blau, 1964) have addressed the origins of power in so-

cial interaction, and it endures as an important issue.

In order to construct predictive models, theorists as far back as Edgeworth have found it useful to make restrictive assumptions regarding the exchange processes. One common assumption is that all persons in the system have full information about interests and control over events before trades occur. In the context of the agricultural example, this would mean that Alex, Bernard, George and Dan each know what the others want and what the others control. Another typical assumption is that all traders are guided by rational self-interest and seek to increase their gains through exchange; thus, persons will enter a trade only if it yields an increment in utility. A third assumption is that persons' interests are stable and not subject to change midway through trading. A fourth assumption is that events are divisible. In the previous example, the events meet this assumption; the event "tomato," for instance, can be divided to one part in 1,000 (since there are 1,000 tomatoes in the exchange system), while the event "onion" can be divided to one part in 300, etc.

While these assumptions are restrictive, they typify those made by many exchange theorists (e.g., Edgeworth, 1881; Newman, 1965; Rapoport, 1970; Coleman, 1972). Unfortunately, the stringency of these assumptions has hampered empirical assessment of exchange theory predictions, for virtually no markets found in natural settings meet all these conditions. Even relatively "pure" markets such as the commodity markets or the stock exchanges fail to satisfy the assumptions of full information and stability of interest.

It is possible, however, to *create* an experimental situation that meets the assumptions of exchange theory. In earlier work, Michener, Cohen and Sørensen (1975) established an experimental market paradigm involving four-event, three-person systems. The present research extends this paradigm to five-event, four-person systems. Systems of this size are substantially more complex than those used in the earlier research, and they afford a good basis for further tests of Coleman's theory.

The remainder of this paper first will describe the experimental procedures and report the findings. Next, it will detail Coleman's (1972) theory of social exchange and derive the relevant predictions. Finally, it will test goodness-of-fit to determine how well the theory's predictions match the empirical observations.

METHOD

Procedures

Subjects in the present study were 144 male undergraduates at the University of Wisconsin. For their participation, subjects received \$1.00 plus whatever money they could earn during the experiment. The subjects comprised 36 four-man groups, which were the units of analysis. Twelve groups were randomly assigned to each of three experimental treatments (termed Configuration 1, Configuration 2 and Configuration 3).

After arriving for the experiment, subjects entered a room containing a large square table. The four positions around the table were designated by name cards (Alpha, Beta, Gamma, Delta), and the experimenter randomly assigned subjects to these roles. During the experiment, subjects were free to talk with one another. Use of the Alpha, Beta, Gamma and Delta labels, rather than the subjects' own names, insured relatively impersonal and business-like interaction.

Seated around the table, subjects read an instruction sheet that described the basic situation. This explained that subjects would be trading colored chips, which constituted the exchangeable events in the system. These colored poker chips (red, white, blue, yellow and green) appeared on the table in full view of all subjects. In total, there were 500 chips, 100 of each color.

Before the trading began, each subject received another sheet of paper indicating the traders' interests and initial control over chips. Each person was staked to a given number of chips, which were piled directly in front of him on the table. The actual number of chips received by a sub-

ject depended both on the role he occupied (Alpha, Beta, Gamma or Delta) and on the experimental treatment (Configuration 1, 2 or 3) to which his group had been randomly assigned. The patterns of interest and initial control for the three experimental treatments are depicted in Table 2. For example, in those groups assigned to Configuration 2, person Alpha initially controlled .6 of the red chips, .1 of the white chips, .3 of the blue chips, .5 of the yellow chips and none of the green chips. In other words, Alpha had 60 red, 10 white, 30 blue, 50 yellow and 0 green chips. Beta had 20 red, 70 white, 0 blue, 50 yellow and 60 green chips. Gamma had 0 red, 10 white, 40 blue, 0 yellow and 20 green chips. Delta had 20 red, 10 white, 30 blue, 0 yellow and 20 green chips. Subjects understood that although some traders controlled more chips than others, they were to do their best with whatever they had.

Each subject received a sheet of instructions indicating how much interest the various traders had in each color chip. Interest was operationalized in terms of tenths of a

cent, so that subjects could earn money by accumulating colored chips in which they had some interest. For example, in Configuration 2, person Alpha had .6 interest in green chips and he therefore received .6¢ for each green chip he controlled at the end of all exchanges. Thus, if he controlled all 100 green chips when trading was completed, he would earn 60¢ from these alone. Similar payoffs could be earned for the other chips, depending on the degree of interest. This payment procedure meant that the last chip gained (of a given color) was worth as much monetarily as the first chip gained (i.e., no diminishing marginal payoff).

Note that the patterns of interest (X) and initial control (C) in Configuration 2 differ from those in Configuration 1 and Configuration 3. Configuration 2 depicts the same pattern as the earlier agricultural example with Alex, Bernard, George and Dan (compare Configuration 2 in Table 2 with Table 1). Configurations 1 and 3 are new. All three configurations were randomly sampled from a (hypothetically infinite) population of possible interest and

Table 2. Experimental Configurations 1, 2 and 3

	Interest Matrices (X)					Control Matrices (C)			
	Red	White	Blue	Yellow	Green	Alpha	Beta	Gamma	Delta
Configuration 1									
Alpha	0	.9	.1	0	0	Red	.6	0	.4
Beta	.3	.5	0	0	.2	White	.1	0	.1
Gamma	.3	0	0	.3	.4	Blue	0	.9	0
Delta	0	.8	0	.2	0	Yellow	0	.6	0
						Green	.4	0	.6
Configuration 2									
Alpha	0	.3	.1	0	.6	Red	.6	.2	.2
Beta	.8	.1	.1	0	0	White	.1	.7	.1
Gamma	.3	0	0	.4	.3	Blue	.3	0	.3
Delta	.3	0	0	.5	.2	Yellow	.5	.5	0
						Green	0	.6	.2
Configuration 3									
Alpha	.6	.2	.2	0	0	Red	.1	.9	0
Beta	.2	0	.4	0	.4	White	.1	.5	.3
Gamma	.9	0	.1	0	0	Blue	.8	0	.2
Delta	0	.2	0	.5	.3	Yellow	.4	.2	.2
						Green	.6	.1	0

control patterns for five-event, four-person systems. The only restriction applied to the sampling of configuration matrices involved rounding off entries in the matrices to the first decimal point. Thus, subjects exerted initial control in terms of tens (0, 10, 20, 30, . . . , 90, 100) and had interests expressed in terms of tenths (0, .1, .2, .3, . . . , .9, 1.0). Since Configurations 1, 2 and 3 differ among themselves, one would expect that the equilibrium pattern of control (after all trading) will be different for the configurations. This adds complexity, of course, but a *general* theory of social exchange must offer predictions for any possible configuration.

Having received information on interest and control for their configuration, the subjects began trading after the experimenter left the room. The experimenter was blind to all theoretical predictions, and his absence from the room precluded experimenter bias.

Subjects were free to negotiate any trades they wished. The only stipulation was that all deals were to be two-way (i.e., between two traders); no three-way or four-way deals were permitted, although subjects could achieve complex deals by a series of two-way trades. The various groups (which could be monitored over an intercom system) differed greatly in the way they conducted exchanges. Some were amicable and jocular; others were tough and sarcastic. Some made only a few trades (involving large numbers of chips); others racked up more than 25 exchanges (involving only a few chips each time). The order and sequencing of trades differed from group to group and appeared to follow no standard pattern. In several cases, a subject decided to conduct an auction for his chips; in other groups, subjects utilized complex mathematical calculations to determine what they considered a proper rate of exchange. When a transaction occurred, subjects literally exchanged chips. Thus, hundreds of chips changed hands during the experimental session. Equilibrium was reached when all subjects decided that they no longer wanted to trade or found that they couldn't promote any further trades with the other subjects. At

that point, they informed the experimenter via the intercom. He reentered the room, carefully tallied who possessed which chips, and administered a postexperimental questionnaire to assess the subjects' perceptions of the trading.

Fixed Parameters

As indicated above, exchange theories in general (and Coleman's theory in particular) are predicated on a set of explicit assumptions. At the operational level, these assumptions must be treated as fixed parameters. Care was taken in the present study to set fixed parameters in accord with theoretical assumptions.

Self-interest motivational orientation. Perhaps the most critical (and limiting) assumption of exchange theory is that each trader pursues his own self-interest. This involves neither a moral assertion nor a presumption of plutocracy; rather, as Newman (1965) has emphasized, it is merely an assumption that traders use certain decision rules in deciding when to favor one potential trade over another. Meeker (1971) places self-interest in perspective by noting that persons can adopt any of several motivational orientations in face-to-face interaction. In addition to self-interest, these include altruism, competition, group gain, equity, status congruity, reciprocity, etc. Consistent with the assumptions of Coleman's theory (1972; 1973: ch. 3), the present study instilled a self-interest motivational set and discouraged the adoption of other orientations. Written instructions urged a self-interested orientation by including such directives as "get the largest number of chips," "obtain the biggest profit," and "do as well as you can." Since most persons will not automatically adopt a purely self-interested stance in face-to-face interaction, steps were taken to legitimate this orientation. First, subjects were informed they had been randomly assigned to roles (Alpha, Beta, Gamma, Delta); this eliminated status congruence as a criterion for trading. Further, each subject realized that the others were adopting a self-interested orientation, and this legitimated his own self-interested stance and undermined any collective or group-gain orientation.

Moreover, the instructions explicitly indicated that traders should not consider themselves in competition with one another; for example, Alpha's success as a trader should be assessed only in terms of his outcomes relative to those of the Alphas in other groups, not relative to those of Beta, Gamma and Delta in his own group. Finally, a pure equity orientation was minimized because the instructions defined "fairness" as "doing as well as you can for yourself." Expressed another way, the instructions espoused a norm of rational self-interest and outlawed other orientations commonly seen in face-to-face interaction.

Subjects' responses to two questions on the postexperimental questionnaire indicate that these instructions accomplished their purpose. One question asked: "How motivated were you to obtain as many valuable chips for yourself as possible?" The other asked: "To what extent were you trying to get the largest profit possible for yourself?" The mean responses on nine-point scales to these two questions were, respectively, $\bar{X}=7.29$ and $\bar{X}=7.57$, which indicate a very high level of self-interested orientation, especially for a face-to-face situation.

Full information. Most exchange theories (including Coleman's) assume that all participants have full information regarding control and interest. This was fixed as a parameter in the present study, and all subjects operated with complete information. Since the colored chips were stacked in front of the subjects on the table and actually changed hands whenever a transaction occurred, all subjects knew who controlled what at all times. Moreover, subjects had a sheet of paper detailing the interest matrix for their particular configuration. This specified each person's interest in each event, assuring that everyone's interests remained stable throughout the trading. A fixed pattern of interest obviates the use of certain power tactics (i.e., withdrawal and demand creation, as detailed in Michener and Suchner, 1972), which would otherwise affect the pattern of trades.

Divisibility of events. The present frame-

work included five events (red, white, blue, yellow and green chips), each of which was divisible to one part to 100. This high degree of divisibility enabled the subjects to operate with substantial precision in establishing exchange rates among the five events.

Outcome Measures

This study incorporates three dependent variables; these parallel the three research questions raised in the introduction. Items on the postexperimental questionnaire measured subjects' perceptions of each trader's resources and each event's value. These measures will be symbolized R_o (for "observed resources") and V_o (for "observed value"). The third dependent variable, the pattern of control over events *after* trading (symbolized C^* , for "observed control at equilibrium"), was determined via direct observation of the chips.

To measure resources, subjects rated each person (including themselves) in terms of four semantic differential scales. These scales were *strong-weak*, *powerful-powerless*, *potent-nonpotent* and *dominant-submissive*. Data from previous research (Michener and Cohen, 1973; Michener, Lawler and Bacharach, 1973) indicate that these scales measure resource position (as opposed to measuring other aspects of power such as aggressiveness or resistance). In the present study, a score was obtained for each person by summing the four items as rated by all four subjects, cubing the sum to sharpen the differences among persons, and then dividing that number by the total score for all persons. This procedure generates a vector of coefficients (R_o) for the persons in the exchange system; these coefficients sum to 1.0. As in past research, these scores prove highly reliable, with a median Cronbach's alpha of .918 (computed across four perceivers for each of four persons perceived).

The value of events (as perceived by subjects) was measured on the postexperimental questionnaire by four semantic differential items: *valuable-worthless*, *important-unimportant*, *sought after-not sought after*, *desirable-undesirable*. Instructions directed each subject to rate each color

chip in terms of its value and importance to all traders within the system and not just to himself. A value score was obtained for each event by summing the four items as rated by all four subjects, cubing the sum to heighten the differences among events, and dividing that total number by the total score for all events. This procedure generates a vector of coefficients (V_o) for the events in the exchange system; the elements in the vector sum to 1.0. These scores prove highly reliable, with a median Cronbach's alpha of .887 (computed across four perceivers for each of five events).

The third dependent variable is C^* , control over events after trading. (Note that the C^* matrix differs from the C matrix, which is the pattern of control before trading.) Since the colored chips changed hands during the trading, the experimenter could simply tally who had what chips when trading was completed. He did this in full view of all subjects and then double-checked for accuracy by reading aloud the amounts he had recorded. Because the subjects were paid according to this count, this procedure provided a measure of C^* that was virtually error-free. Entries in the C^* matrix were computed from the raw data by dividing by 100 the number of chips of each kind held by each subject. Thus, final control of a given event summed to 1.0 over all persons.

DESCRIPTIVE RESULTS

Measures of the three dependent variables (R_o , V_o , C^*) were obtained for each of the 36 four-man groups in the study. From these raw data, mean values for each of the three configurations were computed (12 groups per configuration). Table 3 presents the results (R_o , V_o , C^*) for Configurations 1, 2 and 3.

As might be expected, the results for any given configuration differ from those for the other configurations. The sheer complexity of the empirical data highlights the need for a theory to interpret the findings. The important task, of course, is to predict R_o , V_o and C^* not just for one configuration, but for all three. Coleman's

theory, as will be seen below, is sufficiently general to do this.

COLEMAN'S THEORY OF SOCIAL EXCHANGE

Coleman's (1972; 1973: ch. 3) theory pertains to any exchange system of m events and n persons, where persons have a fixed structure of interest in events (X) and an explicit pattern of initial control over events (C). The theory is based on the same assumptions introduced at the beginning of this paper and established as fixed parameters in the experimental setting. These assumptions include: (1) persons will strive to achieve their self-interest; (2) events are divisible; (3) all persons possess full information concerning the C and X structures.

In Coleman's symbolism, the amount of initial control over event i by person j is represented by c_{ij} , where $0 \leq c_{ij} \leq 1$ and $\sum_j c_{ij} = 1$. Since events are divisible, c_{ij} represents the fraction of i over which person j has full control.

The amount of interest that person j has in event i is represented by x_{ji} , where $0 \leq x_{ji} \leq 1$ and $\sum_i x_{ji} = 1$. That is, x_{ji} represents the fraction of his resources that person j allocates (during trading) toward control over event i , independent of the total size of his resources and independent of the value of event i in the system.

Coleman's theory defines resources for each person in the exchange system as follows:

$$r_j = \sum_{i=1}^m v_i c_{ij} \quad (1)$$

where r_j is the resources held by person j , v_i is the value of event i in the system, and c_{ij} is the initial control that person j has over event i . Expressed in words, equation (1) says that the resources held by person j consist of the sum of initial control he has over all events, each event weighted by its value. Clearly, a person will hold a lot of resources if he has substantial control over a lot of events, provided these events are valuable in the system.

Coleman's theory defines the value of each event as:

$$v_i = \sum_{j=1}^n r_j x_{ji} \quad (2)$$

where v_i is the value of event i in the system, r_j is the resources held by person j , and x_{ji} is the interest of person j in event i . Equation (2) says that the value of an event is the sum of interests in the event, each person's interest weighted by his total resources. This means that an event will be very valuable if many persons have a substantial interest in the event, provided they hold a lot of resources.

Note that resources is defined in terms of value (equation [1]) and that value is defined in terms of resources (equation [2]). At first this may appear circular, but it is possible to achieve a simultaneous solution for these equations by using the fact that the total quantity of resources in the system sums to one (i.e., $\sum_j r_j = 1$) and the value of all events sums to one (i.e., $\sum_i v_i = 1$).

The prime objective of exchange theory is to specify who will control what after trading has been completed—in other words, to predict C^* . Coleman's theory uses c^*_{ij} to symbolize the amount of control over event i that person j has at equilibrium (after trading), and it stipulates the equilibrium condition as:

$$v_i c^*_{ij} = r_j x_{ji}. \quad (3)$$

This equation says the exchange system will attain equilibrium when the amount of resources a person commits to control an event ($r_j x_{ji}$) equals the value of full control over the event times the proportion of control he actually achieves ($v_i c^*_{ij}$). If this condition does not hold for all persons and all events, then at least two persons can benefit from further trading, and further exchange is predicted to occur until the equilibrium condition is reached.

Given information on C and X and equations (1), (2) and (3) above, it is possible to derive predictions regarding R , V and C^* for the three configurations used in the experimental situation. Coleman's pre-

dictions for the outcomes of Configurations 1, 2 and 3 appear in Table 4.

GOODNESS-OF-FIT

How well does Coleman's theory predict the empirically observed outcomes (R_o , V_o and C^*_o) for the three configurations?

It is useful, first, to compare visually the empirical outcomes (Table 3) against the predicted outcomes (Table 4). Note the substantial correspondence between observations and predictions for resources (R) and for value (V). With the exception of one reversal for resources in Configuration 3 and two reversals for value in Configuration 2, Coleman's predictions closely match the data. Even more impressive is the correspondence between empirical observations and the predictions for C^* . The match between observed and predicted coefficients is very close, with the only important reversals being the red chips in Configuration 1 and, possibly, the red chips in Configuration 3. This correspondence appears all the more striking when one remembers that Coleman's theory is making predictions for three different five-event, four-person systems.

Goodness-of-fit for C^*

To obtain a more precise assessment of the theory's predictive capabilities, a test was performed for goodness-of-fit. First, a discrepancy index, d , was defined as the square root of the sum of the squared differences between observed and predicted values, i.e.,

$$d = \sqrt{\sum_{i=1}^m \sum_{j=1}^{n-1} (c^*_{ij_o} - c^*_{ij_p})^2} \quad (4)$$

where $c^*_{ij_o}$ is observed control (after trading) over event i by person j and $c^*_{ij_p}$ is predicted control over event i by person j .¹ This index reflects the discrepancy be-

¹ Computation of the discrepancy index involved summation over m events but over only $n-1$ persons. Since the rows in the C^* matrix are constrained to sum to 1.000, the degrees of freedom in this matrix are limited to $(m)(n-1) = (5)(4-1) = 15$.

tween data and theory; perfect correspondence between observed and predicted outcomes will yield an index score of zero, while poor correspondence will yield a large positive score.

Using Coleman's predictions, discrepancy scores were computed for each of the 36 groups in the experiment. Table 5 presents the mean discrepancy scores for C^* in Configurations 1, 2 and 3. A comparison across configurations reveals that none of the means are equal to zero, indicating some difference between observations and predictions.

An obvious question is: Does Coleman's theory predict the data better than simpler alternative models? This is an important issue because Coleman's theory poses large initial-information requirements; that is, it requires knowledge of both the C and X matrices to make predictions regarding C^* . In many naturally occurring exchange situations, either the C matrix or the X matrix (or both) is unknown or only partially known. If an alternative model requiring less information proves as efficient as Coleman's in predicting outcomes, that model would be preferred.

Three alternative models with low information requirements will be considered here. The first is an elementary baseline model that needs only initial information on m (the number of events) and n (the number of persons). This model predicts that each person will control an equal proportion of each event after trading (that is, $c^*_{ij}=1/n$). Although extremely simple, it can be interpreted in substantive terms as an equalitarian model in which traders are predicted to share all outcomes equally.

A second alternative requires information regarding the initial C matrix as input. It predicts simply that $c^*_{ij}=c_{ij}$. In other words, in the absence of information on X ,

this model takes initial control (C) as the best approximation of final control (C^*).

A third alternative model requires only information concerning interest (X). This elementary model predicts that final control (C^*) is a function solely of interest in events. That is, it predicts that traders will achieve control over an event in proportion to their interest in that event; expressed formally,

$$c^*_{ij}=x_{ji}/\sum_j x_{ji}.$$

To ascertain the predictive adequacy of these three alternative models, discrepancy scores (equation [4]) were computed for each model on the 36 groups in the study. The mean discrepancy scores for these models (i.e., equalitarian, C -only, proportionate interest) are presented in Table 5.

To test Coleman's theory against the three alternatives, the discrepancy scores were analyzed in a 3 (configurations) \times 4 (theories) split-plot factorial analysis of variance (cf. Kirk, 1968:248-83). Each group was nested within one of the three levels of the configuration factor (i.e., 12 groups per configuration). The repeated measures factor (theory) was crossed with configuration.

Results indicate that the difference between theories is statistically significant, $F(1,33)=555.44$, $p<.01$. There is no significant main effect of configuration, $F(2,33)=3.17$, $n.s.$, but the data show a significant configuration \times theory interaction, $F(2,33)=15.27$, $p<.01$, on the discrepancy scores.²

* Because this analysis involved repeated measures over four theories for each of the 36 groups, the Geisser-Greenhouse correction for dependency of observations has been applied to all F -tests reported here. This correction is extremely conservative. Computational procedures are identical to those of a conventional test except that

Table 5. Mean Discrepancy Scores for Final Control (C^*)

Theory	Configuration			Mean across Configurations
	1	2	3	
Coleman	.8523	.6992	.6299	.7272
Equalitarian	1.5049	1.4704	1.5154	1.4969
C -only	2.2701	2.1075	2.1757	2.1844
Proportionate Interest	.8124	.8560	1.3820	1.0168

This interaction results primarily from the differential performance of the proportionate interest model (i.e., poorer performance in Configuration 3).

Separate planned comparisons show that, across the three configurations, Coleman's theory fits the data better than the equalitarian model, $t(33)=20.15$, $p<.01$, better than the C-only model, $t(33)=38.15$, $p<.01$, and better than the proportionate interest model, $t(33)=7.58$, $p<.01$. (The critical values of all tests reported here have been adjusted conservatively to maintain an experimentwise error rate of .01.) These tests show that Coleman's theory is superior to several alternative models having lower information requirements. While Coleman's theory needs more initial information to make predictions regarding C*, its predictions are more accurate than those of the simpler models. This conclusion reinforces that of the earlier experimental test on smaller exchange systems (Michener, Cohen and Sørensen, 1975).

Goodness-of-Fit for V and R

Beyond question, the most important test of Coleman's theory is that involving C*, the prime dependent variable. However, it is possible to conduct ancillary tests of Coleman's theory using the data on value (V) and resources (R). The empirical results (V_o and R_o in Table 3) are based on measures known to be highly reliable and (in the case of the measure for R) to have the intended discriminant validity. These results are closely matched by Coleman's predictions (V_p and R_p in Table 4). With few exceptions, Coleman's theory predicts the rank-ordering of resources and value in all three configura-

tions. A comparison of Tables 3 and 4 reveals only one reversal for resources (Configuration 3) and two reversals for value (both in Configuration 2). Moreover, as shown in Table 6, the mean discrepancy

scores for value, $d = \sqrt{\frac{m-1}{\sum_i (v_{io} - v_{ip})^2}}$, and

for resources, $d = \sqrt{\frac{n-1}{\sum_j (r_{jo} - r_{jp})^2}}$, assume

relatively small magnitudes, indicating a close fit between the empirical results and Coleman's predictions.

As a test of Coleman's predictions for value and resources, discrepancy scores were computed from the data for several alternative models based on less input information (m and n only, C only, X only). Results of comparative tests show that Coleman's predictions are superior in all cases for resources ($p<.01$) and for value ($p<.02$).³

DISCUSSION

The present test, in conjunction with the research on smaller exchange systems reported in Michener, Cohen and Sørensen (1975), provides some empirical support for Coleman's theory. The theory performs nicely in predicting outcomes for the five-event, four-person systems treated here. Presumably, the theory would apply to even larger systems, although the present

³ The sole exception to this statement is an alternative model that treats value as proportional to total interest: $v_i = \sum_j x_{ij} / n$. A test shows that the mean discrepancy score (across configurations) based on predictions from this model does not differ significantly from that based on predictions from Coleman's model, $t(33)=1.26$, *n.s.*

the degrees of freedom are reduced. For a detailed discussion of the Geisser-Greenhouse correction, see Kirk, 1968:142-3.

Table 6. Mean Discrepancy Scores for Value (V) and Resources (R)

Variable	Configuration			Mean across Configurations
	1	2	3	
Value (Coleman)	.1304	.2046	.1503	.1617
Resources (Coleman)	.1882	.1218	.1954	.1685

data do not isolate the limits of applicability.

Beyond the question of size, one must note that Coleman's theory is predicated on several strong assumptions. The present study purposely incorporated these assumptions as fixed parameters, but it remains for future research to determine whether the theory will predict adequately if these parameters are relaxed.

Several assumptions deserve special attention. First, one may ask what happens to the theory's predictive power when traders operate without complete information regarding control (C) and interest (X). Situations of imperfect information give rise to inefficiency; they also provide an opportunity for certain power tactics, such as feigned withdrawal and extension of the power network (Michener and Suchner, 1972). Coleman's theory certainly will be less applicable when traders have only partial information, but the important issue is *how much* less applicable. By experimentally varying the degree of information held by traders, future research could determine the theory's sensitivity to this parameter.

Second, Coleman's theory assumes that persons act in their own self-interest. Empirical researchers recognize that while persons are capable of adopting a self-interested orientation, they can also adopt other motivational sets such as altruism, equity, status congruence, competition, etc. Informal experimentation by the present writers suggests that Coleman's theory is sensitive to relaxation of this parameter and that it loses some of its predictive capacity when subjects adopt other orientations (such as equity). The important question, of course, is *how much* predictive power is lost when traders adopt other motivational sets?

Third, Coleman's treatment of control implies that when a person trades an event, ownership is completely forfeited to another person. This is a reasonable assumption for material goods, but it does not apply readily to other commodities such as information. As Rosen (1966) has observed, a person can trade (or give) in-

formation to another while continuing to "control" it himself. Do the predictions from Coleman's theory apply to commodities having this property or is a distinct theory required?

While issues such as these remain open for future study, the conclusion from the present research is that Coleman's theory performs better than simple alternative models under the given fixed parameters. The theory is an important element in the study of interpersonal exchange, and future experimentation on exchange systems should adopt it as a starting point.

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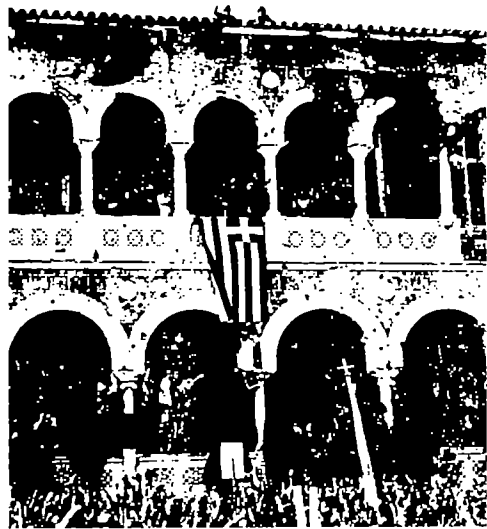
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■ REEVE VANNEMAN (American Perception of Class and Status) is Assistant Professor of Sociology at the University of Illinois, Urbana. His current research includes work on class divisions in the occupational structure and the role of the bureaucracy in Asian stratification. FRED C. PAMPEL will be Assistant Professor of Sociology at the University of Iowa in the coming year. He is presently completing his doctoral dissertation, *Social Indicator Models of Changes in the Status of the Aged*, at the University of Illinois, Urbana.

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■ NORRIS R. JOHNSON (The Emergence of Consensus in Crowds) is Associate Professor of Sociology at the University of Cincinnati. He is presently engaged in experimental studies of collective behavior processes in small groups. WILLIAM E. FEINBERG is Associate Professor of Sociology at the University of Cincinnati. His current research includes work on problems of ratio correlations, density, crowding and human behavior, and computer simulation models of crowds.

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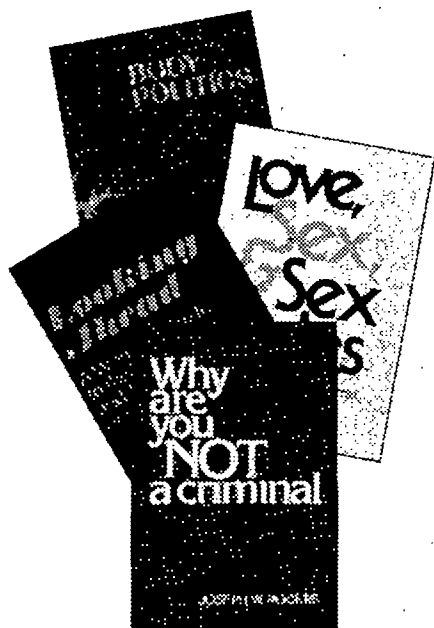
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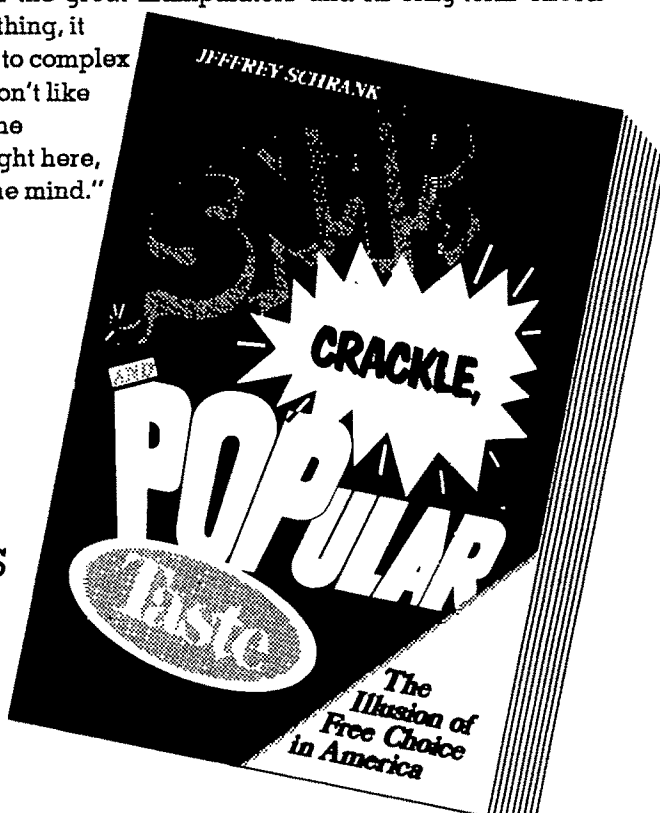
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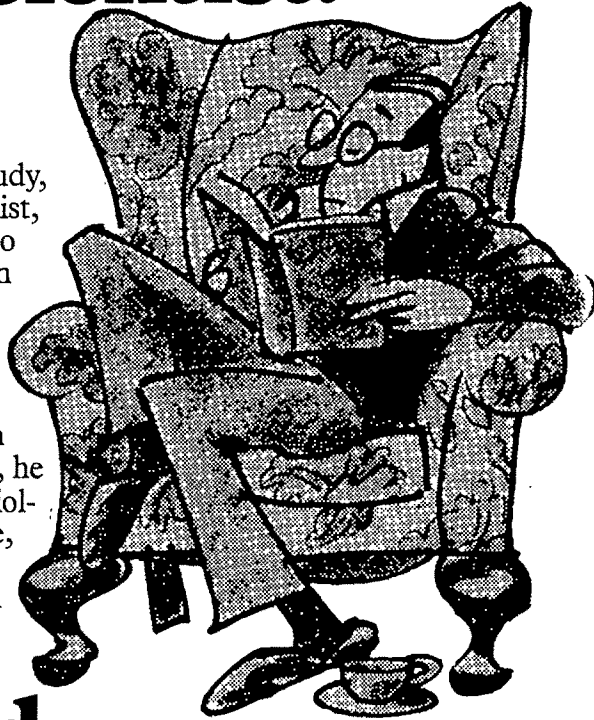


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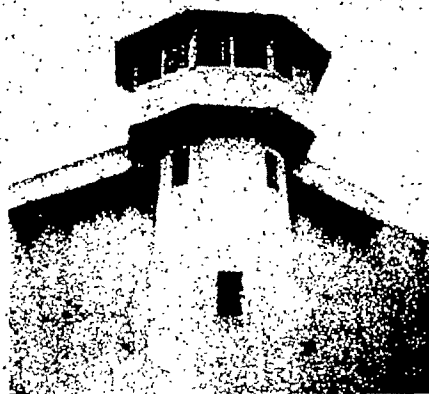
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ITEMS

August, 1977

■ FRED H. GOLDNER (The Production of Cynical Knowledge) is Professor of Sociology, Queens College and Graduate Center, CUNY. He is presently involved as a participant observer in the political economy of health-care planning in New York City. R. RICHARD RITTI is Professor of Sociology and Head of the Pennsylvania Field Research Labora-

tory at Pennsylvania State University. His current research is on the political economy of public organizations such as criminal justice and public welfare organizations. His most recent book is *The Ropes to Skip and the Ropes to Know: Studies in Organizational Behavior* (GRID, 1977). THOMAS P. FERRENCE is Associate Professor and Director,

(Continued on page 675)

THE PRODUCTION OF CYNICAL KNOWLEDGE IN ORGANIZATIONS*

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American Sociological Review 1977, Vol. 42 (August):539-551

The term "cynical knowledge" is used to describe the understanding by members of an organization that presumably altruistic procedures or actions of that organization actually serve the purposes of maintaining the legitimacy of existing authority or preserving institutional structure. This paper explores the processes by which organization members obtain cynical knowledge. A study of the priests of a larger American archdiocese offers evidence that discrepancies in their belief systems, associated with age, can be explained successfully by an understanding of the role that cynical knowledge plays in an organization dependent upon commitment to altruistic ends. In any institution characterized by a strong belief system, authority relationships may be threatened to the degree that cynical knowledge is acquired by participants early in their careers. Such breakdowns of authority relationships may become more common with the organizational changes now occurring in many professional organizations.

All viable organizations rely on a level of commitment from members which goes beyond that engendered by purely instrumental means such as salaries and material benefits. While commitment may be almost nonexistent at lower levels, we expect to find it increasingly as we ascend the organizational hierarchy (Beckford, 1973:68) and we expect it especially in those organizations with a substantial professional component. In this latter case, we find that work duties take place in the context of a specific, conscious and idealistic—albeit institutionalized—belief system about the nature of the occupation and about the services provided by the organization or profession to the society (Ritti et al., 1974).

This reliance on commitment stemming from an idealistic belief system concern-

ing the organization or profession is helpful in promoting organizational stability and motivating the performance of extraordinary duties. Recruitment procedures for professional organizations rely heavily on idealistic or, better, altruistic images. But beyond this, the image of altruism facilitates the maintenance of a wide "zone of acceptance" in authority relationships and in maintaining institutional structure (Simon, 1958:12). In organizations as diverse as the Marines, the Church, universities, and social work agencies, there is a component of belief in the organization which has consequences transcending the purely material correlates of membership.

We are particularly concerned with those organizational beliefs or "myths" that relate motives to consequences for the organization as an entity. Such beliefs are necessarily idealizations of actual facts and, for this reason, are somewhat fragile. Because of this, and because these

* The authors wish to thank Arlene Daniels, Eliot Freidson, Robert Kapsis and Patricia Kendall for their helpful comments on an early draft.

beliefs are important for organizational maintenance, the idealized concept of the organization needs to be protected by mechanisms that will inhibit free and open communication concerning issues which tend to counter idealistic belief. Evidence that the motivations of organizational leaders are not connected to these altruistic belief systems poses a threat to the organization.

We shall use the term "cynical knowledge" to describe knowledge that presumably altruistic actions or procedures of the organization actually serve the purpose of maintaining the legitimacy of existing authority or preserving the institutional structure. Similarly, members of organizations usually construct for themselves explanations for a variety of organizational circumstances about which they have no direct information, but which otherwise make no sense to them. Such explanations may be based on altruistic assumptions. Knowledge that undercuts these assumptions is also cynical knowledge.

If we define altruism as a principle of action for the organization that stresses regard for others or the production of beneficial ends for society generally and cynical knowledge as that denying the goodness or sincerity of these motives, then the particulars of altruism must be interpreted within the framework of the organizational or professional circumstances in question. Within the university for example, altruism relates to motives such as the "advancement of knowledge." It is not unreasonable to interpret "the maintenance of our free enterprise system" as an altruistic motive in the context of business organizations. Consequently, knowledge that top members of the management hierarchy of several organizations have consented to price fixing in order to distribute available business among themselves and avoid competition may be devastating cynical knowledge to committed lower-level participants within management.

It is necessary here also to distinguish cynicism from the failure of idealism. There are other enemies of the belief system that may be damaging to commitment, but through the production of

realism rather than cynicism. For example, realistic knowledge of the inefficacy of medical procedures may damage the commitment of medical students but need not necessarily result in cynicism (Gray et al., 1966; Becker and Geer, 1958). Our concern is about a distinctly organizational phenomenon, particularly the consequences for authority and institutional structure of the development of cynical knowledge about the organization and its motives.

In this paper, we shall use the Roman Catholic Church as an example of what happens when the mechanisms controlling the production of cynical knowledge undergo radical change. The Church is especially vulnerable to cynical knowledge. We have already made the claim that professionals are more likely than others to look for meaning in the profession itself—to have commitment to the overarching purpose of the profession (Ritti et al., 1974). Thus, cynical knowledge is especially damaging in professional organizations where the stated purposes are important in characterizing the organization and where these purposes played a part in the decision of members to join and devote their lives to the profession. The beliefs a recruit holds about an organization to which he has an instrumental relationship are less important—cynicism about them can be compensated for by falling back on the instrumental rewards.

The control and management of communications is all the more important in organizations such as the Catholic Church because there is no "theology of error." The authority of the closed system of theological and dogmatic reasoning depends on the assumption of absolute correctness without regard to a temporal dimension. Apparent change is accounted for by "newly recovered ecclesiological insights." Additional new links, if necessary, can be provided through infallible pronouncements. Because of this, the possible genesis in temporal and political concerns of necessary new links in dogmatic reasoning are carefully guarded secrets. Not unlike other monolithic organizations, such as conservative "Marxist" governments, many large corporations or

elite military organizations, the church pays great attention to its official communications and their content.¹ Thus, the impact of Vatican Council II and the atmosphere of open communication it engendered had an enormous impact on the belief system and organization of the Church.

It is this link between changes in communications patterns and changes in institutional structure that is the subject of this paper. Taking the Roman Catholic Church as an example and using data developed from one large United States archdiocese, we will develop the logic of a cycle linking the failure of organizational mechanisms inhibiting the production of cynical knowledge to other organizational processes that alter institutional structure.² To do this, we will discuss the impact of Vatican II on the opening of communications in the Church and consider that against the background of our respondents' ages which vary directly with their sharply different beliefs. We will then describe the specific content of cynical knowledge within this universe; analyze some of the processes involved in the disclosure of cynical knowledge, including a description of its effects on individual *versus* institutional explanatory schemes; describe the organizational

mechanisms that encourage cynical knowledge; and assert the applicability of those materials to other professional organizations.

The Impact of Vatican II

The sudden and dramatic change in the mode of communications within the Roman Catholic Church introduced by and in Vatican Council II has had enormous and continuing effects. Communication patterns in the Church had been traditionally one-way, with statements of dogma and policy flowing downward from Pope to bishop, from bishops to priests, and from priests to people. The procedures through which decisions on dogma and policy had been reached were not open to the people nor to those at lower levels of the hierarchy.

Change in the Church, as elsewhere, frequently comes about through "enabling mechanisms" which either permit pressures to rise to the surface or which create new situations and definitions. Vatican II was both the origin and the best example of a number of such mechanisms in the Church. It was the first time that most of the attending bishops met together as a body for legislative purposes. It was the first such gathering in 90 years and Vatican I was 300 years after its predecessor. Most of the participants were unlikely to have known those who attended Vatican I, let alone have discussed its nature. During their stay, the bishops were removed from their administrative duties and the responsibilities of "handling" those below them. As O'Dea (1968:123) has put it, they were there to "discuss."

Issues that had not been previously discussed in public were raised, as were those which bishops, in their administrative roles, had had neither the occasion nor time to consider. Unfettered by day-to-day administrative chores, the attending bishops were able to call upon the ideal values that appealed to the core of their religion but that so often were submerged by what appeared to be organizational necessities.³ As one bishop reported (in a

¹ In most cases, consistency with past actions and statements appears to be the overriding concern. For example, before the Tet offensive in Vietnam the Joint Chiefs of Staff had deliberately halved estimates of enemy troops to show a continued decrease in troop strength. The main reason for this appears to have been the fear of "press reaction" with ensuing "stories that enemy strength is greater rather than less." The motivation cited was the "overriding need to demonstrate progress in grinding down the enemy" (New York Times, 1975:7).

² Although our data are from one American archdiocese, we have seen nothing from other surveys of priests in the U.S. to indicate that our findings are not typical. One reader has commented that cynical knowledge has long been present in the Church in other societies without the effects cited here. If we assume this to be true, we might expect such variation to be explained by special characteristics of the American Church such as its minority status in a country with a strong tradition of Church-State separation. Evidence of this distinct character is given in Ellis (1969), Cameron (1973), Sheed (1974) and a recent statement by the Vatican (New York Times, 1972).

³ These arguments about Vatican II are drawn directly from Ference et al., (1971:510).

private conversation with one of the authors) about the final documents: "If the council documents were given to the 2,000 bishops at the beginning, you couldn't have gotten 100 signatures—including mine."

Communications among bishops was not the only social dynamic active at the Council. The decision of Pope John to include observers and other Catholic thinkers made possible a rare diversity of opinion. Large numbers of theologians, including priests, laymen and non-Catholics, were brought into close contact for long periods. Bishops whose everyday duties were overwhelmingly administrative or judicial were thrown into contact with theologians, seminarians studying in Rome and the large numbers of journalists stationed in Rome to cover the story. Traditional barriers and ceremonial shields, by agreement and necessity, were discarded. The bishops were questioned, challenged and pressured on every side into the roles of intellectual leaders of the world's largest institution.

A key impact of the Council resulted from the news of debates and the discussions of personalities that were published throughout the world with remarkable speed. If information had been limited to that contained in the daily bulletins provided by the Secretary General, as was the intent of the Curia, the Council probably would have lost much of its impact, but such was not the case. Although the press was barred from the Council meetings, the initiative of the reporters and the willingness of the Council fathers to provide considerable "unofficial" data led to unprecedented open press coverage. These reports were widely read and made even more exciting because of the clandestine nature of their acquisition. The depth of press coverage of the Council was all the more remarkable, given the obstacles which had to be overcome. Xavier Rynne (1968:51), who was "one" of the most effective journalistic spies describes the effect of the attempt at news management:

Organized by members of the Curia, the Council was conducted in the strictest possible secrecy, but Italian commentators maintained that this only guaranteed the widest

possible circulation of the Council's more intimate doings, and a pasquinade quickly made the rounds: "Why the great conciliar secrecy? Because secrets spread faster."

The Church was faced with an unprecedented situation; issues which were normally not discussed, especially with laymen, were now being broadcast openly. The Council would not operate in the seclusion typical of the Roman hierarchy and then publish only the conclusions. More essential to the aftereffects, unanimity could not be claimed for pronouncements when detailed, reliable descriptions of extended, often acrimonious, debates were widely available.

The evidence of internal discussion and bargaining provided by the press coverage has greatly stimulated the willingness of priests and laymen alike to question and demand justification of the statements and decisions of superiors. Infallibility, a quality which adheres to pronouncements of the Pope or to the Episcopate as a whole under certain limiting conditions, in practice had come to be attributed to virtually all of the statements of members of the hierarchy. As Kung (1971:28) has stated, "The claim to *infallibility* in the Church is always present subliminally even when not given formal expression" (author's emphasis). Theological authoritarianism, itself an exaggeration of a restricted concept, was being applied to administrative matters. The Council, by questioning dogmatic absolutism had legitimated the questioning of administrative absolutism.

The direct consequences of the Council Vatican II, while many and far-reaching, altered the traditional communications patterns within the Church in two crucial ways: first, both by action and recommendation the acceptance of the norm of collegiality among priests and among priests and people encouraged free and open exchange between ranks within the Church; second, because issues that were essentially organizational had been openly debated, new definitions of legitimate topics for such collegial exchange came into being. To understand the impact of these new and open mechanisms of exchange we will turn to the examination of their effects on traditional belief systems in the Church.

The Failure of Belief

While we see the Council Vatican II as a watershed event in the history of the modern Church, our guess is that the reasons for this council had been building for some time. For, while Vatican II is a key event in a complex process of change, in itself it is neither cause nor effect, but both effect and cause of subsequent change in communication patterns. One major manifestation of this change has been a loss of control by the hierarchy over the exchange of cynical knowledge. Table 1 presents some evidence of this process.⁴ Here we see the key elements of the dogmatic belief system arranged by period of ordination. These beliefs range from ac-

ceptance of Christ as God and Man, a tenet central to Christianity generally, to the acceptance of the structure of the Church and the authority of the Pope. Quite obviously, there are large differences by period of ordination and by current status in the priesthood. Making comparisons by rows of Table 1, it can be seen that the only belief with uniform acceptance (completely agree, points 6 and 7, on a 7-point scale) is that belief central to Christian faith—Christ as true God and true Man. However, moving down rows, it is evident that the more specific the belief to the structure and organization of the institutional Roman Catholic Church, the lower the level of acceptance. Thus, the data dramatically illustrate a failure specifically of those beliefs that legitimize the *traditional institutional organization* rather than showing a more general trend toward secularization of priests.

Moving across the columns in Table 1, the second major point to be observed is that there are large differences among the groups represented. By and large, the oldest group of priests accepts completely all of the beliefs presented while the youngest age grouping shows a majority which accepts only the first. A further point to be

⁴ Data are taken from a 1970 survey by the authors of the entire population of the diocesan priests of a major metropolitan archdiocese. Seventy-one percent of the 986 priests returned usable booklets. We note that in many respects other than age this is a highly homogeneous group. Nearly all are urban, of the same ethnicity and education and, obviously, religion. Priests in this diocese appear to hold attitudes very like those of other American diocesan priests generally. Comparisons between the responses in Table 1 with responses from similar questions used in a national study of priests show virtually identical results (N.O.R.C., 1972:95).

Table 1. Acceptance of Beliefs by Period of Ordination or Status

Item	Percentage Agreeing Completely						
	Period of Ordination					Status	
	Prior to 1939	1940– 1949	1950– 1959	1960– 1965	1966– 1970	Ex- Priests	Sem- inarians
Christ as true God and true Man as set forth in Chalcedon is central to my faith	95	91	87	77	66	*	63
Faith is primarily a set of truths to which I assent on the authority of God revealing them	90	71	51	27	19	31	6
Devotion and prayer to Mary is very important to Christian life	86	61	41	21	10	7	10
The hierarchical structure of the Church is divinely ordained by Christ	76	59	38	21	27	7	6
The teaching of the ordinary magisterium of the Pope must be given internal assent	77	59	33	19	18	4	6
Number responding	(182)	(150)	(183)	(93)	(69)	(45)	(52)

observed is that those then in the seminary reflect a belief system almost identical to that of a group of former priests. In fact, if anything, the former priests are *more* conservative. What are we to make of this? Though the structure of the data is clear, as with any point-in-time survey, interpretation of the process is ambiguous. In fact, there are at least four potential explanations of the data. We shall examine these in turn.

Differential attrition. One explanation is that, over the years, priests not accepting the belief system have departed, leaving only those who do accept these beliefs. However, the facts cannot support this explanation. For, if we assume that the oldest group of priests started at approximately the belief level of the youngest, then approximately three-quarters of the younger group would have to leave in order to produce the differences observed if attrition alone were responsible. But factually, attrition levels for the older age cohorts have been nothing like this.⁵

Increasing acceptance over time. A second explanation would be that a process of increasing commitment and, consequently, growing acceptance of beliefs is responsible for the observed data. This would say, for example, that the seminarians who become priests gradually, over time will come to accept the belief system completely. However, when we look at the overall process of change in the Church, this explanation seems unlikely. We have observed that vocations to the priesthood have fallen off considerably from the past and that the priesthood generally is involved in a process of searching for new meanings. We cannot reject this explanation outright but it seems less plausible than others. In addition, these

particular beliefs have never been thought to be associated with the aging process.

Cohort differences. A third explanation postulates that the professional socialization of each of the groups has been different and that the pattern of belief for each of the groups has been different and that the pattern of belief for each of the groups has not changed much from what it was when they first entered the priesthood. This, of course, is the simplest explanation and is consistent with many of our hypotheses about the change process because it ties the changes occurring in this institution to the broader shifts that have been taking place in society. Recruits to the organization have brought with them the changing values of society.

Differential attenuation. This hypothesis has been suggested to us by several priests in the course of our attempts to explain the data and says the following: had we done the survey, say, in the late 1950s, all groups of priests would have looked much like the older group. However, in the ferment of the 1960s and as a result of the opening up of communications following Vatican II, a differential attenuation of belief took place. The youngest groups of priests, less bound to the institutional Church than the older groups, had less reason to cling to conservative views in the face of new knowledge they were receiving. Therefore, what we observe is a rejection of belief which is inversely proportional to the degree of commitment of priests to the institutional Church. By commitment here we mean the degree of a priest's willingness to entertain thoughts of returning to the lay state. Commitment is related inversely to the availability of attractive career alternatives. It also is related to, though not identical with the plausibility of the traditional belief system, including beliefs about the institutional structure of the Church (Ritti et al., 1974). To restate the hypothesis: the oldest groups of priests with no available attractive alternatives to the priesthood have a strong motivation to sustain a traditional belief system which infuses past careers with meaning. Conversely, the youngest groups, less organizationally bound to careers in the priesthood and

⁵ There are no accurate records of past resignation rates because of previous Church secrecy and the very small number of priests who became inactive. As is noted (N.O.R.C., 1972:313), "About 5 percent of the diocesan priests resigned in the four years from 1966 through 1969. Even though the annual rate may not be high in comparison with other professions, it is certainly high compared to impressions about what the rate was in the past."

with available attractive alternatives, have less motivation to sustain a belief system made increasingly implausible by the development of cynical knowledge.

Evidence for this final explanation is presented in Table 2 showing commitment to the priesthood by period of ordination. Generally, the data fit the attenuation explanation quite well, with the rather surprising exception of the group ordained most recently. A possible *post factum* explanation of this departure from the expected relationship, one that also is consonant with the differential attenuation hypothesis, is that the priests of the 1960-1965 period have had greater exposure to certain of the inner workings of Church organization. Hence, on some issues they may be even more cynical than those of the later period.⁶ Still, this is far from saying that we have conclusive reasons to favor this explanation over the cohort hypothesis.

To conclude, whatever the process taking place, obviously it has produced some dramatic differences and requires some sort of explanation. We think it unlikely that anyone of the above explanations, in itself, is sufficient and probably some elements of all are involved. Still, we can

say with confidence that whatever has taken place has involved organizational or societal processes leading to a rejection of the beliefs that have supported the legitimacy of the traditional organization of the institutional Church and to a lessening of commitment of younger priests. Our claim is that changes in communication patterns and the resulting production of cynical knowledge are a key element in producing the data we have observed. We turn now to a detailed examination of these processes.

The Content of Cynical Knowledge

We have described the process by which Vatican II legitimated open communications and have shown the concomitant changes in belief and commitment. It remains for us to document similar changes in the production of cynical knowledge. While we cannot demonstrate causality, our task will be to demonstrate that the data are consistent with our developing line of reasoning. In our study, we wish to show that organizational control of exposure to facts countering official altruistic explanations had progressively weakened over the last few decades. We decided, therefore, not to ask priests *whether* they believed certain counter-altruistic explanations but, rather, to ask *when* they had first heard these topics discussed in the course of their professional careers. Our hypothesis was that early exposure to cynical knowledge before a high level of commitment is built will be considerably more devastating than later exposure.

This fact of the differential exposure to cynical knowledge is documented clearly in Table 3.⁷ Before we get into a discussion of results, the reader should note that the five topics of discussion on the left of the

⁶ Compared to the group ordained most recently, priests of the 1960-1965 period show other small discrepancies in the direction of greater cynicism in Tables 1 and 3. This suggests to us that, on certain topics, cynicism emerges somewhat later with intimate personal exposure to organizational practices. Because of the generally high quality of these data, we think these small differences are real, but we wish to avoid over-interpretation.

Table 2. Commitment to Priesthood by Period of Ordination

Period of Ordination ^a	Percentage indicating they have <i>never</i> thought of returning to lay state
Prior to 1939	71 (178)
1940-1949	54 (149)
1950-1959	37 (183)
1960-1965	17 (93)
1966-1970	28 (69)

^a Categories were pre-coded on questionnaire. Periods prior to 1939 grouped.

⁷ The response categories for this question are as follows: "in seminary or before," "in first assignment," "in later assignment," "as pastor," "never heard this," "too offensive to answer." This last category was provided at the insistence of the Archbishop and his staff. A maximum of six percent responded in this category on the first item in Table 3; none in this category responded on the last item.

Table 3. Exposure to Cynical Knowledge by Period of Ordination

Topic	Percent indicating that they first heard this topic discussed "in first assignment" or before					
	Period of Ordination					In Seminary
	Prior to 1939	1940s	1950s	1960-1965	1965-1970	
That formation of Church teaching can involve bargaining among members of the hierarchy	9	19	39	68	75	77
That members of the hierarchy do not always believe what they say publicly	24	36	48	72	81	83
That pastorates have been awarded on the basis of personal friendship	35	52	77	95	90	96
That priests may remain many years as pastor though they are unfit for the job	44	70	91	98	100	100
That there are alcoholic priests	77	97	97	99	100	100
Number responding	(182)	(150)	(183)	(93)	(69)	(52)

table represent a continuum of clandestine knowledge. This continuum ranges from the first item, which is almost the definition of cynical knowledge as we have described it, through the final item, which is simply an admission of the possibility of individual fallibility and, hence, not cynical knowledge at all by our definition. Examining Table 3 by columns, we can see a consistent relationship between exposure to cynical knowledge and period of ordination. The younger groups are far more likely to have heard these topics discussed in their first assignment or before and seminarians generally acknowledge that they have already heard these topics discussed. Consequently, the data fit our "attenuation process" description quite well. Looked at another way, 68% of the oldest group claim never to have heard the first item discussed while only 23% of the seminarians make a similar claim. Our explanation for this, as we shall discuss later, is that it is necessary to participate in certain kinds of activities in order to gain exposure to counter-altruistic explanations. These mechanisms simply did not exist in the Church for the older priests.

Examining Table 3 by rows, we find a striking correspondence between the level of cynicism characterizing the topic and

the ease of access to that kind of information. Exposure to discussions relating to individual fallibility is commonplace, while exposure to those dealing with topics that are increasingly crucial to the basis of authority and institutional structure correspondingly is not.

The existence of this differential restriction of information is neatly illustrated by our own experience in getting approval for this survey from the Archbishop and the Personnel Committee. They were strongly opposed to the inclusion of the first two items in Table 3. Curiously, their argument did not center so much on disputes as to the truth of these statements as it did to their objection that the open inclusion of these statements would imply that it was possible for a priest to believe these things. On the other hand, there was no objection to the inclusion of the item on alcoholic priests. (In fact, only one percent of the oldest group claimed never to have heard this.) In the process of our own bargaining over the inclusion of the cynical items in the survey, it was suggested to us by the Committee itself that we might want to include an item on homosexual priests. Thus, it is that these data and our discussions with the hierarchy point quite clearly to the distinction

between cynical knowledge and knowledge that counters idealism about characteristics of individuals unrelated to their organization roles.

To summarize, we believe these data point to a significant change in the content of discussions within the Church. When knowledge was closely controlled by the hierarchy, discussions were likely to be concerned with idiosyncratic weaknesses of individuals (Thompson, 1961:122-3). As the control of presumed knowledge has moved from the hierarchy to the body of the Church, the content of discussions has focused increasingly on matters central to the operation of the organization. Cynical knowledge focuses not on the weakness of individual personalities, but on the fallibility of motivation in matters of organizational doctrine and policy. Thus, possession of cynical knowledge can be expected to have an impact on beliefs supporting the legitimacy of traditional organizational forms and of the acceptance of the coherence of the Church as an atemporal whole not subject to the whims of contemporary culture. The perception that some elements may reflect accommodations to temporal necessity make the acceptance of the remainder difficult. For these reasons, the spread of cynical knowledge poses a threat to the institutions of this profession.

Having made the point that cynical knowledge implies a focus on organizational motivations rather than individual idiosyncracies, we shall examine some parallel processes whereby organizational members shift their explanation of certain occurrences from an individual to an organizational level.

*From Personal to Institutional
Inadequacy: A Shift in Focus of
Explanations*

Organizational participants understandably offer explanations of significant events that affect them. These explanations may be at the individual or organizational level and about individual or organizational processes. Shifts in levels of explanation can become important events in their own right. This has hap-

pened in the explanations of those who leave the active ministry.

In the past, leaving the active priesthood was an individual act and an occasion for stigmatizing the individual priest, while now it has become an occasion for indicting the organization for *its* failures. In the past, leaving was interpreted as a weakness of the individual priest—an inability to cope or to maintain the strength needed as a participant in this highly demanding endeavor. Now, it is interpreted as a fault of the organization—an inadequacy on its part—and as an index of the decline of the institution. Personal accounts by ex-priests are no longer full of guilt and are now usually indictments of the organization. Those earlier termed “deserters” are now not even referred to as ex-priests but as “those who have left the active ministry.” And some of these even continue to perform their priestly functions for laity willing to receive their ministrations, albeit, in unofficial or sub rosa situations.

So, too, for all elite organizations, there is a point in their existence when leaving changes from personal inadequacy to institutional inadequacy. The seminary, for example, was viewed as a means to weed out those who “couldn’t take it.” But when vocations drop and numbers leaving the seminary rise to a certain point—when there are insufficient vocations to carry out the tasks—it turns the other way. The reaction to celibacy has had a similar kind of history. More generally, for all organizations, a key index is the interpretation by those who remain of the motives and circumstances of those who leave.

Furthermore, the level of analysis used to explain events is not simply a matter of analytical skills or perceptions, but also ideologies. The shift of explanation from an individual to an organizational level has negative implications for organizational leaders and puts some of the responsibility on them rather than all of it on the departees.

Although a recognition and acknowledgment of the difficulties being encountered by the institutional Church might enable the bishops to lead the Church more effectively through change, it is not surprising that many of them have

made no shift in their focus of explanation. In our diocese, the Archbishop continued to assign blame to the individual resignee rather than to search for explanations within the institution. Predictably, this produced more cynical knowledge which further threatened existing authority and institutional structure.

For example, one well-known departing priest who wrote a long thoughtful letter to the Archbishop explaining his reasons for leaving—including the lack of change within the Church—nevertheless was asked if he were sure there was no woman involved. At the end of his meeting with the Archbishop, he was urged to have a talk with the diocese's psychologist.

In cases such as this, the bishops risk producing cynical knowledge that results from the apparent hypocrisy with which they handle resigning priests. The priest risks formal sanctions against his status as a Catholic unless he receives permission to leave in a process referred to as "laicization." Since resignation has been a secretive procedure in the past, not much was known of the process. However, now that many have left without stigma, the laicization process has become more public. All kinds of negative rumors are likely to circulate in such new situations but, because they are rumors (second- and third-hand word-of-mouth stories that are passed from individual to individual), even if based on truth they can be dismissed by those priests who have no first-hand knowledge or who do not wish to believe "bad" things about those in the hierarchy.

However, unconfirmed rumor and suspicion became open knowledge when the Senate of Priests in the diocese took up the issue of laicizational difficulties and included statements about the process in their committee reports. Thus, the report of the committee on priestly renewal contained the following sentences:

An even more serious matter than that of delay is the degradation to which some, at least, of the applicants for laicization have been subjected. It is a verified fact that some applicants have been told that a favorable decision is more likely if they will testify to immoral conduct or psychological disability on their part. In effect, a priest who has not been guilty of such conduct or who does not

have some psychological weakness is at a disadvantage in respect to laicization.

The discussion that followed this report showed that many neither knew nor wanted to believe that resigning priests would be subject to such treatment. As one priest put it: "It posits that Rome does do this—a laicization process whereby pleading guilty makes it easier. Isn't that a little strong?"

In sum, we have a good example of the shift in levels of explanation due to the introduction of new facts negating the older explanations that are based on the dual premises of the altruism behind organizational motives and individual fallibility as the reason for failure. The emerging explanation quite clearly implies organizationally cynical reasons for laicization: to maintain the existing bases of authority and to mask institutional failure. Significantly, the Senate of Priests, a recent organizational innovation, played a key part in the foregoing illustration. The Senate is both product and producer of cynical knowledge as part of the cycle of institutional change resulting from the failure of the belief that altruistic reasons lie behind institutional structure or serve as an explanation of actions. As producer, the Senate provides a forum where altruistic explanations can be challenged and where facts revealing cynical motives are brought into the open.

The Senate as a Mechanism of Structured Hypocrisy

The Senate, an elected representative body of the priests of the dioceses, was created outside the existing hierarchy following a *Motu Proprio* issued shortly after Vatican II by Pope Paul VI. Set up to provide more participation and consultation in the affairs of the diocese, it has come to modify the uncontested authority of the existing organizational hierarchy. As it evolved, its purposes became those of organizing to modify existing hierarchical relationships, to set standards of organizational mobility, and to adjudicate other personnel issues such as salaries.

The Senate provided a forum that was likely to call forth hypocrisy (hypocritical

assertions by superiors) because it put items on the agenda whose discussion required the administrators to cite reasons for their positions—reasons that might not otherwise be made explicit. But many “real” reasons could not be stated and others had to be stated in their stead. This latter situation encouraged structured hypocrisy.

The above processes are illustrated by the management of a debate in the Senate over whether the survey on which this paper is based should have been conducted. Although the Archbishop was forced to state *some* reason for not having it done, it would have been difficult to make all his reasons explicit. Some reasons are embarrassing because they reflect the naked wish to retain unilateral power. He could and did say that the survey overlapped a concurrent national survey (National Opinion Research Center, 1972) and that it might interfere with the work of the Personnel Board. But he could not say that it might provide evidence about the attitudes and behavior of priests threatening to the actions and beliefs of the hierarchy or that it would confirm, to already dissident priests seeking constituencies, the degree of support for change. He could not say that priests ought not to be informed about how all other priests felt, although he could say there was danger that the data might fall into the hands of newspapers and be misstated and publicized. He could not say that it would raise issues that would force him to take action or that it would make his own inaction obvious. On their part, the senators could not say they wanted it for some of the very same reasons that he did not, nor could they admit explicitly that they wanted it in order to increase their own power. The senators, then, learned more about the political nature of the Church as the new organizational forms contributed both to making that political nature more explicit and actually to increasing politics.

This kind of organizational forum provided by the Senate helps break down the awe in which highly placed leaders are held—an awe that normally serves as a shield against cynical knowledge. Most authors, however, do not deal with cyni-

cal knowledge but with its opposite. Thus, Thompson (1961:70) discusses the importance of “charismatic status rank” and its production of “awe” as a legitimating device for authorities in hierarchical structures:

Incumbents of high office are held in awe because they are in touch with the mysteries and magic of such office; they are “on the inside,” have “inside information,” etc. Since one knows less and less about the activities of superordinates the farther away in the hierarchy they are, the greater is the awe in which he holds them, and consequently the greater is their prestige or status.

Our point is that the less priests know of the organizational particulars surrounding the decisions and actions of members of the hierarchy, the more likely it is that altruistic explanations will be inferred. Awe works to counter questions of “why” with legitimating explanations based on attribution of superior understanding and, in this case, holiness to members of the hierarchy.

Vatican II urged the bishops to mingle with their priests and to meet formally with them to seek advice. As we see in the Senate, these encounters between bishop and priest made it difficult for the bishop to maintain the kind of distance that produces awe. Even more important, the priests obtained and maintained knowledge of the bishop and of his faults. Knowledge that counteracts awe opens the way for cynical knowledge, and knowledge that indicates that awe is deliberately produced is cynical knowledge. Vatican II tried to reduce the pomp and circumstance of the bishop by stating the need for him to express humility and simplicity. In this institution, a humble hierarchy would not by itself destroy awe because humbleness is a prime value of the Church and anyone thought to really possess it would be held in some kind of awe. In this case, cynical knowledge would be the kind of knowledge that revealed the lack of true humility. Cynical knowledge derives from developing an understanding that the pretense of humility (the acting out of humble roles as exemplary for all) serves the motives of organizational control by effectively suppressing protest and by individualizing the

supposed motives for protest by priests. Thus, the invocation of the sin of pride (the antithesis of humility) is used to suggest that the inability of priests to accept discipline is one of the reasons they protest against constituted authority and institutional structure.

Summary

Changes in authority relations and in the institutional structure of the Church are rooted in the production of knowledge that counters assumptions of altruism as an explanation for organizational actions. Data from our survey reveal widespread differences in beliefs among priests of varying ages suggesting a process whereby younger priests, not yet bound into the institutional structure of the Church, are gaining access much earlier in their careers to information that counters altruistic assumptions about the organizational motivations behind Church actions. The attempt to explain the wide differences in beliefs among priests of varying ages has led us to our thesis that cynical knowledge is both a cause and effect of changes now occurring in the Catholic Church in America.

In the past, cynical knowledge, regardless of the amount that existed, had little effect because of the setting in which it was obtained. Cynical knowledge was obtained within the context of a coherent ideological and organizational system which did not question the permanence of either the institution or the place of the priest in it. The lifetime permanence of the priest's vocation prevented cynical knowledge from having the effect it now has—the priest was totally committed to the organization. Men did not leave without severe stigmatization, and those who did leave had few occupational opportunities. Without viable alternatives from ordination onward, the priest had to take his continuance in the priesthood for granted.

In any institution characterized by a strong belief system in its goals, especially where these are in part altruistic, that belief system is threatened if the behavior of higher officials can be characterized as flowing from internal political motivations. The plausibility of the entire struc-

ture of the institution may be threatened. The priest—with viable alternatives to his present calling—can now afford to take seriously the cynical knowledge that comes from widely published and open sources. And much of that cynical knowledge is generated by activities that take place in organized settings which provide an opportunity for extended discussion.

As these discussions serve to threaten the positions of those in authority, so, too, do the explanations offered by priests about problematic circumstances. So, for example, we find a shift in explanations for the failure of commitment on the part of priests from those positing individual weakness to those pointing to flaws in institutional structure.

To conclude, we feel that the changes that we have noted in the Church may be symptomatic of possible changes to come in other professional institutions as they become imbedded in more complex organizational networks. The politicization and bureaucratization of the Church may not be unlike those in other professional groups as they create the organizational mechanisms which hasten the acquisition of cynical knowledge among younger members.

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THE GROUP STRUCTURE OF COCITATION CLUSTERS: A COMPARATIVE STUDY*

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Using block modeling of data from a sociometric questionnaire, we analyze the patterns of social structure shown by authors of two highly cocited clusters of biological-science papers. Analyses of anecdotal data, background information, and data on citations support the findings from the block models. The density of contacts and the patterns of sociometric and citation data show that the authors of papers in the clusters form social groups. Each group has a center-periphery pattern, and the two groups show structural differences that appear to reflect differences in the timing and diffusion of their respective major research findings.

Sociologists interested in the social and intellectual features of scientific change have emphasized the importance of small-scale, informally organized groups of scientists interested in the same or closely related research problems. Research on such groups was originally stimulated by the work of Kuhn (1962) and Price (1963), and many case studies have been published (Griffith and Mullins [1972], summarize much of this research). Systematic comparative analyses of the structure and development of groups are few (exceptions are Crane, 1972; Mullins, 1973), largely because their boundaries are typically indistinct and their lives often transitory. As a result, investigators have disagreed on the appropriate methods for determining the boundaries and interrelations among groups (Chubin, 1976). There is even disagreement over which term most adequately portrays these groups: "specialty," "subspecialty," "research area," "network," "network cluster" and "invisible col-

lege" all have been suggested (Whitley [1974] and Hagstrom [1976:758] have tried to resolve some of these terminological differences).

Small and Griffith (1974) have proposed cocitation analysis as a way to identify the small, informal groups of scientists that play a major role in scientific change. Defining the strength of cocitation as the number of times two documents (either articles or books) are cited together in other documents, they argued that during a given time period groups of documents with strong cocitation ties (1) reflect the interests of publishing authors and (2) probably are important to work in existing specialties. By identifying clusters of strongly cocited documents reported in the *Science Citation Index* (SCI), Small and Griffith tried to outline the specialty structure of contemporary science.

Of course, there is no necessary relationship between clusters of documents and groups of researchers, and many sociologists of science have been skeptical of Small and Griffith's work because there may be no strong relation between membership in a research group and individual scientists' citation of documents (Mulkay, 1974). Indeed, Chubin (1976:452) argued that cocitation analysis produces a scientific "specialty" that is only "the most visible slice of the literature—nothing

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more, nothing less." These objections, like Small and Griffith's original argument, are *a priori* and have yet to be evaluated empirically.¹ In this paper, we present sociometric data for the authors of papers in two biomedical cocitation clusters and use block modeling (White et al., 1976) to assess the two positions.

DATA AND METHODS

We began our investigation with lists (provided by Small and Griffith) of highly cocited documents on reverse transcriptase and Australia antigen. The documents on reverse transcriptase are associated with the discovery of an enzyme that can reverse the normal flow of information in cells. In 1970, two articles reported this discovery and a third reported a replication of the findings. Additional documents, with strong cocitation ties to these articles, formed a small cluster that grew until 1973. In that year, it merged with other clusters into a huge cluster of 329 documents associated with the general area of viral genetics. We chose 137 articles for analysis: the documents from six small clusters in 1971 that merged to form the 1972 cluster and documents in the 1973 cluster that had been written by the authors of documents in the 1972 cluster.

Documents in the second cluster have strong cocitation ties with papers that report the discovery of an antigen associated with type-B viral hepatitis. Although these papers first appeared in 1966, intensive research on the implications of the discovery was not reported until after

1969. Unlike the reverse-transcriptase cluster, the Australia-antigen cluster remained an independent cluster from 1970 through 1973. Therefore, for analysis, we chose all documents included in the cluster in any of the four years—a total of 82.

For each cluster, our next step was to list all first authors of documents and any subsequent authors who coauthored more than one paper. Using this specification, we obtained the names of 105 scientists for the reverse-transcriptase cluster and 65 scientists for the Australia-antigen cluster.

For each scientist, we obtained sociometric data from two sources. The first was a questionnaire mailed to the individuals in the groups asking each to report on nine types of sociometric tie with each of the other scientists on the list for his or her group. The nine ties included three measures of awareness ("Have you ever heard of X?"; "Do you follow the work of X?"; "Do you know X well?"), two indicators of teacher-student relations ("Are you a teacher of X?"; "Are you a student of X?") and four measures of collegueship ("Are you a colleague at the same institution with X?"; "Have you ever been a co-worker with X?"; "Are you presently a co-worker with X?"; "Have you ever been a coauthor with X?"). We obtained completed questionnaires from 82% of the authors in the Australia-antigen cluster and 86% of the authors in the reverse-transcriptase cluster. The questionnaires were mailed and collected during May-July, 1975. The second source of sociometric data was the *SCI*. From it we obtained for each year between 1967 and 1973, each author's citations of any document by any of the other authors in the list for his or her group.

Since we wanted to determine the extent to which social patterns among scientists reflect features of their intellectual products (see Griffith and Mullins, 1972), we searched for published reviews of the research reported in the papers and interviewed selected authors of documents in the two lists. This information was gathered between May, 1975, and December, 1975.

¹ Small (1977) reported a case study of a cocitation cluster concerned with collagen research in which specialists were asked to list (1) the most important papers in its recent history and (2) the leading investigators in collagen research. He found that (1) usually the more higher-cocited papers were more often mentioned by his respondents and (2) the researchers frequently named as leading investigators were more likely than other investigators to have written highly cocited papers. This result is consistent with Chubin's characterization of cocitation clusters as highly visible slices of the scientific literature, but it does not show whether the authors of higher-cocited papers form a coherent social group.

RESULTS

The Cognitive Developments

Both the published reviews and the interviews revealed that the documents in both clusters did, in fact, reflect important intellectual developments and were not merely highly visible slices of scientific literature. The reviews and the interviews also showed important differences between the two areas. As we noted above, the original discovery of Australia antigen was reported in 1966. Immediately afterward, ambiguities about the source and nature of the antigen attracted the discoverers' attention. For some time, researchers believed that a new mechanism of disease causation might be involved (Maugh [1975] discussed developments in this research through 1975). Because of these difficulties, the development of a vaccine for hepatitis B was delayed until 1975. Nevertheless, from 1966 on, research was continuous (becoming more intense after 1968), the original discoverers continued to do research on the topic.

Reverse-transcriptase research has followed a different pattern (Culliton [1971] reviewed research up to 1971). In the middle 1960s, investigators had suggested the possibility that genetic information could be transferred from RNA to DNA, but convincing evidence of the transfer was not presented until 1970. This evidence was presented simultaneously by two groups working independently on somewhat different materials and the investigators involved received Nobel Prizes for the research. After a flurry of activity devoted to specifying more precisely the exact mechanisms that transferred information from RNA to DNA, the researchers moved on to study related by somewhat different topics in oncology and viral genetics. Thus, unlike the research on Australia antigen, which has been fairly continuous since the middle 1960s, work on reverse transcriptase has been declining since the early 1970s.

We now consider whether the sociometric data show that the authors in each group have relationships that would be expected of either currently or recently

existing scientific specialty groups.² We focus on three separate but interrelated questions: To what extent do the authors in each group show high levels of mutual awareness and relatively dense social contacts?; To what extent do the two groups of authors exhibit social structures consistent with previous descriptions of social structure in scientific specialties?; To what extent do the two groups show patterns of growth and change consistent with our knowledge about their patterns of cognitive development?

Awareness and Social Contact

Since there are no previously established levels of awareness, teacher-student ties and collegial ties that we can use as standards for deciding whether a given group of scientists is a scientific specialty, we compare our findings with those from previously published case studies. Table 1 shows the results for the three measures of awareness. The figures are the proportions of the total possible links ($N^2 - N$) that respondents reported for each of the three measures. Thus, .74 for "ever heard of" among reverse transcriptase researchers shows that they formed nearly three-fourths of the 10,920 links that were possible among the 105 respondents from this group.³ Breiger's (1976) results, from reanalysis of data on another biomedical specialty, are not strictly comparable to ours because he reported responses to a question about "unawareness" rather than "ever heard of"; also, he gave the proportion of "mutual

² Given the lack of agreement on a term to characterize the social entity we are studying, we use the term "specialty" to mean typically small and informally organized groups of researchers who carry out work on a given question or topic. The groups could also be called "invisible colleges" (Price, 1963) or "research areas" (Whitley, 1974), but "specialty" is the term most often used by previous researchers who have studied these groups.

³ In the analysis, we have included those who did not return questionnaires because they were recipients of "choices" from the respondents. Obviously, since the nonrespondents reported no ties with the respondents, our results necessarily underestimate the true densities of ties for our groups. Nevertheless (see below), our densities exceed those reported in previous studies, even those from data that also exclude nonrespondents (Breiger, 1976).

Table 1. Overall Network Densities for Three Measures of Awareness, by Specialty

Level of Awareness	Specialty Area		
	Reverse Transcriptase	Australia Antigen	Neural Control ^a of Food and Water Intake
Ever heard of	.74	.84	.47 ^b
Follow the work of	.40	.35	... ^c
Know well	.15	.16	...
Mutual contact07
N	105	65	107

^a Source: Breiger (1976:119-22).

^b This figure is the complement of that reported by Breiger for the proportion of respondents who indicated unawareness.

^c ...=no data for cell.

contacts" rather than the proportion who knew each other well. Since "know well" probably captures a higher level of awareness than is indexed by "mutual contact," the results show that levels of awareness in our two groups are higher than the levels among researchers studying the neural control of food and water intake.

Crane (1972) described student-teacher and colleagueship ties in two specialties. One, in mathematics, concentrated on the theory of finite groups; the other, in rural sociology, concentrated on the diffusion of agricultural innovations. She computed "group connectivity scores" that included both direct ties between specialty members (e.g., the kinds of tie reported in Table 1) and indirect ties for all numbers of steps (Crane [1972:41-4] describes this technique). To obtain comparable figures for our two groups of scientists, we computed connectivity scores for four ties that

are similar to hers. Table 2 shows three kinds of information for each tie: density of direct contacts (as in Table 1) for our two groups, and connectivity scores reported by Crane for her two specialties.

The first two ties, current collaboration and published collaboration, are measured in comparable ways in our study and Crane's. In both cases, our scores are substantially higher than hers. Indeed, in our two groups, direct ties for these two relationships are as dense as all ties, both direct and indirect, in Crane's two specialties. In our two groups, only the connectivity scores for "teacher of" are of sizes comparable to those for Crane's specialties. On the other three relationships, the authors of our two clusters of papers exhibit denser patterns of contact than do the members of other scientific specialties that have been studied. This result supports the claim that highly co-

Table 2. Network Densities and Connectivity Scores for Teacher-Student and Collegial Ties, by Specialty

	Connectivity Scores					
	Ties				Theory ^a of Finite Groups	Diffusion ^b of Agricultural Innovations
	RT	AA	RT	AA		
Current Collaboration	.014	.018	.046	.125010
Published Collaboration	.031	.043	.452	.394	.030	.022
Thesis Directors010	.028
Teacher of	.006	.006	.019	.022
N	105	65	105	65	102	221

Note: RT=reverse transcriptase; AA=Australia antigen.

^a Source: Crane (1972:147-9).

^b Source: Crane (1972:146-7).

cited papers are produced by members of scientific specialties.

Social Structure

Scientific specialties typically display a social structure that, for many social relationships, can be described as a "center-periphery" pattern (Breiger, 1976:128-9). The center is a subset of members who are recognized by others as being central to the specialty. These scientists are more productive than their peers, and among them are those who made the original discoveries that are the intellectual basis of research in the specialty. These scientists usually have central locations in the specialty's pattern of information flow and usually are important to its organizational development also (Mullins, 1972; 1975; Crane, 1972: 49-56). In contrast, peripheral members are less visible to both the core and each other. They are usually linked to only one or a few members of the central group. In addition to central and peripheral members, some specialties include outsiders who, although they have published research on the specialty's topic, show little awareness of core and peripheral members who, in turn, show little awareness of the outsiders. These scientists may show high levels of productivity and eminence in the larger discipline, but their primary research orientations apparently are outside the specialties studied (Breiger, 1976:131-2; Cole and Zuckerman, 1975). To determine whether these general structural features were present in our groups of authors, we did a block-model analysis of each group using the nine sociometric ties reported in the

questionnaires. Block modeling (White et al., 1976) partitions the members of a population into mutually exclusive and exhaustive subsets (blocks). For all sociometric ties in the analysis, the members of each block have equivalent relations with members of other blocks. Thus, for the ties in a given study, the members of a block are structurally equivalent (White et al., 1976:737-40).

We partitioned the two groups of authors by using a variation of the CONCOR hierarchical clustering algorithm (Breiger et al., 1975).⁴ The algorithm was applied to a sociomatrix containing the nine types of tie listed above. In both instances, a four-block partition provided a detailed picture of structure without excessive loss of cases in the blocks. Figure 1 shows the successive partitions of the two groups. The roman numerals are labels for the blocks.

Table 3 shows the patterns for three levels of awareness, presented as 4×4 matrices in which each matrix is a reduced form of the original, individual-level sociometric matrix. For example, for the Australia-antigen (AA) authors, we began with a 65×65 matrix showing ties for "ever heard of." We permuted the matrix to sort the authors into the four AA blocks shown in Figure 1. Thus, the 4×4 AA matrix for "ever heard of" (shown in Table 3) represents the structure of this type of awareness for the four blocks of structurally equivalent authors. The

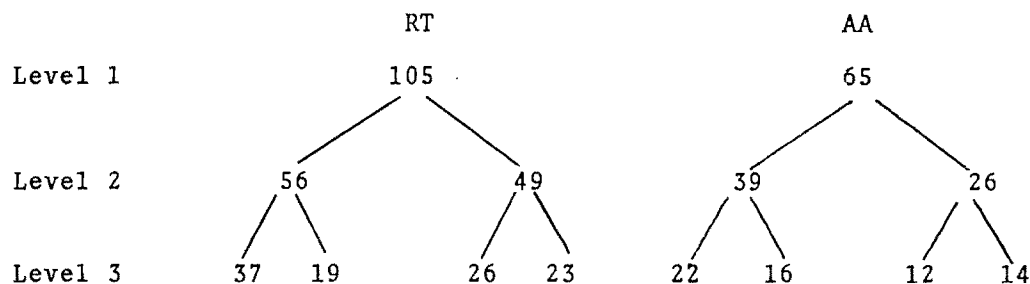


Figure 1. Successive Partitions of Authors

⁴ CONCOR is an inefficient technique for extracting information from sociomatrices (Schwartz, 1977), but is adequate for detecting the general structural characteristics of interest in this article.

Table 3. Four-Block Densities for Three Types of Awareness among Authors of Documents

Reverse Transcriptase					Australia Antigen				
Ever Heard of									
	I	II	III	IV		I	II	III	IV
I	.98	.91	.72	.62	I	.99	.85	.86	.53
II	.92	.78	.58	.36	II	.96	.88	.66	.66
III	.92	.78	.69	.57	III	.91	.77	.83	.67
IV	.83	.59	.47	.45	IV	.89	.81	.73	.70
Follow Work									
	I	II	III	IV		I	II	III	IV
I	.72	.58	.38	.26	I	.51	.67	.58	.34
II	.72	.55	.40	.23	II	.40	.30	.21	.18
III	.44	.30	.20	.11	III	.36	.19	.19	.10
IV	.42	.26	.17	.13	IV	.27	.24	.13	.12
Know Well									
	I	II	III	IV		I	II	III	IV
I	.52	.21	.11	.08	I	.56	.26	.15	.08
II	.22	.12	.12	.07	II	.20	.09	.02	.04
III	.11	.07	.04	.02	III	.10	.04	.18	.01
IV	.12	.03	.04	.04	IV	.08	.08	.02	.02

entries in the matrix are the densities of contact within and between the blocks. Specifically, the entry in cell i, j of this matrix is the number of times members of block i report that they have ever heard of an author in block j divided by the number of such reports that were possible ($n_i \cdot n_j$ when $i \neq j$, $n_i^2 - n_i$ when $i = j$).

If a center-periphery structure exists among the blocks, it will appear as strong ties within one or more core blocks, strong ties between a center block and its "follower" blocks (but not between these follower blocks themselves) and, perhaps, weak or nonexistent ties between some blocks and both center and follower blocks. With proper arrangement of the blocks, this kind of structure would be reflected in high-density cells at one of the two main-diagonal corners of a matrix, with increasingly low densities becoming prominent as one moves toward the opposite, main-diagonal corner.

The results in Table 3 generally confirm the hypothesis that the two groups of authors conform to the center-periphery structure. The types of awareness shown in Table 3 range from the weak "ever heard of" (for which overall density is high) to the strong "know well" (for which overall density is low) and, as expected within a given type of awareness,

the densities decline fairly uniformly from the upper-left to the lower-right corners of the matrixes. In all but three cases,⁵ the exceptions to the general pattern are small enough to be easily attributed to sampling variation and unreliability of measurement. For each group, members of block I are both more visible to members of the other blocks and more aware of those members than they are of themselves. This pattern is repeated if we successively remove blocks I and II from consideration. Thus, in each specialty, the members of block I constitute the center of the group, and the members of blocks II through IV are increasingly peripheral. Unlike Breiger's hunger and thirst specialty, however, there appears to be no outsider block in either of the two groups.

Table 4 shows the densities for five types of social contact. In general, the results are consistent with those in Table 3. For example, in both RT and AA, the members of block I are more likely than members of any other block to have

⁵ For Australia antigen, block III shows unexpectedly high intrablock awareness for "ever heard of" and "know well." For the same two relationships, members of block II show relatively low awareness of members of blocks III and IV. For reverse transcriptase, relatively few of the authors in block II have ever heard of authors in blocks III and IV.

Table 4. Four-Block Densities for Five Types of Contact

RT Authors					AA Authors				
					Teacher of				
	I	II	III	IV		I	II	III	IV
I	.02	.01	.02	.01	I	.02	.01	.02	.01
II	.01	.00	.00	.00	II	.00	.00	.00	.00
III	.00	.00	.00	.00	III	.00	.00	.01	.00
IV	.00	.00	.00	.00	IV	.00	.00	.00	.00
					Colleague at Same Institution				
	I	II	III	IV		I	II	III	IV
I	.08	.04	.03	.04	I	.05	.02	.08	.01
II	.03	.04	.01	.01	II	.00	.01	.00	.00
III	.01	.01	.01	.01	III	.09	.01	.17	.01
IV	.05	.01	.01	.03	IV	.00	.01	.00	.00
					Ever a Co-worker				
	I	II	III	IV		I	II	III	IV
I	.07	.03	.03	.03	I	.12	.05	.07	.03
II	.04	.02	.02	.01	II	.02	.01	.00	.00
III	.02	.01	.01	.01	III	.04	.00	.10	.00
IV	.02	.01	.01	.02	IV	.01	.02	.01	.00
					Co-worker Now				
	I	II	III	IV		I	II	III	IV
I	.03	.02	.01	.01	I	.07	.02	.04	.01
II	.03	.02	.01	.01	II	.01	.01	.00	.00
III	.00	.00	.00	.00	III	.02	.00	.03	.00
IV	.01	.00	.01	.00	IV	.01	.00	.00	.00
					Coauthor				
	I	II	III	IV		I	II	III	IV
I	.08	.04	.02	.03	I	.14	.05	.09	.02
II	.06	.04	.02	.01	II	.04	.04	.00	.01
III	.02	.01	.02	.01	III	.05	.00	.04	.01
IV	.03	.01	.02	.01	IV	.00	.02	.00	.00

taught members of other blocks. The other four types of contact repeat the center-periphery pattern. Overall, these results are consistent with previous theories about teacher-student relationship patterns of information flow in scientific specialties (Mullins, 1975). The major exceptions to the center-periphery pattern in Table 4 apparently explain some of the deviations from this pattern in Table 3 (see footnote 5). In particular, members of AA block III show considerable contact with each other, a finding that is consistent with the AA block III's high density of "know well" ties (Table 3). In contrast, AA block II shows surprisingly little contact with blocks III and IV and this finding may account for the relative lack of ties for "ever heard of" and "know well" be-

tween AA block II and AA blocks III and IV (Table 3).

Thus far, our inferences about center and peripheral groups have been based solely on the sociometric patterns of awareness and contact. By themselves, the sociometric results do not necessarily prove that these two groups of authors exhibit the social structure of a scientific specialty. Therefore, we will now show that the blocks identified above as central blocks (I in AA and RT) do indeed consist of the highly productive and eminent scientists specified in our earlier discussion of a scientific specialty's typical social structure.

Preliminary evidence is the fact that in each group, scientists in block I wrote the original discovery papers. More complete

evidence is presented in Table 5, which shows the block-specific means for six variables that reflect status differentials among the blocks in each group. The results are consistent with the patterns of awareness and contact shown above. For example, RT's block I authors had the oldest Ph.D. degrees (a measure of professional experience), and they exceed the members of the other blocks on the five other measures of status (which indicate performance). RT's blocks II through IV show no consistent pattern on age of degree, but they do show a consistent hierarchical ordering on the five performance measures. The AA authors show no clear pattern for professional experience and papers published, but they do show the expected hierarchy for the more recent performance measures (funding received and 1970 and 1973 citations). Thus, for both groups the data on status generally confirm the inferences drawn from the block models.

Patterns of Change

Thus far, our analysis has been based on cross-sectional sociometric data. The data are fully consistent with the hypothesis that both groups of authors of highly cocited documents constitute scientific specialties. Now we analyze longitudinal data on patterns of citations to all documents, including the highly cocited papers, among the authors of each cluster.

In this analysis, we emphasize the fit between changes over time in patterns of citation and what we know about the cognitive development of the two specialties.

Table 6 shows citation patterns for both groups for four periods during the years from 1967 to 1973. These four periods were determined by examining the density levels and patterns for all years and then combining consecutive years that showed strong similarity. Two results for AA are especially noteworthy. First, there are substantial changes in overall density. Beginning with fairly low densities during 1967 and 1968, there were substantial increases in 1969 and 1970. The high densities in 1970 were maintained until 1973, when they began declining. Second, there are changes in the pattern of citation over the period, and these changes occur precisely when the density changes. During the earliest period, block I was cited slightly more than any other block, but all blocks show intrablock densities that are higher than any of the interblock densities. In 1969 a center-periphery pattern began to emerge. It was quite strong during the period from 1970 to 1972 but the pattern began to weaken in 1973.

These two changes conform fairly closely to the intellectual development of the specialty described earlier. Intensive research in the area did not develop until the late 1960s and early 1970s. When it did develop, the authors who had made the

Table 5. Block-Specific Medians for Measures of Status

Block	Year of Ph.D.	Research Funding ^a	Papers Published ^b	Citations Received		
				1967	1970	1973
Reverse Transcriptase						
I	61.5	3.0	35.0	47.0	72.0	136.0
II	65.0	1.0	14.0	2.0	10.0	64.0
III	65.0	0.5	12.5	3.5	12.5	29.0
IV	67.5	0.0	8.0	0.0	5.0	40.0
Australia Antigen						
I	59.0	2.0	39.0	11.0	40.0	88.0
II	61.0	0.0	21.5	2.5	42.0	59.5
III	63.0	0.0	13.5	0.0	11.0	37.5
IV	64.0	0.0	19.5	0.0	14.0	30.5

^a Research grants and contracts for 1974 and 1975, in \$10,000s. Source: Smithsonian Scientific Information Exchange.

^b Total papers published, 1967-73. Source: *SCI*.

Table 6. Four-Block Densities of Citation Flows among Authors of Documents

RT Citations					AA Citations				
1967-69					1967-68				
	I	II	III	IV		I	II	III	IV
I	.04	.01	.00	.00	I	.03	.00	.01	.00
II	.01	.02	.00	.00	II	.01	.02	.00	.00
III	.01	.00	.01	.00	III	.02	.00	.03	.00
IV	.01	.00	.00	.01	IV	.00	.00	.00	.03
Density=.011					Density=.011				
1970					1969				
	I	II	III	IV		I	II	III	IV
I	.12	.03	.02	.02	I	.13	.05	.09	.01
II	.04	.02	.01	.01	II	.06	.05	.03	.01
III	.03	.01	.02	.01	III	.02	.01	.04	.00
IV	.01	.00	.00	.01	IV	.00	.00	.00	.02
Density=.030					Density=.042				
1971					1970-72				
	I	II	III	IV		I	II	III	IV
I	.17	.08	.05	.05	I	.26	.14	.11	.03
II	.10	.08	.05	.02	II	.14	.13	.05	.04
III	.05	.02	.03	.00	III	.05	.02	.06	.01
IV	.02	.01	.00	.01	IV	.08	.07	.04	.05
Density=.036					Density=.100				
1972-73					1973				
	I	II	III	IV		I	II	III	IV
I	.09	.04	.02	.02	I	.19	.09	.06	.02
II	.04	.06	.02	.01	II	.07	.07	.02	.02
III	.02	.02	.02	.00	III	.04	.01	.04	.00
IV	.00	.00	.00	.01	IV	.01	.01	.00	.02
Density=.028					Density=.056				

original discoveries in 1966 constituted a clearly bounded central group. This group maintained its position after 1972 but, as work in the area became less intense, citation densities among the group declined.

Similar results are shown for RT authors. The citation densities were relatively low from 1967 to 1969, and then increased to a peak in 1971, the year following the publication of the discovery papers. Then a decline in densities occurred during 1972 and 1973. Once again, the earliest period shows a pattern wherein block I receives more citations than the other blocks, but intrablock densities are generally higher than interblock densities. This pattern changes to a center-periphery pattern as densities increase, but the original pattern reemerges when the densities decline (1972-1973). The chief difference between the two groups is the more sustained period during which Australia-antigen authors main-

tained high citation densities and a center-periphery pattern.

In general, the longitudinal results for the two groups of authors correspond fairly closely to the cognitive development. Both groups show the same general changes in densities and patterns, and these changes occur at the expected points in time. Furthermore, the chief difference between the two groups is consistent with the fact that research on reverse transcriptase occurred during a shorter period of time than research on Australia antigen.⁶

⁶ Given the lack of methods for testing statistical hypotheses about changes in the densities and patterns of ties in sociometrics (Granovetter, 1976), we cannot determine whether the results shown in Table 6 can be reasonably attributed to random sampling and measurement error. Given the consistency of our social structural results with known intellectual characteristics of the two groups, however, we doubt that either sampling or measurement error produced the social structural results.

For both groups, the decline in citation densities and the weakening of the center-periphery pattern during the most recent period cannot be attributed to a decline in the scientific activity of their members. Data on publication (not presented here) show that the scientists in our study have continued to publish at the rates they did during the two groups' peak years. Furthermore, although the members of each group now cite each other less often than during the peak years, they have received increasing numbers of citations from the scientific community as a whole. These results suggest that, with the passage of time, the scientists in each group have turned their attention to other research topics. This kind of shift among the central scientists in a specialty has been suggested as the modal pattern of specialty decline (Mullins, 1975; Mulkay, 1975). In sum, the longitudinal citation data for both groups of authors further corroborate the conclusion that the two clusters of highly cocited documents are indeed the products of scientific specialty groups.

CONCLUSIONS

In this paper, we have examined various features of the social structure exhibited by the authors of two clusters of highly cocited papers. Both groups are dense social networks, and both show patterns of social ties that are consistent with previous accounts of the social structure of scientific specialties. In addition, changes over time in the flow of citations among the authors in each group closely match the cognitive development of the two research areas. Finally, differences between the two groups, such as the different times of peak activity and the difference in citation densities during these periods, also match closely the different cognitive development of the two areas.

On the basis of these results, we conclude that the highly cocited papers were produced by scientists who were, when the papers were written, members of scientific specialties. The findings strongly indicate that cocitation analysis can identify scientific specialties and thus be a

promising tool for the comparative analysis of specialty growth and development. Two directions of inquiry seem promising. First, researchers should do more detailed analyses of the social structures of groups such as those discussed in this paper. The consistency in the relationships among blocks across different types of social contact suggests the existence of coherent role relationships, and the algebra of these relationships (Boorman and White, 1976) needs explication. Second, the kinds of research group identified by cocitation analysis are not equivalent to some of the entities discussed in previous work on scientific specialties. The two groups examined in this paper, for example, fit the models of neither the "revolutionary" nor the "elite" specialty (Mullins, 1973:299-300). We believe that analyses of additional research groups detected by cocitation analysis will yield information about the variety of structures currently encompassed under the term "specialty" and the dynamic processes of specialty growth, transformation and decline.

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THE REALISM OF EDUCATIONAL AMBITIONS IN ENGLAND AND THE UNITED STATES*

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Differing characteristics of the educational systems in England and the U.S. lead to the hypothesis that educational expectations should become "realistic" earlier in adolescence in England than in the U.S. Several definitions of realistic are used in the analysis of data obtained from thirteen-year-old boys in both countries. The hypothesis is generally supported, but the English boys are found to overestimate the significance of ability in the process of educational attainment. This overestimate is also found for older English boys, while older American boys report the most realistic expectations of any of the samples studied. These findings are interpreted as indicating different effects of the institutional characteristics of the two educational systems.

Discussions of the aspirations of adolescents note that the levels of expected (or hoped for) attainment reported by young people are often quite unrealistic. This usually means that there are many more respondents who aspire to high levels of attainment than can possibly be expected to reach them.¹ The difference between ambition and probable attainment evidently becomes smaller as the respondents get older, however, and by the

time American students are in twelfth grade, there is a rather close correspondence between their educational expectations and their probable attainments (Kerckhoff, 1974a).²

The unrealistic ambitions appear to reflect ignorance of the association between antecedents and attainments. Thus, if ambitions become increasingly more realistic as adolescents near maturity, there must be some process through which this greater realism is brought about. There is evidently a growing appreciation both of the probabilities of reaching various levels of attainment and of one's own potential in relation to this set of probabilities. Our current understanding of the process of social stratification leads us to believe that, initially, the two most important predictors of ultimate level of attainment are one's social origins in the stratification system and one's academically relevant ability (Sewell and Hauser, 1972; Haller and Portes, 1973).

Information about the factors associated with various levels of attainment and about one's own position in the distribution of these factors undoubtedly comes from a variety of sources. Family, friends, the mass media and the school all contrib-

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¹ For instance, in Turner's (1964) study of Los Angeles high school seniors, over half of the boys expected to attain professional or semi-professional occupations, and two-thirds expected to obtain some college education. In light of their backgrounds, which were generally similar to those of the U.S. population, such expectations almost certainly could not be realized.

² The same source suggests that this is much less true for occupational expectations, twelfth graders being almost as unrealistic as sixth graders.

ute to this understanding. The school is especially important for obtaining an assessment of one's own relative position, since the school uses objective assessment methods and deals with nearly the full range of individual abilities. In all modern societies, the educational systems not only teach the child but also report on how well he/she is doing in relation to others at the same level of development. Schools also (at least implicitly) inform the individual of the long-range significance of different levels of academic performance.

School systems differ, however, in the kind and amount of feedback they provide to their students. Some provide much more explicit reports of the students' levels of performance and, especially, of the long-range significance of these levels. The more explicit the feedback, the more realistic one would expect the child's view of the future to be. For instance, school systems in societies having what Turner (1960) has called "contest" and "sponsored" mobility processes would be expected to differ in the explicitness of feedback and, thus, we would expect adolescent ambitions to differ in their degree of realism. In a system of *contest* mobility,

elite status is the prize in an open contest and is taken by the aspirants' own efforts. . . . Under *sponsored* mobility elite recruits are chosen by the established elite or their agents, and elite status is *given* on the basis of some criterion of supposed merit and cannot be *taken* by any amount of effort or strategy. (Turner, 1960:856)

Turner uses England and the United States as examples of sponsored and contest mobility systems, respectively. In the traditional English system, a decision is made when the child is eleven or twelve years old regarding the type of secondary school he/she will attend. The major differentiation is between two rather different types of schools. They are the grammar school and the secondary modern school, the former providing a much more demanding and academically-oriented curriculum intended to prepare students for post-secondary education. The selection process depends very heavily on the children's performance on achievement tests and the expert judg-

ment of educators.³ This division thus separates the students into two groupings having significantly different long-range possibilities. In contrast, the American system of contest mobility provides somewhat more opportunity for adolescents to change course throughout secondary school and encourages the belief that such change is possible.

Information about one's performance and potential which is received from the school system thus would be expected to have very different meaning in these two cases. In the English case, a clear-cut and presumably irrevocable message, which defines the future within rather narrow limits, is received at an early age. Turner (1960:859) interprets the English system: "The earlier that selection of elite recruits is made, the sooner others can be taught to accept their inferiority and to make 'realistic' rather than phantasy plans." In the American case, on the other hand, feedback is likely to include expressions of hope and encouragement for better performances in the future. This hope and encouragement provide a basis for unrealistic expectations and they reduce the likelihood that children will use the most relevant bases for estimating their potential for future attainment.⁴

The present paper is part of a broader investigation of educational expectations and attainment in England and the U.S. which takes this differentiation between

³ The English school system has been changing significantly during the past decade. In particular, there is much less dependence on examinations (the so-called 11+) as the basis of secondary school assignment, and a new form of school, the comprehensive school, has been introduced. The latter includes both types of students, though often in thoroughly segregated tracks. These changes had hardly begun, however, at the time the data analyzed here were collected. Thus, the dual secondary school system, in which assignment was heavily influenced by ability and performance measures, was predominant. There were, of course, several other kinds of schools besides the grammar and secondary modern schools, even in the 1960s, but the great majority of students attended one of these two types, and the others can rather easily be classified into the dichotomy formed by these two. Finally, the school-leaving age has been raised from fifteen to sixteen since the data used in this analysis were collected.

⁴ Yuchtman (Yaar) and Samuel (1975) use a similar conceptualization of the differences in the systems when comparing Israel and the U.S.

the educational systems in the two countries as its point of departure. The primary concern here is to examine the general hypothesis that educational expectations become realistic much earlier in England (under a sponsored mobility system) than in the U.S. (under a contest mobility system). In making this comparison, three forms of analysis involving three levels of specification of the general hypothesis will be employed. First, the average educational expectations of samples of English and American adolescents will be compared with the average attainments of young adults in the two societies to see if there is a general tendency for the American adolescents, more than the English, to have unrealistically high expectations. Second, a predicted level of attainment will be estimated for each individual in both samples, based on a prediction equation derived from earlier research, and the individuals' own expectations then will be compared with these predictions. The hypothesis would call for a greater similarity between these predictions and the individual's own expected attainments in England than in the U.S.

A third form of analysis will be used to assess the degree to which adolescents, in reporting their educational expectations, weight the relevant factors in ways that are similar to their weights in the determination of actual attainments. Specifically, we will see if the relative weights of social origins and ability are the same when the subjects' expectations are regressed on these variables as they are when the attainments of young adults are regressed on them. This is the most refined way we have of assessing the degree of realism of the boys' expectations. If the English system of sponsorship functions more effectively than the American contest system to inform students about the process of attainment, we would anticipate that the two equations would be more nearly the same in England. Since assignment to one of the two types of secondary schools is presumably an important source of information about one's future in England, this third type of analysis also will examine the effect of secondary school assignment on expectations in England.

Although there is adequate reason to

hypothesize that these English-American differences will be found at some point in the life of an adolescent, it will be important to take the age of the subjects into account. This is true for two reasons. First, it is not necessary to infer that in a system of contest mobility young people *never* obtain a realistic view of their probable attainments; it is only necessary to infer that they are slower to do so than young people in a system of early sponsorship. Thus, our guiding hypothesis is that English adolescents will become more realistic *earlier*. Second, the differences in the educational systems make it difficult to equate young people solely on the basis of age. A large proportion of English adolescents leave school before age sixteen while most American adolescents are still in school at seventeen. Thus, it will be necessary to consider not only chronological age when comparing adolescents in the two countries but also to consider how close they are to the average school-leaving age. Both reference points will be used in the analysis.

Method

In 1964, the Council of the International Project for the Evaluation of Educational Achievement (I.E.A.) conducted a study of mathematics performance in twelve countries, including England and the U.S. (see Husén, 1967, for a discussion of the overall study). National samples were drawn from those students who were thirteen years old—most of them were presumably in the eighth grade in the U.S. and the second form in England. Although both boys and girls were included in the samples, only data from the boys are used in the present analysis. There are 2,489 American and 1,231 English boys in the samples studied.

The primary analyses will be conducted with these samples, but two other data sets will also be analyzed. The I.E.A. thirteen-year-olds are at different points in the normal pattern of secondary school education in the two countries, the English boys being closer than the American to the point at which most students leave school. Thus, the two other data sets are from samples of boys who are at

different ages but who are at the same general place in the educational system, namely, at the point just prior to when most students leave school. One data set is from a sample of fourth form (roughly fifteen-year-old) English boys studied by Peaker (1971). This is a subsample of 382 boys taken from a larger national sample. The other is from a sample of American white male high school seniors studied by Kerckhoff (1974a). The 956 boys in the sample are from Fort Wayne, Indiana. Although the variables were not measured in the same way in these two studies, nor in the same way as in the I.E.A. study, all data sets contain measures of the same variables. The measurement methods are outlined in Table 1, and the details are reported in the research reports cited.

The analysis will be presented in four stages. First, the average number of years of additional education expected by the I.E.A. thirteen-year-olds will be compared with experts' estimates reported at the same time. Second, a regression equation found to be effective in predicting the actual attainments of young adult males in the two countries will be used to generate

individual attainment predictions, and these will be compared with the individual expectations reported by the boys in the I.E.A. samples. Third, the regression weights in the attainment prediction will be compared with the weights found using the same independent variables to explain the I.E.A. boys' expectations. All three of these analyses will be used to test the hypothesis that the English expectations will be closer to the predicted outcomes than will the American expectations. Finally, an analysis which parallels the third will be conducted using the fifteen-year-old English and the seventeen-year-old American subjects. This analysis will be used to see if the inter-nation comparison is the same when the samples are equated on the time away from the average school-leaving age rather than being equated on chronological age.

Results

The simplest definition of realism is based on a comparison between a group's average level of expected attainment and the experts' best estimates of predicted

Table 1. Measures of Variables Used in the Analysis

Variable	Data Set		
	I.E.A. Thirteen-Year-Olds	English Fourth Form	U.S. Seniors
Father's Occupation	Five categories ranging from "Professionals and Managers of Large and Medium-Sized Organizations" to "Unskilled Workers." Coded 5-1	Six categories ranging from "Professional" to "Unskilled Manual." Coded 6-1	Duncan SEI Scale scores
Father's Education	Number of years of education received	Age at which he left school	Nine categories ranging from "Eighth Grade or Less" to "Graduate or Professional School." Coded 1-9
Ability	"Total Mathematics Test Score." Constructed by summing several sub-test scores	"Total Test Score." Composed of both verbal and quantitative sub-tests	Lorge-Thorndike I.Q. Test Score
Educational Expectations	Number of years of additional full-time education expected	Age to which they expected to stay in school	Amount of education expected. Six precoded answers ranging from "Quit High School before Graduating" to "Graduate or Professional School." Coded 1-6

attainment. Estimates of the probable average educational attainment of boys who were thirteen years old in 1964 were obtained from Robbins (1963) for the English and from Simon and Grant (1967) for the American samples.⁵ These sources indicated that American boys of thirteen would probably obtain 5.6 additional years of education on the average, and English boys would probably obtain 3.5 additional years on the average. The American boys in the I.E.A. sample said they expected an average of 7.4 years of additional education, and the English boys said they expected an average of 3.9 additional years. This is, at best, only a crude index of the boys' realism, but there is little doubt that, according to this index, the English boys were more realistic than the American boys. The American ambitions were clearly too high by this definition, but the English ambitions were only slightly high.

The prediction equation used to derive individual predicted attainments for the I.E.A. boys was taken from Kerckhoff (1974b). In that study, it was shown that a path analysis using social origins and ability as predictors of the actual educational attainments of young males produced surprisingly similar results in the two countries. That analysis suggests that a single set of standardized regression coefficients can be used to predict educational attainment in the two countries,

⁵ Both of these sources provides tables in which educational attainments are estimated for past and future years. In Simon and Grant (1967:7), Table 8 reports "estimated retention rates, 5th grade through college entrance" for the beginning years (when students were in 5th grade) from 1924-1925 through 1959-1960. Figure 2 (1967:8) reports "estimated retention rates, fifth grade through college graduation" for the beginning year of 1959-1960. The combination of these two sources makes a fairly exact estimate possible, though the estimate does not control for sex. In Robbins (1963:103), Table 3 gives "percentage of each age group [ages 15 through 19 and over] at school in January" for England and Wales, by sex, 1950-1985. Table 31 (1963:148) reports "initial entrants to higher education as percentage of the age group," by sex, 1954-1962. Table 34 (1963:151) gives similar information for 1955-1985. These latter two tables, together with data in Section 8 on "length of courses," make estimates of post-secondary school attainments possible. In both sources, there are other statistics cited against which the estimates used in the present analysis could be checked.

using social origins and ability as predictors. The equation derived from that source and used in the present analysis is:

$$\hat{Y} = \sum_{i=1}^k [\beta_{ij}(X_{ij} - \bar{X}_{ij})/S_{ij}]S_{yj} + \bar{Y}_j$$

where β is the standardized regression coefficient applicable to both countries, i refers to the particular independent variable (father's occupation or education or son's ability), j refers to the country (England or the U.S.), S_{yj} is the standard deviation of the particular variable for the country, and \bar{Y}_j is the predicted mean additional years of education for the country. By using the means and standard deviations appropriate to the particular country, it is possible to transform the original equation, based on standardized coefficients, into an equation based on metric coefficients appropriate to the particular measures used in this study.⁶ The \bar{Y} scores produced in this way can be rounded to the nearest whole year and then used as an individual's predicted attainment level. Throughout the discussion, the boys' estimates of their future attainments will be referred to as "expected attainment" while the rounded scores produced in this way will be referred to as "predicted attainment."

When the predicted attainment scores are cross-tabulated with the expected attainment scores, the two scores are identical for 33.4% of the English and 18.8% of the American boys; they are either the same or within one year of each other for 74.6% of the English and 52.2% of the American boys.⁷ Thus, again, by this more refined definition, we find the English I.E.A. boys' expectations to be more realistic than those of the American boys.

⁶ The standardized coefficients used in the analysis conducted here are those reported by Kerckhoff (1974b: Table 3) as "Duncan (uncorrected)" for the American equation and "Douglas (a)" for the English equation. An analysis that is fully parallel to the one presented here was conducted reversing the standardized coefficients but leaving the rest of the computation the same. It produced somewhat different results, but the overall pattern was the same.

⁷ The gamma, computed for the cross-tabulation of these two sets of scores, is .709 for the English and .338 for the American sample.

Yet, the analysis presented thus far does not demonstrate that the closer fit between predicted and expected levels of attainment in England result from the English boys' having based their expectations on the same antecedents which help explain actual attainments. Since the best prediction equation available uses social origin and ability as the bases of prediction, a wholly realistic group of boys (by this definition) would report expectations that are associated with these antecedents the same way as attainments are.

Table 2 provides a number of bases for comparing the antecedents of educational attainment (the source of our prediction equation) in the two countries with the antecedents of the boys' expectations. The "Path Coefficients" are taken from a path analysis in which expectations (or attainments) are regressed on ability and SES. "Gross Effects" refers to the proportion of the variance in expectations (or attainments) which can be explained by ability or SES alone. It is the square of the correlation between ability and expectations (or attainments) and the square of the multiple correlation of father's occupation and father's education with expectations (or attainments), respectively. "Unique Effects" refers to the additional variance of expectations (or attainments) explained by ability or SES when added to the gross effects already attributable to the other source. R^2 is the total variance explained by the combination of ability and SES. (It is the sum of the ability gross effects and SES unique effects as well as the sum of the SES gross effects and ability unique effects.) For the moment,

only the first two rows of Table 2 will be discussed.

Not surprisingly, given the earlier analysis, the R^2 of expectations with social origin ability is much larger in England than in the U.S. Also, both the gross and the unique effects of both ability and social origin are stronger in England.⁸ All of these findings are consistent with the general hypothesis and with the earlier findings. English boys report educational expectations which show strong effects of social origin and ability. Also, consistent with the presumed basis of sponsorship in the English system, ability is a stronger source of explanation of expectations in England than social origin is, while the two effects are nearer to being equal (though weak) in the U.S. All of this is consistent with the view that the sponsorship system provides relatively young boys a clear indication of their probable attainments, while a system of contest mobility leaves the outcome more ambiguous.

When these results are compared with those based on a parallel analysis of educational attainment (reported in the last row of Table 2), however, a somewhat different impression is gained. On most of the measures, the results from the analysis of educational attainment fall between the two sets of results for educational expectations. Clearly, if we use the effects of

⁸ It should be noted that some of the differences between metric coefficients are not the same as the differences between the path coefficients, due to differences in variance of some variables in England and the U.S. The overall contrast is still large and in the same direction, however.

Table 2. Antecedents of Educational Expectations and Attainments in England and the United States

Dependent Variable and Data Set	Path Coefficients			Gross Effects of		Unique Effects of		R ²
	FaOcc	FaEd	Ability	SES	Ability	SES	Ability	
Expectations								
English 13-year-olds	.187	.179	.456	.269	.351	.088	.170	.439
U.S. 13-year-olds	.051	.261	.192	.123	.081	.075	.033	.156
English 4th Form	.109	.136	.538	.133	.358	.041	.265	.399
U.S. 12th Grade	.165	.246	.367	.227	.225	.126	.124	.351
Attainments in England and U.S.*	.233	.204	.381	.209	.234	.134	.159	.368

* These effects are based on simple averages of the effects reported in Kerckhoff (1974b: Table 3) for the "Duncan (uncorrected)" and "Douglas (a)" analyses for the U.S. and England, respectively.

the antecedent variables on attainments as our definition of realism, the English boys are almost as unrealistic as the American boys, only in the opposite direction. They tend especially to *overestimate* the importance of ability in educational attainment.

A strong emphasis on the importance of ability, even an excessively strong emphasis on ability, might be anticipated in a country like England whose educational system is organized to sponsor the development of promising young people. Just two years prior to the time these data were collected, these English boys experienced the all-important division into two types of secondary education, a division that is presumably based largely on measures of academic achievement and promise. Those selected for grammar school realistically should expect higher levels of educational attainment than those attending secondary modern schools, and the type of school attended is certain to be associated with ability level. If the direct effect of secondary school assignment is the reason for the strong emphasis on ability, however, two other outcomes also should be observable. First, we should find a high correlation between ability and type of school attended. Second, when school type is introduced into an analysis of the antecedents of educational expectations, it should sharply reduce the effect of ability on expectations.

These outcomes are found, but they are weaker than might have been expected. The correlation of ability and school type is .49. Given the presumed bases of the secondary school selection process, this is not very high, but it is consistent with others' findings.⁹ When school type is used as a fourth antecedent variable (together with son's ability and father's occupation and education) in the explanation of educational expectations, it does reduce the direct effect of ability on expectations but by less than one-fifth. (Reductions in the two SES paths are inconsequential.) The path coefficient for ability in such an analysis is .379, compared with the .456 reported in Table 2. By comparison, the path coefficient for school type is

.174. Here again, the effect is smaller than would be expected if secondary school assignment were the major source of influence on English educational expectations. Even when the specific effect of school type is partialled out, the remaining direct effect of ability is still stronger in England than in the U.S. The English boys are sensitive to the significance of ability in educational attainment, over and above its direct relevance for secondary school assignment. It is not clear why this is so, but it might be due to the use of "tracks" within each type of school or simply to a heightened salience of ability in all school activities.

All of these findings are based on data from thirteen-year-olds in the two countries. One might legitimately ask whether the age of the subject is an important factor in determining the results reported. Are the differences reported found only among young adolescents or do they persist throughout the pre-adult period? Do American boys continue to be less realistic and do English boys continue to overestimate the importance of ability? Can the differences noted be attributed in part to the fact that the English boys were, on the average, closer than the Americans to the point at which most of them leave school? Similarly, are the English results affected by the English boys' recent experience with the secondary school selection process, a process that heightened their awareness of the importance of ability?

Although such questions cannot be answered with certainty, additional analyses using the two older samples described earlier help to put the previous findings in perspective. It will be recalled that the older English boys were in the fourth form and the older American boys were in their senior year in high school. Therefore, they were at or nearing the age (fifteen in England, seventeen in the U.S.) at which most boys leave school. Their views of their probable educational attainment should be as realistic as we could hope to find among adolescents in the two countries.

Analyses parallel to those reported above were conducted with these data, and the results are reported in rows three and four of Table 2. A comparison of these

⁹ Douglas (1964) examines this relationship in detail.

results with those for the thirteen-year-olds in rows one and two indicates the following: (1) The English fourth-form data produce results that are very similar to the English thirteen-year-old findings. If anything, the relative effect of ability on expectations is even stronger among these older boys. (2) The American seniors' data produce results that are quite different from those for the American thirteen-year-olds. Social origins and ability both contribute more to an explanation of expectations among these older boys than among the younger American boys. (3) Of the four analyses of expectations, the one which most closely approximates the analysis of attainments (reported in the last row of Table 2) is from the American high school seniors.

The data used in this further analysis are certainly not ideal for our purposes, but they do provide both support for the initial findings and a basis for interpreting those findings. The strong effect of ability on educational expectations in England is found among the older as well as the younger boys. There is thus no indication that the young English boys' overemphasis on the importance of ability is a temporary effect. We also find among these older boys, as we did among the younger ones, that the strong effect of ability is not wholly dependent on secondary school assignment. Secondary school assignment is only modestly correlated with ability in both cases (.48 compared with .49 among the younger boys). And when type of secondary school is used in the analysis, it again reduces the ability path by only one-fourth (from the .538 in row three to .424). Finally, as with the younger boys, the path coefficient for school type is significant but still not very large (.257 compared with .174 for the younger boys).

Though there seems little doubt that the English boys obtain a more realistic idea of their probable educational attainments earlier than the American boys do, the findings for the American high school seniors indicate that the American expectations eventually become quite realistic. When the antecedents of the seniors' expectations are compared with the antecedents of educational attainment, the seniors' expectations seem to be more

realistic than even those of the fourth-form English boys.

Of course it can be argued that it is equally unfair to compare fifteen-year-old English boys with seventeen-year-old Americans as it is to compare thirteen-year-olds who are, to different degrees, removed from entry into the adult world. There is no real solution to this problem, since the two educational systems function in such different ways. Whichever comparison is made, though, the English emphasis on the significance of ability stands out. What also stands out is the "free-floating" quality of early American expectations, expectations that evidently become increasingly more firmly linked to social origins and ability as the boys pass through high school.

Conclusion

If we return to the original question concerning how realistic the educational expectations of adolescents are in the two countries, the answer appears to be more complex than anticipated. There is no doubt that the English boys' expectations correspond more fully with the predicted outcomes in early adolescence, and it is clear that both social origins and ability contribute significantly to an explanation of their expectations. In contrast, the young American boys' expectations have little relation to either their social origins or their ability. All of this is consistent with the anticipated greater realism in England and with the English system's emphasis on ability in the process of sponsored mobility.

Yet, the detailed analysis indicates that the English boys' expectations are more strongly influenced by ability than actual educational attainments are. If anything, the English boys are overly impressed with the importance of ability. This overemphasis on ability continues into the fourth form, the point at which most English boys leave school. In contrast, the American boys' expectations become much less free-floating by the time they reach their senior year in high school, and their expectations are related to ability and social origin in a manner that is very similar to the relationship between these

antecedents and actual educational attainments. By that definition, the American seniors are the most realistic of all the groups of boys studied.

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INTELLIGENCE AND DELINQUENCY: A REVISIONIST REVIEW

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Recent research on intelligence and delinquency suggests that (1) the relation is at least as strong as the relation of either class or race to official delinquency; (2) the relation is stronger than the relation of either class or race to self-reported delinquency. In an analysis of the history of the research on the IQ-delinquency relation, we trace the developments leading to the current textbook position that IQ is not an important factor in delinquency. This position, which came into vogue about forty years ago and is still held by many sociologists, has its roots in: (1) a medical to sociological paradigm shift in this century; (2) the failure of subsequent research to substantiate the early exorbitant claims that low IQ was a necessary and sufficient condition for illegal behavior; (3) early negative reviews of research on this question by Sutherland and others; (4) reservations about the validity of the measurement of both IQ and delinquency; (5) erroneous interpretation of research findings; (6) speculation regarding factors which might account for the relation. It is noted that many currently prominent sociological theories of delinquency implicitly or explicitly use IQ as a crucial theoretical element. We show that IQ has an effect on delinquency independent of class and race, and we argue that this effect is mediated through a host of school variables.

Few groups in American society have been defended more diligently by sociologists against allegations of difference than ordinary delinquents. From the beginning, the thrust of sociological theory has been to deny the relevance of individual differences to an explanation of

delinquency, and the thrust of sociological criticism has been to discount research findings apparently to the contrary. "Devastating" reviews of the research literature typically meet with uncritical acceptance or even applause, and new theories and "new criminologies" are con-

structed in a research vacuum, a vacuum that may itself claim research support.

A major source of this stance toward individual differences is the notion widely held in the field of deviance that "kinds of people" theories are non- or even anti-sociological. Most of the major theorists in the area (Sutherland, Merton, Cohen, Becker) have more or less explicitly argued this point, and efforts to bring criminology "up-to-date" with the rest of sociology frequently imply that interest in individual differences is an outmoded relic of the field's positivistic past (e.g., Matza, 1964; Taylor et al, 1973). Another source of this stance toward difference is frankly moral. According to Liazos (1972), who provides extensive documentation, sociologists repeatedly assert that deviants are "at least as good as anyone else." If Liazos' analysis is any guide, we may assume it is easy to, confuse the moral-evaluative "as good as" with the empirical "the same as." For example, Liazos goes on to argue that the repeated assertion that "'deviants' are *not different*" may raise the very doubts we want to dispel." Sociologists have observed for some time that, "always and everywhere, *difference* is the occasion and excuse for ignoring the equal claims of others" (Ross, 1901:25). They therefore feel duty-bound, it seems, to protect delinquents from those who would justify abusing them on these grounds.

Among the many possible individual differences between delinquents and non-delinquents, none is apparently more threatening to the integrity of the field and to its moral commitments than IQ. To the standard list of scientific and moral arguments against IQ, the sociological student of crime and delinquency can add the weight of a half-century struggle against biological theories and the predatory social ethic they are alleged to foster. In fact, the single argument against IQ developed within criminology is sufficiently simple and persuasive that the standard list need not be invoked. At the time criminology became a subfield of sociology, marked differences in IQ between delinquents and nondelinquents were pretty much taken for granted, and a major task confronting those wishing to

claim the field for the sociological perspective was to call these alleged differences into question. This task was successfully accomplished. IQ, it was confidently suggested, doesn't matter (see Sutherland, 1924:108). Today, textbooks in crime and delinquency ignore IQ or impatiently explain to the reader that IQ is no longer taken seriously by knowledgeable students simply because no differences worth considering have been revealed by research.

As we shall show, the textbooks are wrong.¹ IQ is an important correlate of delinquency. It is at least as important as social class or race. This fact has straightforward implications for sociological theorizing and research, most of which has taken place within the context of official denial of IQ differences. Its implications for social policy are variably straightforward and are, in any event, strictly irrelevant to questions of the current impact of IQ on delinquency: the actual relation between IQ and delinquency must be the standard against which all arguments, including our own, are judged.

The Current Textbook View

Many textbooks do not even mention IQ (e.g., Gibbons, 1970; Bloch and Geis, 1962). Most, however, introduce the subject and then argue against its significance. The basic position is that there are no differences in IQ between delinquents and nondelinquents. The research and reviews most frequently cited in support of this conclusion are now over forty years old (e.g., Murchison, 1926; Sutherland, 1931; Zeleny, 1933). The tendency to rely on summaries provided by other textbooks, especially, in this case, those written by psychologists, is much in evidence.

Despite the selectivity of textbook summaries of the evidence, most of them leave the reader with the distinct impression that IQ may be a very important

¹ In a more general treatment of the measurement and correlates of delinquency, Gordon (1976) independently reaches conclusions about the importance of IQ that are very close to those reported here.

cause of delinquency after all. Few textbook writers seem able to resist additional arguments that have the effect of undercutting their basic position:

It is now generally recognized that so-called intelligence tests tend to measure the degree to which the individual has assimilated and internalized middle-class values rather than intelligence.

We could anticipate that a feeble-minded individual would be more readily incarcerated than other individuals. (Haskell and Yablonsky, 1974:216)

It is not mental deficiency per se which results in crime; rather the inability of a mentally deficient person to make adequate social adjustments. . . . (Johnson, 1968:173)

Although a higher percentage of delinquent children come from the ranks of the mental defective, particularly from those of borderline intelligence, it is not the mental deficiency per se but the inability of the child to make adequate school or social adjustments that usually results in delinquency. (Sutherland and Cressey, 1974:174, quoting Coleman, 1950)

The great proportion of persons with low intelligence scores undoubtedly are non-deviants, whereas there are large numbers of persons with above normal intelligence who are. (Clinard, 1968:170)

All of these arguments take for granted a negative correlation between IQ and delinquency. The "middle-class values" interpretation of IQ tests suggests that scores on these tests may well be the strongest predictor of delinquency available. The "not per se" argument asserts that the relation is, in fact, causal in the usual meaning of the term—i.e., non-spurious. The "more readily incarcerated" view contradicts the "not per se" argument by suggesting a direct link between IQ and, at least, official delinquency. And the "great proportion" argument asserts only that the relation is not perfect. Still, the current view, simply stated, is that IQ makes no difference. This view is not supported by the results of research.

Recent Research on Official Delinquency

At least half a dozen recent studies permit examination of the effects of IQ on

official delinquency. These studies have been conducted in diverse settings, they rely on a variety of measures of IQ and of delinquency, and they all employ some measure of control for the effects of such variables as social class and race. All of them show IQ to be an important predictor of official delinquency.²

How strong is this effect? Since social class and race are considered important correlates of official delinquency by almost everyone, they should provide a sufficiently stringent criterion and be available for comparison. Further, since both class and race are frequently used to discount the effects of IQ, this comparison will provide evidence relative to the common argument that IQ effects are merely a by-product of race and class effects.

IQ, Social Class and Official Delinquency

Reiss and Rhodes (1961) examined the juvenile court records of more than 9,200 in-school white boys in Davidson County, Tennessee. Using three-category divisions on occupational status of the head of household and on IQ, they found that the rate (per 100) of court adjudication ranged from 5.7 in the high to 9.6 in the low status groups, and from 4.8 in the high to 10.3 in the low IQ groups. In other words, the rate of adjudication in the lowest occupational group was 1.7 times that of the highest occupational group, while the rate of the adjudication in the lowest IQ group was 2.1 times that of the highest IQ group.³ Since the distributions of occupational status and IQ were roughly comparable, in the Davidson County data IQ is more important than social class as a predictor of official delinquency among white boys.

Hirschi (1969) examined the police rec-

² Unless otherwise noted, all references to "the relation between IQ and delinquency" assume an inverse correlation.

³ When father's occupational status was dichotomized and IQ trichotomized, the two variables were shown to have independent effects, with some tendency toward interaction: the effects of occupational status were more marked as IQ decreased, which also says that the effects of IQ were more marked for blue-collar than for white-collar boys.

ords of over 3,600 boys in Contra Costa County, California. Since previously published analyses do not directly compare the effects of social class and IQ, we have reanalyzed these data, with the results shown in Tables 1 and 2 (for details of data collection, see Hirschi, 1969:35-46).

In these data, the effect of IQ on official delinquency is stronger than that of father's education. Among whites, the gamma for the relationship between IQ and delinquency is $-.31$, while the comparable gamma for father's education is $-.20$; among blacks, the gammas are $-.16$ and $-.05$, respectively. Although the data are not shown, a composite measure of family status which includes employment and welfare status, presence of the father, and education and occupation of the parents shows results comparable to those for father's education in both racial categories. For whites, the gamma is $-.18$; for blacks, it is $-.09$. When the effects of this measure of family status and IQ are examined *simultaneously* within racial groups, the results are consistent with the zero-order relations. Both family status and IQ are independently related to official delinquency; the superiority of IQ in comparison with family status, however measured, is especially noticeable among blacks.

Wolfgang et al. (1972) obtained IQ scores on 8,700 of the 10,000 boys in their Philadelphia cohort. They do not present measures of association for these IQ scores and delinquency, nor do they show tabular material in which IQ is treated as an independent variable. They do, however, present average IQ scores by

number of contacts with the groups homogeneous on class. The differences in average scores for chronic offenders and non-offenders range from nine IQ points among low socioeconomic status nonwhites to thirteen IQ points among low socioeconomic status whites (Wolfgang et al. 1972:93). Again, although no direct comparison with social class is possible, the Philadelphia data reveal a strong relationship between IQ and delinquency independent of social class.

West (1973:84) followed 4,000 boys over a ten-year period and compared the delinquent and non-delinquent groups on the prevalence of low IQ. In the same way [he] compares other factors such as poverty, family status, or criminal parents.' The relationship between IQ and delinquency in West's data is substantial. One-quarter of those with IQ scores below 90 more had a police record, than one-half of those with IQ scores of 90 or less. Even more impressively, only one in fifty boys with an IQ of 90 or less was a recidivist, one in fifty with an IQ of 90 or less fell in the recidivist category. West (1973:84-5) concludes from his thorough analysis that 'low IQ is a significant precursor of delinquency to much the same extent as other factors.' Although he reports a strong relationship between family income and delinquency, in American studies, IQ was able to explain delinquency on equal terms and to supplant family income and several other factors of family culture were controlled in the matching procedure.

It should be noted that the differences in delinquency produced by IQ in West's data reflect a difference of about 12 points between non-offenders and recidivists—a difference within the range of the race-specific differences calculated from Wolfgang et al. data. West's results, with those of Wolfgang et al., suggest that the effect is largely attributable to chronic offenders (recidivists), which may

Table 1. Percent Committing Two or More Official Delinquent Acts by IQ (Stanford Binet) and Race^a

	IQ				
	0-19	20-39	40-59	60-79	80-99
White males	22.6 (204)	25.6 (282)	14.6 (309)	8.4 (341)	6.2 (403)
Black males	38.2 (429)	36.2 (273)	26.2 (153)	19.7 (71)	19.0 (42)

^a IQ scores are shown as percentiles. Gammas, calculated on the entire range of delinquency scores (0-4), are $-.31$ for whites and $-.16$ for blacks.

⁴ Wolfgang et al. used the Philadelphia Ability Test. The typical IQ test has a standard deviation of 15.

Table 2. Percent Committing Two or More Official Delinquent Acts by Father's Education and Race^a

Race	Father's Education				
	Less than High School Grad.	High School Graduate	Trade or Business	Some College	College Graduate
White Males	17.7 (356)	14.3 (485)	13.4 (82)	8.0 (201)	7.8 (306)
Black Males	33.8 (343)	34.4 (209)	42.1 (57)	30.8 (123)	19.1 (84)

^a Gammas, calculated on the entire range of delinquency scores (0-4), are -.20 for whites and -.05 for blacks.

the relatively weak performance of IQ in studies of self-reported delinquency.

IQ, Race and Official Delinquency

Comparison of the effects of race and IQ is more difficult than the class-IQ comparison because of a greater paucity of data or, at least, of appropriately analyzed data. There can be no doubt that IQ is related to delinquency within race categories. All of the studies mentioned are consistent on this point. The relative strength of the two variables is, however, open to question.

The multiple regression analysis using number of offenses as the dependent variable presented by Wolfgang et al. (1972:275-9) includes both race and IQ. Unfortunately for present purposes, it also includes highest grade completed and number of school moves, variables which account for the bulk of the explained variance in this measure of delinquency. Thus, the fact that race places third behind these school variables and IQ accounts for virtually nothing cannot be taken as direct evidence of their relative importance. We know that IQ is strongly related to delinquency in the Wolfgang

data independent of race. We know, too, that IQ is strongly related to the school variables ($r=.468$ for highest grade completed) that, in variance terms, do most of the work. Therefore, we know that if these intervening variables were excluded from the analysis, the proportion of variance accounted for by IQ would increase substantially.

In the Contra Costa data, IQ and race have virtually identical effects on official delinquency. For illustration, we compare a dichotomous measure of IQ with the two categories of race in Table 3.

Measures of association between IQ and delinquency and between race and delinquency reflect the percentage differences in Table 3: race and IQ are virtually identical in their ability to predict delinquency. For race, $r=.26$; for IQ, $r=.27$.

The findings of McCord and McCord (1959:66, 203) from the Cambridge-Sommerville Youth Study are sometimes cited (e.g., West, 1973:91) as showing "no connection between low IQ and delinquency." Although in the McCords' data those in the lowest IQ group (80 or below) did have an intermediate rate of conviction during the follow-up period,⁵ within the normal range of IQ scores (above 80) there was a monotonic decrease in rates of conviction from almost one-half in the 81-90 IQ group to one-quarter in the 110 or more IQ group. Because those in the lowest IQ group are only ten percent of

Table 3. Percent Committing Two or More Official Delinquent Acts by IQ and Race^a

Race	IQ	
	Low	High
White males	24.3 (486)	9.4 (1053)
Black males	37.6 (702)	23.3 (266)

^a IQ scores dichotomized at the 40th percentile.

⁵ Our figures are for the experimental and control groups combined (McCord and McCord, 1959:66, 203). Strictly speaking, the McCord data apply to adult criminality as well as juvenile delinquency, since the average age of their subjects was 27 at the time data on convictions were obtained.

the sample, the McCords' data, too, show an inverse relation between IQ and official misconduct.

Such problems of interpretation do not arise in Short and Strodtbeck's (1965) study of gang delinquency in Chicago. They report that gang boys scored lower on "all six intelligence measures" than non-gang boys in the same (lower) class; this difference held for white and black respondents alike.

Toby and Toby (1961) found "intellectual status" to be a significant forerunner of delinquency independent of socioeconomic status. And Reckless and Dinitz (1972) found that their teacher-nominated "good" boys had IQs from 8 to 12 points higher than their teacher-nominated "bad" boys in a class-homogeneous area.⁶

All in all, it seems reasonable to conclude on the basis of currently available data that IQ is related to official delinquency and that, in fact, it is as important in predicting official delinquency as social class or race. We know of no current research findings contrary to this conclusion.

Self-Reported Delinquency

A significant consequence of the no-IQ-difference position was that it helped set the stage for extensive use of self-report methods of measuring delinquent behavior. This position explicitly asserts that delinquents are as likely as others to possess the various skills reflected by IQ tests. If, however, the assumption of equal ability is unfounded, the measurement of delinquent behavior by the self-report method may be confounded with IQ, i.e., those most likely to commit delinquent acts may be least able to report adequately on their behavior. The self-report method, especially questionnaires,⁷

therefore does not provide an unambiguous test of the hypothesis that IQ is related to delinquent behavior.

In any event, most studies do find a relation between IQ and self-reported delinquency, but this relation is less robust than that found in official data. At one extreme, West (1973:158) found that 28.4 percent of the worst quarter of his sample on self-reported delinquency had low IQs, as compared to 16.6 percent in the remaining three-quarters—a difference only slightly smaller than his finding for official delinquency.

Weis (1973), too, found differences as strong as those typically reported when delinquency is measured by official data. In his study in a white upper-middle-class community near San Francisco, Weis collected Wechsler-Bellevue IQ scores and self-reports of delinquency for 255 male and female eleventh-grade students. One of the clusters emerging from his analysis was a property deviance scale that included items on theft, burglary, shoplifting and vandalism. When these scores were trichotomized, Weis found that 27 percent of those with IQ scores of less than 110, and 49 percent of those with IQ scores of 110 or more, had low scores on the property deviance scale. He found a similar difference (23% versus 41%) on a social deviance scale that included items on marijuana, alcohol and gambling.⁸

More typical of self-report studies, however, are the relations from the Contra Costa data shown in Table 4. Among white males, twice the proportion in the lowest as in the highest IQ group report involvement in two or more of a possible six delinquent acts; among black males the comparable ratio is 3:2.

Whatever the strength of the relations in Table 4, we believe they should be evaluated by comparison with social class and race. As Table 4 shows, race has no impact on self-reported delinquency—a finding consistent with much of the self-report literature (e.g., Williams and Gold, 1972). The same literature has consistently re-

⁶ The Toby-Toby and Reckless-Dinitz studies may be marginal to the question of IQ effects. However, this concern would carry greater weight if their results were contrary to research focusing directly on the IQ question.

⁷ Early warnings that the questionnaire method is especially limited by the high rates of illiteracy among delinquents (Erickson and Empey, 1963) have gone essentially unheeded.

⁸ For details of data collection, see Weis (1973). The data reported in the text cannot be found in Weis' dissertation. We are grateful to him for making them available to us.

Table 4. Percent Committing Two or More Self-Reported Delinquent Acts by IQ and Race ^a

Race	Low IQ				High IQ
White males	24 (196)	26 (270)	20 (302)	19 (336)	12 (396)
Black males	27 (393)	26 (257)	19 (149)	19 (68)	18 (39)

^a IQ scores are grouped in percentiles as in Table 1. Gammas, calculated on the entire range of delinquency scores (0-6), are -.15 for whites and -.07 for blacks.

vealed a weaker relation of social class (e.g., Nye et al., 1958; Akers, 1964) to self-reported delinquency than that found in Table 4. The weight of the evidence is that IQ is more important than race and social class. The voluminous criticisms advanced against self-report delinquency research—with an eye to rescuing social class—presumably would have the same or even greater consequences for IQ. For example, the heavy reliance on in-school populations, the overabundance of minor offenders, and the dependence on subject cooperation may work to attenuate the relationship between social class and delinquency. If so, there is reason to believe that these factors would also depress the relation between self-reported delinquency and IQ. In fact, Hirschi (1969:46) reports that among those with the highest grades in English who had no police records, 79 percent cooperated with the self-report survey, while among those with the lowest grades in English who had police records, only 38 percent cooperated. More importantly, not only did grades in English and official delinquency substantially affect cooperation with the self-report survey, the two factors were found to interact: low ability boys with police records were disproportionately unlikely to appear in the self-report sample. Since official delinquents are likely to be "self-report" delinquents (if sampled), the number of self-reported delinquents in the sample is considerably depressed, especially at the low end of the ability scale.

In short, however delinquency is measured, IQ is able to compete on at least equal terms with class and race, the major

bases of most sociological theories of delinquency. At the same time, a relation between IQ and delinquency is routinely denied in sociological textbooks.

Implications for Theory

Our original purpose in introducing theory was frankly argumentative: we expected to find theorists struggling with a conflict between their own logic and the erroneous "results of research" on IQ. In short, we expected to find that they had often been led astray by the anti-IQ climate of criminology.

Actual examination of currently influential theories required revision of our plans. In most cases, theorists were not paying all that much attention to the "results of research." We had been led astray by the naive textbook assumption that theory organizes research and research tests and modifies theory. In the case of IQ, however, it would be more accurate to say that theory opposes research and research ignores theory.

Theories from the period (Merton, 1938; Sutherland and Cressey, 1974) when most researchers considered low IQ a strong correlate of delinquency ignore this variable,⁹ while theories from the period when IQ was almost universally considered irrelevant predict either very strong negative (Cohen, 1955) or weak but important positive relations (Cloward and Ohlin, 1960) with delinquency. And a theoretical tradition (labeling) spanning both periods has managed to take a position opposite to research in both of them. Although all of these theories have been heavily researched, investigators have paid little or no attention to their views regarding IQ.

Since it is difficult to argue with those who agree, we will briefly show that resistance to consideration or inclusion of IQ does not characterize any current theory; that, on the contrary, several important theories require a relation between IQ and delinquency. Explicit recognition of this

⁹ The Gluecks reported periodically throughout the thirties that their delinquents were "burdened with feeble-mindedness" (e.g., Glueck and Glueck, 1934).

fact would only increase their scope, the plausibility of their claims, and their consistency with research findings.

The best example is Cohen's (1955) effort to relate social class to delinquency by way of differential experience in the educational system. In Cohen's theory, children differentially prepared or qualified encounter a school system that treats all comers alike. Children inadequately "prepared" for success in school find the experience painful and are likely, as a consequence, to turn to delinquency. The place of IQ in this process would seem obvious and, in fact, Cohen (1955:102-3) could not be more explicit on this question:

It may be taken as established that ability, as measured by performance in conventional tests of intelligence, varies directly with social class. . . . The conventional tests do test for abilities that are highly prized by middle-class people, that are fostered by middle-class socialization, and that are especially important for further achievement in the academic world and in middle-class society. In short, *the results of these tests are one important index of the ability of the child to meet middle-class expectations, to do the kinds of things that bring rewards in the middle-class world.* (emphasis added)

In Cohen's theory, intelligence intervenes between social class and delinquency or it is at least an important indicator of the social class of the *child*. In either case, IQ should be more strongly related to delinquency than such indirect measures of the ability of the child to meet middle-class expectations as "*father's* occupation."

Cohen's views on the interchangeability of IQ and class illustrate how the former could have been used to extend the scope of his theory beyond the confines of "lower-class delinquency." The situation facing the middle-class child with low IQ may not be all that different from the situation facing the lower-class child and, if such a situation explains the delinquency of one of them, it may explain the delinquency of the other as well. If both lower- and middle-class delinquency can be explained by the same mechanism, Cohen's reliance on a separate mechanism for middle-class boys (Cohen, 1955:162-9) is

inexplicable or is, at the very least, theoretically and empirically inelegant.

If a zero relation between IQ and delinquency would falsify Cohen's theory, it would virtually falsify the theory of Cloward and Ohlin (1960) as well, but for quite different reasons. Cloward and Ohlin (1960:111) suggest a positive relation between intelligence and delinquency:

Some persons who have experienced a marked discrepancy between aspirations and achievements may look outward, attributing their failure to the existence of unjust or arbitrary institutional arrangements which keep men of ability and ambition from rising in the social structure. Such persons do not view their failure as a reflection of personal inadequacy but instead blame a cultural and social system that encourages everyone to reach for success while differentially restricting access to the success-goals. In contrast to this group there are individuals who attribute failure to their own inadequacies—to a lack of discipline, zeal, intelligence, persistence, or other personal quality.

In other words, the lower-class boy with a high IQ whose talents go unrecognized and unrewarded is a prime candidate for delinquency.

On the basis of available evidence, Cloward and Ohlin are wrong. For present purposes, however, the point is that their theory requires research on the IQ of juvenile offenders and is enduring testimony to the dangers in the view that IQ need be "no longer seriously considered" by criminologists.¹⁰

At first glance, labeling theory would seem to be an exception to our argument that IQ is important, since this theory puts no stock in the notion that individual differences may act as causes of delinquent behavior. In one of the first efforts by a labeling theorist to neutralize individual-difference research, Tannenbaum (1938:6) focused special attention on IQ, arguing that "whatever 'intelligence' is, it has no demonstrated relationship to crime." As labeling theory has "progressed," how-

¹⁰ Although Merton (1938) ignores IQ and its "success" implications, IQ is obviously relevant to any opportunity theory.

ever, as it has become more closely associated with the conflict perspective according to which "society organizes itself for the protection of the ruling classes against the socially inferior" (Doleschal and Klapmuts, 1973:622), it has tended more and more to recognize that it too is dependent on individual differences. The generally low IQ of official delinquents is now accepted by labeling theorists and is used as evidence for their view that the system discriminates against or creates the disadvantaged (Doleschal and Klapmuts, 1973:612, 616; Polk and Schafer, 1972:34-54).

If labeling theorists argue that discrimination produces the relation between IQ and delinquency, then the mechanism that connects IQ to delinquency is the bone of contention between labeling and conventional theories—not the fact of a relation itself. We will return to the mechanism question.

Perhaps the only major theory strictly silent on the question of IQ is Sutherland's "differential association" (Sutherland and Cressey, 1974:75-7). Sutherland (1931) played a major role in constructing the current position of criminology on IQ. He rejoiced in its alleged failure to discriminate between delinquents and nondelinquents, and his influential text continues to belittle "mental testers" to the present day. Even so, differential association has nothing to fear from intelligence. This theory faintly suggests a positive association among those exposed to the delinquent culture (as does any theory that emphasizes the need to learn crime), but it really cannot be used to predict even the sign of the relation in the general population. If the theory cannot predict the sign of this relation, it is, nonetheless, capable of accounting for any relation between IQ and delinquency that might be revealed by research.

A final set of theories might be grouped under the heading of "social control" (for a convenient summary, see Nettler, 1974). These theories focus on a broad range of causal variables, and they are relatively open to individual differences, to the idea that "in learning to conduct ourselves, some of us need more lessons than

others" (Nettler, 1974:232). Although none of them may now consider IQ of central importance, most suggest a negative association, and none would have difficulty absorbing this variable. In fact, for those sociologically-oriented control theories that emphasize "stakes in conformity" (e.g., Toby, 1957), IQ is of obvious importance.

Most sociological theories, then, have been saying for some time that IQ should be related to delinquency for the same reason that social class is, or should be related to it. Given the theoretical overlap of IQ and social class, the contrast in how the research community has reacted to their varying fates would be hard for an outsider to understand.

The finding that social class was unrelated to self-reported delinquency produced a large volume of follow-up research. The "finding" that IQ was unrelated to any measure of delinquency was, in contrast, accepted without so much as a murmur of protest. The literature on IQ contains none of the "what may have gone wrong" kinds of methodological critiques so often encountered in efforts to save social class. Instead, it is marked by considerable speculative ingenuity directed against an established relation. The extent to which this relation has been established may be revealed by a review of the history of IQ testing as it applies to delinquency and crime.

History

As a cause of delinquency, IQ got off to a very strong start in the first years of this century. The notion that "imbeciles" and "idiots" would be unable to resist criminal impulses or, for that matter, even to distinguish right from wrong, was a straightforward extension of Lombroso's then prestigious theory of the born or biologically defective criminal. Initial research did nothing to dampen enthusiasm for this idea. Goring (1972:255) in Great Britain reported that criminals "as a class, are highly differentiated mentally from the law abiding classes," and Goddard (1914:7) in the United States concluded that "probably from 25% to 50% of the

people in our prisons are mentally defective and incapable of managing their affairs with ordinary prudence." In the period 1910–1914, the "percentage feeble-minded" in fifty studies of institutionalized delinquents had a median value of 51 (Sutherland, 1931:358). Since it was then assumed that the proportion feeble-minded in the general population was less than one percent (Goring used an estimate of .46 percent), the conclusion that faulty intelligence was the "single most important cause of crime" followed, or at least seemed to follow directly from the evidence.

If we follow the fate of IQ through mainstream criminology, we discover that its day was very brief. Less than two decades after Goring estimated .6553 as a "minimum value" for the correlation between mental defectiveness and crime, Sutherland (1931) was poking fun at the absurdities of the "mental testers."¹¹ His negative review of their research was so influential that the "modern" or "recent" position on IQ described by today's textbooks appears to have been firmly established at that time, i.e., forty-five years ago.

Sutherland's stance is not difficult to understand. As Savitz (1972:xviii) has reminded us, the medical profession seized power in criminology before the end of the nineteenth century and still maintained a preeminent position in the early days of intelligence testing—both Goring and Goddard were physicians. A short time later, however, criminology had become a subfield of sociology. Given this shift in disciplinary dominance, an equivalent

paradigm shift is now pretty much accepted as a logical necessity. "Intelligence" was a central element of the "old" paradigm. It just had to go. And go it did.

The history of IQ in research findings is not so quickly or easily told. The initial claims about the proportion of feeble-minded delinquents were excessively high because—as Merrill (1947:159) has pointed out—researchers were basing their cutting point on children in institutions for the mentally deficient. The logic of this procedure went something like this: if no child in an institution for the feeble-minded has a mental age in excess of twelve, then a mental age of twelve or less is sufficient to classify a person feeble-minded. There was nothing especially silly about this procedure, it merely made the mistake of assuming that the same procedure would not also classify a large portion of the general population feeble-minded. As it became apparent that a too-large portion of the general population would be classified feeble-minded, the mental age requirement was first abruptly and then gradually lowered, with the result that the proportion feeble-minded among delinquents also first abruptly and then gradually declined. Sutherland (1931) called attention to this twenty-year trend—which, in fact, continued for another 30 years (Woodward, 1955; Caplan, 1965)—and allowed his readers to conclude that it would continue until the initial claims of difference between delinquents and nondelinquents had no foundation in fact.

The most direct evidence against an IQ difference resulted from the extensive testing of the draft army in World War I. Murchison (1926) and Tulchin (1939) reported that the distribution of intelligence in the draft army was virtually identical to the distribution among adult prisoners. Without including details of the investigation, Murchison also reported that the prisoners in a certain midwestern institution were more intelligent than the guards, an anecdotal fact even now more widely quoted than the results of many carefully conducted studies showing important differences in favor of the intelligence hypothesis. Although Sutherland (1931:364) acknowledged that "serious questions

¹¹ Sutherland summarized about 350 studies conducted between 1910 and 1928 noting downward trends in the proportion feeble-minded in delinquent and criminal groups, as well as inconsistencies in the results. "In those early days of mental testing the influence of Goddard was very great; he had asserted that the more expert the mental tester the larger the proportion of delinquents he would find to be feeble-minded. Many of the testers attempted to demonstrate their superiority in that manner." "Consequently a report regarding the proportion of a delinquent group feeble-minded is of primary significance in locating the mental tester upon a scale of mental testing methods. In this sense the psychometric tests of delinquents throw more light upon the intelligence of the mental testers than upon the intelligence of delinquents." (Sutherland, 1931:358–62).

have been raised regarding the validity of these tests and the validity of using the draft army as a sample of the general population," he carefully noted that "the consistency in results is a fact that cannot be overlooked."

By the late 1920s and early 1930s, the evidence was sufficiently mixed that summaries of the research literature were arriving at variant conclusions. Thomas and Thomas (1928:365) concluded from their review of the same literature examined by Sutherland that important differences between delinquents and non-delinquents on IQ were "beyond question." They reached this conclusion by focusing on the many studies reporting such differences and by discounting the draft-army research as being so clearly out of line as to be suspect. In 1935, Chassell published an extensive review of research on this question. Her general conclusion, based on nearly 300 studies:

Undoubtedly the relation between morality and intellect in the general population is considerably higher than usually found in restricted groups. Nevertheless, it is hardly probable that this relation is high. Expressed in correlational terms, the relation in the general population may therefore be expected to fall below .70. (Chassell, 1935:470)¹²

As IQ tests improved, the average score of samples of delinquents also improved until, with the advent of the Revised Stanford Binet and the Wechsler-Bellevue scales in the late 1930s, they were obtaining an average IQ of about 92 (Merrill, 1947; Woodward, 1955; Caplan, 1965). With the advent of these improved tests about 35 years ago, the marked trends and occasional fluctuations of earlier research apparently came to an end. Since that time, it has been reasonable to expect that samples of delinquents would differ from the general population by about eight IQ points. This conclusion has been accepted by Woodward (1955) and Caplan (1965) in

major reviews of the literature and is generally consistent with the more recent research reviewed in this paper.

The question, then, is how a reliable eight IQ point difference was converted to the no-difference conclusion of the textbooks. One possibility is that an eight IQ point difference was not seen as theoretically or practically important. This possibility is easily disputed: no modern reviewer has questioned the importance of a difference of this magnitude.¹³ Assuming that ten percent of the population is delinquent, this difference would produce a correlation (Yule's Q) between IQ and delinquency of about -.4.

The neglect of IQ after a reliable and important difference had been established may be traced to the initial plausibility of an unusual number of counter-arguments. These arguments are so numerous and diverse that we can hope to deal with them only generally and briefly.

The Spuriousness Argument

Scholarly reviews of the literature have made much of the hypothesis that the low IQs of delinquents are a spurious consequence of differences in class or culture. Against the estimated eight IQ point difference between delinquents and non-delinquents, Woodward (1955) assembles a good deal of material suggesting the possibility that cultural factors are at work: the children of professionals differ from those of unskilled manual workers by about 20 IQ points; average IQ scores are low in *areas* with high delinquency rates;¹⁴ children in large families have low IQ scores and are more likely to be delinquent; overcrowding is related both to low IQ and to delinquency; finally, studies based on sib-sib comparisons (such as Healy and Bronner, 1936) and on other methods of control for cultural factors

¹² Present-day researchers would not be so modest about a correlation of .70! Chassell's caution may be indicative of the standards against which empirical relations were judged in the early days of quantitative research. These standards may account for the ease with which reviewers were able to reject IQ as a "significant" causal variable (see also footnote 1).

¹³ Caplan (1965:104) refers to this eight-point difference as a "first class" relationship. As noted below, however, he cautions the reader that cultural factors be taken into account before it is accepted as genuine.

¹⁴ This is an example of what might be called the reverse ecological fallacy: because IQ and delinquency are related at the ecological level, it is *unlikely* that they are related at the individual level.

"tend to support the contention that complete control *would* eliminate the difference between delinquents and non-delinquents" (Woodward, 1955:289; emphasis added).¹⁵ As we have seen, the evidence says otherwise. Differences by class and race do not account for IQ differences between delinquents and non-delinquents. These differences remain pronounced within groups homogeneous on these variables. If there exists a cultural correlate of both IQ and delinquency strong enough to account for the relation between them, it has not yet been identified.

Ten years after Woodward's influential review (see Wootton, 1959:302), Caplan (1965) was unable to find additional research material bearing directly on her cultural hypothesis. His conclusions about the effects of IQ are, however, if anything, more skeptical than Woodward's, because he is able to cite an additional source of concern.

Arguments Focusing on the Measurement of Delinquency

The advent of the self-report method helped Caplan (1965:120-1) call into question the measures of delinquency upon

which the original findings of IQ differences were based. Once again, the evidence against IQ was inferential rather than direct: if official data measure delinquency imperfectly, then imperfections in measurement rather than the phenomenon itself may account for the observed relation. And, indeed, few have been able to resist ascribing IQ differences between officially identified delinquents and non-delinquents to the ability of the bright delinquent to avoid detection or to differential response of officials to high and low IQ adolescents (e.g., Sutherland, 1931; Doleschal and Klapmuts, 1973; Stark, 1975).

Both the differential detection and differential reaction hypotheses require that IQ have a direct or independent effect on official delinquency.¹⁶ Such direct effect hypotheses compete with intervening variable hypotheses and may be directly tested when the latter are available. A competing hypothesis widely mentioned in the literature (e.g., Short and Strodtbeck, 1965:238; West, 1973:44) is that IQ affects delinquency through school performance. If IQ has the direct effect suggested by the differential detection and reaction hypotheses, nothing consequent to IQ can explain the zero-order relation. Two studies bear on this question. When Wolfgang et al. removed by statistical adjustment the effects of such intervening variables as highest grade completed, the relation between IQ and such "detection" measures as number of offenses virtually vanished (Wolfgang et al., 1972:275-9). (We have replicated this finding with the Contra Costa County data.) Taking a somewhat different approach, West (1973:217) also was able to reduce the relation between IQ and official delinquency below the significant level by matching on peer and teacher ratings on "troublesomeness." These ratings were made at ages eight and ten, well before the delinquent acts recorded by officials. Once again, then, *the*

¹⁵ Healy and Bronner (1936) controlled cultural factors by matching 105 delinquents with their same sex, nondelinquent sib nearest in age and then comparing IQ test scores. Although they found an IQ difference in favor of the nondelinquents, this difference was not statistically significant and was not interpreted as practically or theoretically significant by them. (Thirty-four percent of the delinquents and 26 percent of the nondelinquents had IQs under 90.)

The difficulty with this widely cited study (e.g., Wootton, 1959) is that its design makes the outcome a statistical necessity. Pushing the logic of Healy and Bronner's matching procedure one step further, we would compare identical twins raised together, only one of whom was delinquent. Since the correlation between the IQs of identical twins raised together is about .87, a figure "nearly as high . . . the correlation between two parallel tests for the same individual" (Eckland, 1967:177), we would be asking whether errors in IQ measurement are related to delinquency. By the same token, knowing that the "control" is a brother or sister reared in the same household tells us a good deal about what to expect in the way of IQ (in most studies the sib-sib correlation is in the neighborhood of .55), and there is little reason to expect the original relation to survive with anything like its "natural" magnitude.

¹⁶ Contrary to the "intelligence per se is not a cause . . ." arguments with which it is often paired, the differential detection argument suggests that, in fact, intelligence per se *is* a cause of delinquency—when delinquency is measured by official records.

differential ability to avoid detection and the differential official reaction on the basis of IQ arguments are not supported by available evidence. (The tests of the official reaction hypothesis are limited by available data to reactions by the police.)

Tests of these and related direct effect hypotheses¹⁷ at the same time identify the mechanism linking IQ to delinquency. This mechanism, the data suggest, is performance in and attitudes toward the school. That school variables are strong enough to account for the impact of IQ should come as no surprise. Their significance for delinquency is nowhere in dispute and is, in fact, one of the oldest and most consistent findings of delinquency research (e.g., Thrasher, 1963; Gold, 1970; Hindelang, 1973; Weis, 1973). What should come as a surprise is the easy acceptance of the no-difference-on-IQ conclusion, since the consequences of IQ differences are generally accepted as major predictors of delinquency. This brings us to the most troublesome of the arguments against IQ effects.

Arguments Focusing on the Measurement or Meaning of IQ

The facts we have presented compete with a wide variety of counter-arguments that focus on the meaning or measurement of IQ: "anybody can learn anything" (Eckland, 1967:174-5, quoting Faris, 1961:838), "it is impossible to make intelligence part of any respectable theory" (ASR referee, 1975), "so-called intelligence tests measure only 'test intelligence' and not innate intelligence" (Clinard, 1968:170), and "mainly they [IQ tests] measure the socioeconomic status of the respondent" (Chambliss and Ryther, 1975:373). Excellent discussions of many of these issues are available in the sociological literature (Eckland, 1967;

Gordon, 1975). We will deal only with those counter-hypotheses that have a direct bearing on the relation between IQ and delinquency and that can be addressed to some extent using data already presented.

The cultural bias of IQ tests. The argument against IQ tests most frequently encountered in the sociological literature is that these tests are biased against low-income and minority group children. Specific test items (e.g., "What color are rubies?") are often presented to show the obviousness of this bias (Chambliss and Ryther, 1975:373). Since the groups said to be discriminated against by IQ tests are the same groups with high rates of delinquency, the cultural bias hypothesis is certainly plausible. In form, it is identical to the traditional cultural hypothesis previously encountered and may be tested using the same data. These data show that the bias hypothesis is inadequate: important differences in IQ between delinquents and nondelinquents *within* race and class categories cannot be explained by argument or evidence that these tests are biased in favor of middle-class whites.

The stability of test scores. To the extent that IQ test scores are unstable and subject to subtle social influence; the meaning of a correlation between IQ and delinquency is open to question. It may be that reaction to the misbehavior of the child influences his IQ, that the low IQ child today may be the high IQ child tomorrow, and so on. These possibilities are summarized in assertions that "the scores are highly unstable through time" (Polk and Schafer, 1972:195). Unfortunately for such assertions, they are not consistent with the evidence: the IQs of children at four or five years of age have a correlation of about .7 with their IQs at age 17 (Bloom, 1964); after age ten, test-retest correlations (regardless of the number of years between the tests) fall between the test's reliability and the square of its reliability (Jensen, 1969:18). For that matter, the ability of IQ tests to predict delinquency at some period far removed from their administration is inconsistent with the gross implications of the instability argument.

A fall-back position for those who

¹⁷ Other very old direct effect hypotheses are that IQ differences stem from (1) the inability of the unintelligent to understand distinctions between right and wrong or (2) their inability to foresee and appreciate the consequences of their acts. These hypotheses assume that low IQ children are more likely to be delinquent, regardless of the social consequences (e.g., school difficulties) of their lack of IQ. Again, current data do not appear to support hypotheses of this form.

would argue instability is that these scores *could be* manipulated by simple and straightforward shifts in the environment of the child:

We may treat people differently out of ignorance or prejudice, but the result is the same as if the supposed differences were real. Studies have shown that school children seen as liable to be educationally backward become educationally backward and that, vice versa, children seen as educationally capable become educationally capable. (Taylor et al., 1973:142; see also Polk and Schafer, 1972:46; Schur, 1973:164)

The study cited in support of such arguments is Rosenthal and Jacobson, *Pygmalion in the Classroom* (1968). In this study, students in grades K through 5 in one elementary school were given group-administered IQ tests at the end of the 1964 academic year. The following fall, a random 20 percent of the students were identified to their teachers as students expected to show unusual intellectual gains during the academic year. In May, 1965, all students were re-tested on the same IQ test. Although both the experimental and the control subjects showed IQ gains, the experimental group showed a 3.8 point greater gain, with the bulk of this gain coming in the first and second grades. On the basis of these results, Rosenthal and Jacobson (1968:98) conclude that favorable expectations of teachers "can be responsible for gains in their pupils' IQ's and, for the lower grades, that these gains can be quite dramatic."

Unfortunately, *Pygmalion* has problems. Snow (1969:197) asserts that the study "stands as a casebook example of many of Darrell Huff's (*How to Lie with Statistics*) admonitions to data analysts" and that it "fails to come close to providing an adequate demonstration of the phenomenon" (the effects of teacher expectations on IQ scores). Thorndike (1968:708) begins his similarly negative review with what has turned out to be a prophetic statement:

In spite of anything I can say, I am sure it (*Pygmalion in the Classroom*) will become a classic—widely referred to and rarely examined critically. Alas, it is so defective technically that one can only regret that it ever

got beyond the eyes of the original investigators!

Thorndike concludes that "the basic data . . . are so untrustworthy that any conclusions based upon them must be suspect." And, indeed, this too was prophetic. Elashoff and Snow (1971) report that *none of nine attempts to replicate the effects of teacher expectations on IQ scores has been successful*. One would think that this would be enough to put an end to the "Rosenthal effect." However, Beeghley and Butler (1974:750) still maintain that the effects of teacher expectations on IQ "have been forcefully demonstrated by Rosenthal and Jacobson," and they muddy the waters by citing two "replications" of *Pygmalion*. In the first, "changes in intellectual functioning were not expected" by the investigators themselves (Meichenbaum et al., 1969:307) and in fact, as far as we can determine, IQ was not even a variable in the study. In the second, the author summarizes a variety of research results and concludes the findings do not "provide any direct proof that teacher expectations can influence pupil performance" (Pidgeon, 1970:126). Ironically—for a study which Beeghley and Butler purport to be a replication of *Pygmalion*—Pidgeon (1970:126) notes that the Rosenthal and Jacobson study "would bear repetition, providing conditions could be found for employing a more satisfactory research design." As of now, it is clear that no labeling or expectation effects of the sort alleged by Rosenthal and Jacobson (and widely cited in the crime and delinquency literature) have been established.

Conclusions

The assertion that IQ affects the likelihood of delinquent behavior through its effect on school performance is consistent with available data. The corollary descriptive assertion that delinquents have lower IQs than nondelinquents is firmly established. Both of these assertions are inconsistent with the "no-IQ-difference" view of the textbooks. They are clearly inconsistent with the image of the delinquent in much sociological writing on the subject, and those planning prevention and treat-

ment programs would do well to take them into account.¹⁸

Interestingly enough, most modern theories of delinquency assume (and some explicitly state) that IQ affects delinquency. That their views have been ignored by researchers testing them speaks to the depth of the concern that individual differences are both non-sociological and positively dangerous. In this sense, IQ is doubly significant in that it represents an entire class of variables traditionally ignored by sociological students of crime and delinquency. Variables in this large residual category (virtually everything beyond class, culture, and official processing) will not lose their status as alternative hypotheses simply by being ignored, and they will continue to restrict and even embarrass sociological theory until some effort is made to incorporate them.

For that matter, IQ is a poor example of a variable that may require modification of sociological perspectives. As of now, there is no evidence that IQ has a direct impact on delinquency. The police bias, differential ability to avoid detection, and inability to appreciate moral distinctions hypotheses are not consistent with current data. If the mechanism linking IQ to delinquency is school performance and adjustment, then IQ does not lead away from the arena in which sociological theories have focused their quest for the antecedents of delinquency; rather, it helps illuminate the social processes occurring there.

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REDISCOVERING DELINQUENCY: SOCIAL HISTORY, POLITICAL IDEOLOGY AND THE SOCIOLOGY OF LAW*

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This paper examines a Marxian social historical approach to the study of legal evolution. The emergence of the Marxian perspective and the logic of its premises are reviewed. Using Canadian delinquency legislation as an historical example, it is found that the Marxian perspective assumes a great deal that is unconfirmed (e.g., that this legislation serves the teleologically inferred "basic interests" of an ambiguously identified "ruling class"), asserts other things that are wrong or misleading (that this legislation increased imprisonment, "invented" new categories of youthful misbehavior, created a "specialized labor market" and increased "industrial discipline"), and ignores much that an organizational analysis helps to reveal (that the emergence of probation work as an organizational concern was the prime factor in the development of Canadian delinquency legislation). Implications of these findings are considered.

It is now common to begin sociological discussions of deviance by reaffirming the shift, in the 1960s, from studying the antecedents of rule-breaking behavior to a concern with the origins of legal norms and the statuses that may or may not follow their violation. Equally noteworthy, however, is an increasingly apparent shift from studying the entrepreneurial and organizational origins of legal norms (e.g., Becker, 1963; Gusfield, 1963; Lemert, 1970; Dickson, 1968) and their enforcement (e.g., Wheeler, 1968; Reiss, 1971; Blumberg, 1967), to a more monotheistic focus in the 1970s on class conflict as the independent variable of concern (Taylor

et al., 1973; Quinney, 1975a; Chambliss, 1973; Platt, 1975).

Several recent papers have questioned the accuracy of class conflict propositions about normative dissensus (Rossi et al., 1974) and the enforcement process (Hagan, 1974; Chiricos and Waldo, 1976). These studies have helped stimulate a clarification and reformulation of propositions (Turk, 1976). At the same time, however, the presumed strength of a Marxian conflict perspective continues to be a class-based, social historical approach to the study of legal evolution (Platt, 1973:30; Chambliss, 1974:8; Taylor et al., 1973:266). This paper reviews recent developments in the sociology of law and critically examines the theoretical and empirical usefulness of the class conflict

* Authorship is alphabetized and does not reflect seniority or priority; the authors share equal responsibility for this paper.

approach, using the origins of juvenile delinquency legislation in Canada as its data.

Conflict, Consensus, and the Sociology of Law

It is still less than a decade since most sociologists adopted a rather agnostic view of the "conflict-consensus debate." (For discussion of this debate see Chambliss and Seidman, 1973; Hills, 1971; Chambliss, 1973; Hagan, 1977.) Chambliss (1969:8) expressed a common view when he reasoned that "a resolution of this debate . . . would be premature"; that "in many cases there is no conflict . . ."; and that "the influence of interest groups . . . is but one aspect of the processes which determine the emergence and focus of the legal norms" (Chambliss, 1969:10). However, two years later, Chambliss (Chambliss and Seidman, 1971:19) finds the literature far more conclusive: "Indeed, the empirical studies . . . make it quite clear that the value-consensus model is . . . incapable of accounting for the shape and character of the legal system. . . ."

Similarly, Quinney's (1969:1970) early work contained pluralistic themes and a restrained optimism about legal change. Perhaps nostalgically, Quinney (1969:5) noted that criminal prosecutions emerged in Athens, in the sixth century B.C., and that "this step protected . . . the lower class of Athens from aggression by the rich and powerful." Furthermore, Quinney (1970:41) conceded that "groups . . . similar in power may well check each others' interests . . ." and that "interest groups receive their individual claims in return for allowing other groups to press for their interests." At this stage, he was a reformed pluralist, denying the assumption that a diversity of interests typically is resolved through compromise, but acknowledging that a plurality of interests operate, and clinging to the Poundian hope that "the public interest may become an ideal fulfilled . . ." (Quinney, 1970:42; cf. Pound, 1943).

The "New Criminologists" (Taylor et al., 1973:265-6) responded by arguing that "the view of law as . . . in the hands of 'powerful interest groups,' does not take

us far enough. . . ." Quinney (1975b:193) soon agreed that "from the evidence of radical scholarship, government and business are inseparable." Thus, "whilst pluralists may suggest that there are diverse and conflicting interests among groups in the upper class, what is ignored is the fact that members of the ruling class work within a common framework . . ." (Quinney, 1975:194). Taylor et al. (1975:3) endorse Quinney's new position as a "move to a Marxist economism."

The key proposition in this new Marxian perspective on law creation is that "The criminal law is . . . first and foremost a reflection of the interests and ideologies of the governing class . . ." (Chambliss, 1974:37; see also Quinney, 1975b:192). Chambliss (1974:37) comes closest to identifying this "ruling class," but is ultimately unable to decide "whether that class is private industry or state bureaucracy." Instead, he offers the contradictory conclusion that "government bureaucracies may, in the last analysis, be controlled by those who influence the society's economic resources . . ., but they also have a life and a force of their own . . ." (1974:27). Assuming these two possibilities were not mutually exclusive, which they are, undermining the notion of a *single* ruling class, some significant questions would remain unanswered. For example, how much of private industry and state bureaucracy is to be included within the "ruling class"? How diverse and extensive can these groupings be and still be considered a single "ruling class"? To what extent is there conflict within and between private industries and state bureaucracies? And, under what conditions do various industrial or bureaucratic groups prevail?

Quinney's response to such questions is to argue that "in contrast to pluralist theory, radical theory notes that the *basic interests*, in spite of *concrete differences*, place the elite into a distinct ruling class" (1975b:194, emphasis added). A difficulty with this argument is that "basic interests" are not identified with sufficient specificity to allow a predictive test of legal control strategies presumed to follow from these interests. Thus, one "Marxist criminologist" includes "direct release"

as evidence of an "integrative control" used to perpetuate "state capitalism" (Spitzer, 1975:647-9). Unfortunately, this type of conceptualization encourages tautologous theorizing and, in a manner similar to functionalist formulations of the past, engages the fallacy of affirming the consequences. Thus, discussions of "interests served," like those of "functions performed," characteristically are retrospective in form, reasoning teleologically from selected consequences to presumed motivations (cf. Rock, 1974:598; Hirst, 1972).

A different type of evidence sometimes offered in support of class conflict propositions consists of information on the backgrounds and contacts of persons active in lawmaking. For example, Chambliss (1974:21) regards the fact that "legislatures, appellate court judges, and committee members are drawn largely from upper-class members of society . . ." as evidence of what Schattschneider (1960) calls the "mobilization of bias." However, since the membership of a class itself can be in conflict and since membership in one class need not exclude the possibility of siding with another (Mintz et al., 1976:316, 317), the use of this "guilt by membership" argument often amounts to a genetic fallacy and the tendency to argue *ad hominem*.

Finally, Chambliss and Quinney give little empirical attention to the actual level of conflict, consensus or apathy that may accompany the operation of interests, ignoring that the pursuit of these interests may occur with the explicit or tacit support of those affected (Hopkins, 1975:616). The issue is whether "class interests" operate as causes or whether, in many instances, the association with assumed effects may be spurious.

Where the active pursuit of class interests can be tested for its influence in the presence or absence of consensus, a scientific purpose can be served. It is, however, the recent extension of the Marxian perspective beyond its scientific base that concerns us. Quinney (1973:594) argues that "ideas are to be put at the service of the community . . . , and we ourselves must engage in people's struggles." Our concern is that this ideological mission is

based on a perspective that is (1) prone to logical errors, (2) largely unconfirmed, (3) often unconfirmable and (4) possibly quite frequently false.

The Social History of American Delinquency Legislation

The patterns observed in the development of a Marxist theory of law creation are repeated in Anthony Platt's analysis of the origins of American juvenile delinquency legislation. Platt's original work, *The Child Savers*, is grounded in a theoretical tradition (Ranulf, 1938; Gusfield, 1963; cf. Platt, 1969:3,7) that focused on middle-class interests: "Child-saving may be understood as a crusade which served symbolic and ceremonial functions for native, middle-class Americans" (Platt, 1969:98). The culmination of this "symbolic crusade" was the Illinois Juvenile Court Act of 1899, and Platt adopted a partially pluralist stance in explaining that "its success was due in large measure to the fact that it was *widely* sponsored and in turn satisfied *diverse* interest groups" (1969:134, emphasis added). At this point, Platt's most pressing concerns were that "the juvenile court system . . . 'invented' new categories of youthful deviance . . ." (1969:145), that the child-savers "recommended increased imprisonment . . ." (1969:135), and therefore that " 'delinquents' were increasingly committed to institutions . . ." (1969:145).

Four years later, Platt (1973:26) argues that "ideology is healthy and should be made explicit," and concludes that "the problem with *The Child Savers* is that . . . [it] focuses too much attention on the middle-class reformers. . . ." Platt reasons anew that the impetus for delinquency legislation flowed from close and compromising links between members of the middle and upper classes (1974:369), and that "the juvenile court system was part of a general movement directed towards developing a specialized labor market and industrial discipline under corporate capitalism by creating new programs of adjudication and control . . ." (1974:377).

Evidence for this new, class-interested Marxian theory is the same as that presented in Platt's (1969:ch. 3) original

account and is similar in type to that proposed by Chambliss and Quinney. Descriptions of a "new penology" are offered, but no evidence is provided that passage of juvenile court legislation resulted in an increase in the number of juveniles incarcerated, that the industrial elite benefited significantly as a result of this incarceration, or that the "ruling class" played any direct role in the passage of this legislation. Instead, the primary evidence presented engages a genetic fallacy of the type we previously associated with the "mobilization of bias" argument. Thus, the main body of information consists of a cataloguing of the class backgrounds and backings of persons involved in the child-saving movement. Platt (1969:367-8) concludes that this movement "would not have been capable of achieving significant reforms without the financial and political support of the wealthy and powerful" and that "Even the more radically-minded child-savers came from upper-class backgrounds." What is lacking in this account is any concrete evidence that this sponsorship was a *causal* factor that operated independently of widespread support for the ensuing legislation.

Summarizing, Platt's primary concerns are that links between middle-class reformers and upper-class sponsors resulted in the wealthy and powerful using the passage of American delinquency legislation to "invent" new forms of youthful misbehavior and increase imprisonment, all in the larger interest of "developing a specialized labor market and industrial discipline under corporate capitalism." Alternatively, we have argued that this Marxian perspective on the origins of American delinquency legislation is either unconfirmed or unconfirmable, plagued by logical errors and, therefore, quite possibly false. In the following section, these conclusions are tested anew with historical information on the origins of Canadian delinquency legislation.

The Social History of Canadian Delinquency Legislation

This part of our analysis is based on historical data drawn from a variety of

sources: historical accounts written by participants in, and observers of the child-saving movement; personal correspondence and accounts drawn from the archives of leading advocates of delinquency legislation; proceedings of conferences concerned with child welfare; reports of government commissions; legislative debates; and statistics drawn from the Toronto Juvenile Court. Particular attention is given to an extensive correspondence between the two leading advocates of delinquency legislation in Canada: J.J. Kelso and W.L. Scott.

Our analysis focuses on entrepreneurial interests and activities leading to the emergence of the juvenile court, the organizational development of this court, and the objective consequences of this process (Becker, 1963; Dickson, 1968). Of particular concern is the manner in which the entrepreneurial interests of individuals are aggregated and polarized into organizational issues (Lemert, 1970). Weber reminds us that these organizational issues can be directed to various ends: "bureaucracy as such is a precision instrument which can put itself at the disposal of quite varied—purely political as well as purely economic, or any other sort—of interests . . ." (Gerth and Mills, 1946:231). Thus, Platt's Marxian position is only one among the possibilities entertained by Weber. With this in mind, we will consider first the link proposed by Platt between class interests in social control and the coercive consequence of increased juvenile imprisonment.

Data available for the City of Toronto¹ do not support Platt's position. In the year preceding juvenile court operations, 123 juveniles were sent to industrial schools; 71 were sent to such schools in the first

¹ In the following, emphasis is placed on the development of a system of juvenile justice in Ottawa and Toronto, and the Province of Ontario, as well as on the federal level. Much of the federal legislation was anticipated and first implemented in Ontario, where many of the most influential child-savers lived. As a result of limitations in resources, the provisions of this Act allowed that it not take immediate effect in some cities and provinces, therefore, dates of implementation varied. Toronto was among the first Canadian cities to establish a Juvenile Court in 1912 (Ontario Law Reform Commission, 1974).

year of juvenile court proceedings (City of Toronto, 1912:14). More detailed information on other forms of institutionalization (including a working-boys home, two hospitals and a training school) is available from the court reports of succeeding years (see Appendix 1). Here again, no consistent pattern of increase is apparent, and at no time during the forty-year period does the *total* institutionalized population exceed the number sent to industrial school alone in the year before court operations began.² In the following historical discussion, we will see that the inaccuracy of Platt's position derives from a misrepresentation of legal developments that preceded delinquency legislation in Canada. Furthermore, we will find little evidence, either in personal correspondence or in public documents, that members of the industrial elite expressed an active economic interest in the passage of delinquency legislation. The activities of other individuals and interest groups, particularly those involved with the probation movement (cf. Schultz, 1973), will be considered, but the involvement of the "ruling class" seems to have been peripheral at most. We discuss three periods that preceded the passage of delinquency legislation: an initial period in which lengthy stays in reformatories replaced sentences spent in common gaols; a second period during which treatment-focused industrial schools began to replace reformatories; and a third period when organized probation emerged as a new treatment strategy influential in the development of delinquency legislation.

The child-savers focused first on juveniles convicted as criminals. Two acts were passed in 1857: *An Act for Establishing Prisons for Young Offenders* and *An Act for the More Speedy Trial and Punishment of Young Offenders*. The first directed the construction of "reformatory prisons," while the second provided for summary trial procedures and increased powers to discharge juveniles in order "to avoid the evils of their long imprisonment

previously to trial." Thus, as early as 1857 in Canada, there were provisions for special institutions and trial procedures for juveniles.

In 1874, Ontario passed *An Act Respecting Industrial Schools*, intending to provide residential institutions that would be less severe than reformatories and to which "neglected, uncontrolled, and delinquent" children could be sent. Subsequent to this legislation, J.J. Kelso, a crusading news reporter who later became Ontario's Superintendent of Neglected and Dependent Children, emerged as a key "moral entrepreneur" (cf. Becker, 1963) in the Canadian child-saving movement. In 1887, Kelso brought together the Toronto Humane Society for "better laws, better methods, [and] the development of the humane spirit in all affairs of life" (Kelso, 1911:17; Hodgins, 1888).

Generally consistent with such principles, Ontario passed *An Act for the Protection and Reformation of Neglected Children* in the following year. Provisions of this act allowed that "the Lieutenant-Governor may . . . appoint . . . commissioners . . . to hear and determine complaints against juvenile offenders . . ." and that "their cases be disposed of . . . separately from other offenders. . . ." However, this act also authorized the courts to commit neglected children to industrial schools. Kelso consistently tempered this approach by arguing that "the aim . . . is not to steal children from their parents . . . , but by every available means to make the home and family all it ought to be" (Province of Ontario, 1895). It took Kelso several years to translate these views into law.

In 1890, two additional statutes were enacted in Ontario (*An Act Respecting the Custody of Juvenile Offenders* and *An Act Respecting the Commitment of Persons of Tender Years*), each of which further restricted the use of reformatories and expanded the use of industrial schools for selected children. In 1891, a Commission of Inquiry into the Prison and Reformatory System of Ontario (Province of Ontario, 1891) completed a report that seemed to forecast the shape of things to come. In addition to dealing with institutional reforms, the Commission recom-

² This finding is consistent with less comprehensive figures reported by Scott (1906-1908) for Ottawa and in his correspondence with provincial officials in many other parts of Canada.

mended that magistrates grant discharges to first offenders convicted of trivial offenses and that various powers be given to probation officers.

Later the same year, the Commission's chairman and one of its most prominent members joined Kelso in Toronto to organize a public meeting at which the Children's Aid Society and Fresh Air Fund was founded, with Kelso as President. A letter announcing the goals of this meeting signaled a growing interest in an important organizational innovation: "The appointment of a probation officer to ascertain and submit to the court full particulars of each child brought up for trial, and to act in the capacity of the child's next friend" (see Kelso, 1911:69).

As reform efforts gathered momentum, a theme of professionalism emerged: "What is needed," wrote Kelso (n.d. (a):20), "is personal service, the complete organization of charitable forces, harmony of action, and the appointment of trained and experienced workers. . . ." Gradually, persons involved in the movement seemed to develop an interest in their own positions in an emerging bureaucracy. The influence of this emerging interest group on the legislative process is suggested in Kelso's comment to the Sixth Canadian Conference of Charities and Corrections that "we have as much if not more law than we can assimilate, and the governments are ready to give new measures whenever they are asked to do so . . ." (Proceedings, 1903-1909:21).

The legislation of this period was particularly useful in setting the organizational base for probation work. The first *Criminal Code of Canada* was passed in 1892, providing for separate trials of persons under sixteen where it was "expedient and practicable" to do so; and, in 1893, at the urging of Kelso and others, the Ontario legislature enacted a comprehensive *Children's Protection Act* giving explicit recognition and authority to Children's Aid Societies. The latter act specifically stipulated that it was the duty of the court to notify the Executive Officer of the Children's Aid Society (if one existed in the county) prior to initiating proceedings against a boy under twelve or a girl under thirteen; this officer was then

to investigate the charges, inquire into the child's family environment, and report back to the court with his findings. These procedures were reaffirmed in the following year with a federal *Act Respecting Arrest, Trial and Imprisonment of Youthful Offenders*.

In a short Commons debate of this act, the Minister of Justice and Attorney General noted that "a great many magistrates from motives of humanity" were already conducting separate trials for juveniles (Canada, 1894, June 24:4940-1). A magistrate in the Toronto Police Court later confirmed this, noting that "in 1892 we instituted the Children's Court. . . . We set apart a small room in the lower part of City Hall . . . , and I was accustomed to go down to that room to try all charges against children . . ." (Denison, 1920:254). These developments, and Kelso's role in them, became the basis for claims that the juvenile court had a Toronto origin and was therefore a "Canadian enterprise" that had been appropriated by "American social workers."³ Thus, Kelso later expressed his concern to Scott that "our Ontario work should not be overlooked as I advocated the Children's Court here twenty years ago, gave addresses in Chicago and elsewhere in favor of it and got the law (the Children's Protection Act) passed here in 1893" (Scott, 1906-1908: 12/27/1906; 7/4, 8/1907). One address to which Kelso refers was given in Chicago on October 11, 1893 (Proceedings, 1893; see, also, 1895). Kelso notes that "Judge Hurd consulted with me as to the drafting of the Juvenile Court following my address,"⁴ and that a much discussed extract from this speech appeared in the *Chicago Tribune* the following day (cf. Flexner and Baldwin, 1914:3-4). Kelso's view was that ". . . of course, the Denver

³ For example, these claims are made in a 1933 Toronto newspaper article titled "Juvenile Court had Toronto Origin" (see Hagan, 1977:21). These claims (as contrasted, for example, with those made for New York and Massachusetts) are less important for their factual accuracy than for their indication of close connections between Canadian and American child-saving efforts.

⁴ This comment is found in Kelso's (n.d.(b):19) handwriting at the bottom of a page of an article by Hurly on "The History of the Illinois Juvenile Court Law."

and Chicago courts have far outstripped us but at the same time we gave them the inspiration that led to their present success (Scott, 1906-1908: 12/27/1906).

In 1903, a subsection was added to the Ontario *Children's Protection Act*, specifying that without being convicted of a provincial offense, persons under sixteen could be placed by a judge under the care of a probation officer, who would report periodically "concerning the progress and welfare of the child." The idea of a probation system "to help the children before they become criminally disposed" was increasingly discussed (Kelso, 1907:107). Kelso gave this top priority: "We want to bring about what is called the Probation System, following the children up from their first offense . . ." (Proceedings, 1903:21). The assumption behind this proposal was that imprisonment frequently could be avoided: "Whenever there is an offense there is a cause behind it and our children's court and probation system should be able to reach that cause and . . . remove it for the safety and protection of the children in the home" (Department of Neglected and Dependent Children, 1907:15).

To this point, we have focused on J. J. Kelso as the central "moral entrepreneur" in the advocacy of probation and delinquency legislation. However, he was joined in this pursuit by a "professional" (cf. Becker, 1963:152) counterpart, W.L. Scott, Local Master for the Supreme Court of Ontario and President of the Ottawa Children's Aid Society. Kelso and Scott vocalized three primary concerns. First, additional funds were needed to elevate "philanthropic work to the status of a profession and to encourage University graduates to become specialists in social and moral reform work" (Proceedings, 1907:8). Second, it was claimed that probation officers were now hampered in their work by a lack of "legislative recognition" (Scott, 1906-1908:1/2/1907). And, third, there was a perceived need for special judges: "The Children's Court should undoubtedly be . . . conducted by specially selected persons . . ." (Kelso, 1908:164).

Drafted by Scott and others in response to these concerns, the federal *Juvenile De-*

linquents Act was introduced first in the Speech from the Throne in 1906, reintroduced to the Senate in 1907 and eventually passed in 1908. A "juvenile delinquent" was defined broadly by this act as any child under sixteen who violated any federal or provincial statute, or municipal by-law, or who was liable by any other act to committal to an industrial school or juvenile reformatory. Thus, this act consolidated various previously illegal behaviors into a new category called "delinquency," but it did not add any behaviors to those already specified under existing statutes and by-laws. It would be misleading to conclude, as Platt does for the United States, that Canadian delinquency legislation "'invented' new categories of youthful misbehavior."⁵

The act did, however, give juvenile courts exclusive primary jurisdiction in cases of delinquency. It also provided for the formation of a voluntary Juvenile Court Committee to consult with and advise probation officers, or to appoint an officer where remuneration was available and an officer was not already appointed under provincial authority. Furthermore, probation officers were assigned powers of a constable, with their duties including: conducting investigations, being present and representing the interests of the child

⁵ Sixteen years later, *An Act to Amend the Juvenile Delinquents Act*, 1924, proposed the addition of an omnibus clause—"or who is guilty of sexual immorality or any other form of vice"—to the definition of a juvenile delinquent. However, by this time, such omnibus clauses had been a part of legal definitions of neglect for more than thirty years. It is sometimes argued that such clauses define as delinquent behaviors that are most common among underclass youth. However, this alone cannot count as support for a Marxian class conflict perspective, since the underclass may agree with such legal definitions. The latter possibility is supported by the finding of Black and Reiss (1970) that black complainants are more likely than whites to insist on the arrest of blacks accused of delinquency. Perhaps most significantly, however, the same Marxian theorists (e.g., Liazos, 1974) who claim underclass behaviors are defined differentially by law as delinquent, also cite self-report studies reporting an absence of relationship between social class and delinquent behavior. Probably the safest conclusion is that vague statutory clauses like those defining delinquency depend most heavily for their consequences on the discretion of those who must interpret and enforce them: citizens, the police and prosecutors.

in court, furnishing the court with such assistance and information as required, and taking charge of any child before or after trial, as directed by the court.

Discussions of these and other provisions of the act mainly involved two groups. The ultimately successful group included those who advocated treatment and prevention through probation and a special court; they distributed copies of the bill, circulated petitions, and invited such speakers as Judge Lindsay, of the Denver Juvenile Court, and Mrs. Schoff, of the Philadelphia Mother's Union, to address various gatherings. The opposition included police officers and magistrates who already assumed organizational responsibilities for children and who, therefore, had a very immediate interest to protect (see Scott 1906-1908:3/7, 3/15/1907). This group advocated a more "punitive" approach to delinquency. Particularly vehement in this view were several police officials, including Inspector Archibald and Police Magistrates Denison and Kingsford, associated with the Toronto Children's Court. The debate was often bitter, with Archibald, in a report circulated to gain support for the police position, charging that the new proposals:

work upon the sympathies of philanthropic men and women for the purpose of introducing a jelly-fish and abortive system of law enforcement, whereby the judge or magistrate is expected to come down to the level of the incorrigible street Arab and assume an attitude absolutely repulsive to British subjects. The idea seems to be that by profuse use of slang phraseology he should place himself in a position to kiss and coddle a class of perverts and delinquents who require the most rigid disciplinary and corrective methods to ensure the possibility of their reformation. (Archibald, 1907:5)

In response, Scott (1906-1908:3/19/1907) labeled Archibald a "person of very limited intelligence," while Kelso (Scott 1906-1908:5/14/1907) called him "self-opinionated" and opposed to those who failed to treat him with "deference." Scott (5/2/1907) concluded that the members of the Toronto Police Department felt the new proposals were intended to supplant them and were therefore a reflec-

tion on their past work. Moreover, Archibald's particularly negative attitude was said to be based on the fact that he "had prepared all the legislation on the subject during the last forty years and . . . is apparently deeply offended that anyone else should have usurped this prerogative" (Scott, 1906-1908 4/16/1907).

Scott (1908:894) went on to campaign successfully for the *Juvenile Delinquents Act*, emphasizing in his arguments "that probation is the only effective method for dealing with young offenders." Thus, when the bill was reintroduced to the Senate in May, 1908, Senator Coffey referred to "the difference of view as to the means and methods whereby the best results may be achieved," and explicitly dismissed the position of Inspector Archibald as being characterized by an outmoded "spirit of rigidity and severity" (Canada, 1907-8:975-7). Although some objections were raised in the House of Commons by those concerned with the legal rights of children, the temper of the ensuing debate probably is summarized best by one member of Parliament who observed that the act "is to be laughed through as a joke" (Canada, 1907-8:1240-1). The legislation was passed, but this did not end the organizational search for support. The following years brought a succession of minor revisions and extensions of the original legislation, concerted efforts to secure appropriate provincial legislation creating the newly authorized juvenile courts, and continued efforts to obtain sufficient funds from various levels of government to employ probation officers (Proceedings, 1909; Scott, 1952).

The consequences of the entrepreneurial activities we have described are illustrated in the statistical records of the Toronto Juvenile and Family Court (see Appendix 1). Probation officers in this court predominated in both their number and scale of activities. Thus, while the number of *official* occurrences recorded by the Juvenile Court actually was lower in 1952 than in 1912, during this period the number of probation officers increased from 4 to 14, and the number of occurrences handled *unofficially* by probation officers escalated more than 900 percent, from 735 to 6,574 cases. The Probation

Department apparently did this in a manner the Toronto Police Department seemed initially to fear, that is, by attracting their own new cases and refusing to refer them either to the police or court. Thus, the Probation Department publicly proclaimed that "it is our aim, as far as possible, to settle through the Probation Department all difficulties without making them court cases" (City of Toronto, 1920:5), while also annually advising that "we again, as many times before, earnestly solicit those having problems of various kinds, within the jurisdiction of the Court, coming and allowing us to assist before these problems become too acute so as to require official action" (City of Toronto, 1933:9). Specifically, the Probation Department offered social and clinical investigations in all such cases, leading to dramatic increases in the number of interviews counted annually into the Court Reports. This aspect of the court's work was augmented in 1929 with increased jurisdiction over family matters, followed by increasing rates of court referrals and support actions in succeeding years. In this way, *the social work of the Probation Department steadily increased, without simultaneous increases in the number of adolescents institutionalized or placed on probation*. Thus, in those years when official delinquency actually declined, the Probation Department was quick to observe optimistically "that while we had a decrease in delinquency . . . we had an increase of the numbers who came of their own free will to get help. If it is safe to draw an observation from this fact, it would be reasonable to assume that the non-official effort of the court is reducing its official acts" (City of Toronto, 1933:9). This conclusion was, of course, self-serving; the long term effect on children of this increase in non-official activity remains unclear.

Discussion and Conclusions

The conflict that surrounded the *Juvenile Delinquents Act* in Canada was less about normative definition than about the organizational arrangements under which violators of the norm would be processed. More specifically, it was sup-

porters of the police and advocates of probation who quarreled most about the organizational procedures to be followed. J.J. Kelso, a moral entrepreneur who rose from news reporter to a high position in the child-care bureaucracy, collaborated with W.L. Scott, a professional counterpart and philanthropist who occupied an administrative position in the Ontario Supreme Court, to engineer a legislative movement whose organizational goal became the prevention of delinquency through juvenile probation work. Opposition came from representatives of existing police organizational interests whose more punitive philosophy was reflected in writing prior legislation and their current activities in handling juveniles. Advocates of probation prevailed in the passage of delinquency legislation in 1908, and the consequences of these efforts became apparent in the organizational composition and activities of the new Juvenile Court in Toronto: the handling of *unofficial* occurrences by probation officers dramatically increased, while the level of *official activity* showed some signs of decline.

Whether the eventual success of advocates of probation served the basic interests of the ruling elite is unknown, and probably unknowable, for the various reasons discussed above; however, little influence of the industrial elite was revealed in the personal correspondence and public documents of the key proponents of this legislation. Moreover, in contrast to Platt's account of the passage of delinquency legislation in the United States, no evidence was found in the Canadian data that institutionalization increased as a result of such legislation, or that this legislation was useful in developing a "specialized labor market" or "industrial discipline."

Part of Platt's (1974:389) argument proceeds from a basic Marxian assumption that "as the contradictions (of capitalism) become more apparent and the control system more unsuccessful, the methods of coercion become similarly more explicit and more desperate." It is apparently for this reason that Platt places such great emphasis on the role of the "New Penology" in the emergence of delinquency legislation. In contrast, the argument of

Appendix 1. City of Toronto Juvenile and Family Court Statistics, 1912,^a 1920, and at Two-Year Intervals to 1952

Year	1912	1920	1922	1924	1926	1928	1930	1932	1934	1936	1938	1940	1942	1944	1946	1948	1950	1952
Court Staff																		
Judges ^b	1	1	1	1	1	1	2	2	2	2	2	2	2	5	4	4	4	5
Probation Officers ^c	5	7	7	6	6	6	11	12	12	12	10	10	13	14	14	15	15	15
Clerical Staff	1	5	5	6	5	5	7	7	7	7	8	8	8	8	8	9	10	9
Psychiatrists & Psychologists	0	0	0	1	1	1	1	1	2	1	1	1	1	1	1	1	2	2
Other	0	0	0	0	1	1	2	2	2	2	2	2	2	2	2	2	2	3
Total	7	13	13	14	14	14	23	24	25	24	23	23	26	30	29	31	33	34
Probation Department Statistics																		
Interviews	-	-	2976	2814	3643	4651	10624	9984	9789	8547	9547	9895	11711	-	-	-	-	-
Occurrences	725	955	1753	1243	1636	1701	4127	4021	3962	4780	3415	3488	3800	4732	7336	6574	-	-
Percent Referred to Court	20.7	7.9	6.0	4.5	2.9	1.8	2.7	1.1	3.2	5.3	6.8	10.2	13.1	13.0	10.3	-	-	-
Court Dispositions																		
Probation	769	733	731	877	350	207	304	99	425	629	488	486	544	415	316	387	418	345
Fines	202	77	92	245	228	94	28	28	13	15	29	31	80	111	30	14	17	18
Suspended Sentence & Adjudgments ^a	387	679	764	971	1596	2105	1584	846	780	520	416	479	805	413	216	227	280	273
Institutionalized	85	78	43	61	34	49	71	30	44	46	53	50	85	120	67	71	72	97
Dismissed & Withdrawn	105	83	132	66	21	38	56	34	15	28	48	59	107	99	56	39	70	79
Other ^c	191	59	43	77	52	45	74	7	10	0	60	73	72	56	55	12	33	15
Total	1744	1709	1805	2298	2281	2538	2122	1483	1287	1238	1094	1178	1693	1214	740	750	890	827
Support Actions																		
	-	-	-	-	-	-	756	586	510	435	411	689	880	1089	1349	1202	1167	1310

^a Juvenile court work began in 1912 in Toronto; reports were not issued from 1913-1919; reports ceased in 1952. Dashes indicate years for which specific types of data were not recorded.

^b Includes commissioners, judges and magistrates.

^c Includes probation officers, social investigators and social workers.

^d Includes commitments to industrial schools, Working Boys Home, Orillia Hospital, Ontario Hospital and training schools.

^e Includes cases pending, remanded, wards of Children's Aid Society, home placements and transfers to higher courts.

this paper is that it was an emphasis on probation work, and not imprisonment, that led to this new legislation. Thus, although this legislation substantially changed the operations of the juvenile and criminal courts, probably with consequences both good and bad, intended and unintended (cf. Hagan, 1975), the overall effect was not to intensify a formal and explicit system of coercion, but rather to reinforce and increasingly intervene in informal systems of social control, particularly the family. These findings raise the possibility that assumed contradictions in the economic sphere may have only marginal significance for the juvenile justice system, or that these assumed contradictions need not necessarily lead to more explicit methods of social coercion. In any case, this paper provides a considerably different account of the origin of delinquency legislation than is offered by Platt, and it is an account which encourages a reconsideration of the conditions under which this version of a Marxian, class conflict perspective can be usefully applied.

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SUBJECTIVE CLASS IDENTIFICATION: A MULTIPLE INDICATOR APPROACH*

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Research on subjective class identification has tended to use the same single-item measure of this construct without regard to its validity and reliability. To address these and related measurement issues, multiple indicators of subjective class are analyzed within the framework of a path analytic model with unobservable variables. Measures of objective "class" and political alienation are also included in the construction of an overidentified model, and Jöreskog's procedure for the analysis of covariance structures is used to find an acceptable fit to the data. Final models for black and white subsamples support the assumption of unidimensionality which underlies previous measures of subjective class identification. Validity and reliability estimates are given for various indicators, including the conventional measure. Also, advantages of the multiple indicators approach are demonstrated by comparing regressions of the standard, single-item measure and a composite measure of subjective class on education, occupational status, and income.

Recent research (cf. Hodge and Treiman, 1968; Jackman and Jackman, 1973) on the relationship between objective and subjective placements in the system of social inequality has focused almost entirely upon theoretical issues. The empirical finding that this relationship is far from perfect has tended to be treated as a social fact, and the methodological problems inherent in tests of the relationship have been largely ignored. This is surprising, especially since these same studies have employed research measures which may only crudely approximate the relevant theoretical constructs. In particular, while objective placement has been

consistently treated as multidimensional, subjective placement has been judged *a priori* as unidimensional. Further, there has been no assessment of the validity and reliability of the standard survey question consistently taken as the single indicator of subjective placement. In order to address these measurement issues, we will examine multiple indicators of subjective social class identification within the framework of a structural equation model with latent variables.

Subjective Class Identification

Subjective class identification has been given a variety of meanings. For some it appears to be a substitute for class consciousness (cf. Hodge and Treiman, 1968; Guest, 1974); for others it is treated as

* Authors are listed alphabetically. We wish to thank Eliot Smith and an anonymous reviewer for several helpful comments.



only a component or partial indicator of class consciousness (cf. Morris and Murphy, 1966; Lopreato and Hazelrigg, 1972; Jackman and Jackman, 1973). Little distinction has been made between "class" and "status" consciousness, and class identification often has been used to cover both of these constructs. For example, Jackman and Jackman (1973:569) refer to subjective class identification as "the individual's perception of his own position in the status hierarchy."

Despite differences in conceptualization, the persistent use of the same single-item measure of subjective class identification implies a common assumption of unidimensionality. This consideration alone ought to be sufficient for raising questions about the validity and reliability of the standard measure. But the assumption of unidimensionality not only belies the multiple usages of the term "subjective class," it also seems counter to classical discussions of single versus multiple bases of *objective* class.

Following Weber's (1946) orientation toward the analysis of social inequality, the measurement of objective placement generally has been approached multidimensionally through various socioeconomic indicators. The argument has been made that this approach should be extended to subjective placement. As Morris and Murphy (1966:310) remark, "If Weber's analytical distinctions among class, status, and power have any heuristic significance at all, and the majority of sociologists seem to think they do, then it behooves us to carry out these distinctions in matters of the subjective meaning of stratification."

Taking this consideration into account focuses attention on certain measurement issues. If we consider the possibility that there are different types of subjective identification corresponding to Weber's distinctions between class, status, and power, then we are faced with the question of which of these types the current single-item indicator is measuring. The importance of this question is underscored by the realization that an individual's perceived positions in the three orders may be incongruent. Writing principally in

terms of class consciousness, but with obvious implications for subjective class identification, Lopreato and Hazelrigg (1972:123-5) note:

Weber's taxonomy introduces the distinction of *types*, as opposed to degrees, of social consciousness. There is a type of consciousness corresponding to each of the three orders. The difficulty of treating these phenomena in a single, consistent scheme of analysis stems from the circumstance that, as a society grows in complexity, the likelihood that a given individual will hold an equivalent position in all three orders steadily diminishes. Concomitantly, the likelihood that he will experience his power, prestige, and class position as congruent with one another also diminishes. When we speak of "class" consciousness, therefore, the first question we must raise is, is it *class* consciousness, *status* consciousness, or *power* consciousness? And, further, how are the three interrelated?

Possible incongruence among self-placements ("consciousness") in the different orders raises doubts concerning some of the current interpretations of research findings regarding the relationship between objective and subjective "class." Hodge and Treiman (1968) and Jackman and Jackman (1973), using the same data and the standard single-item class identification measure, found that class identification was only moderately determined by various measures of objective class position. If, however, multiple indicators of self-placement corresponding to Weber's typology were used, then stronger relationships might emerge between analogous dimensions of objective and subjective class. For example, income may be a stronger predictor of subjective class based upon economic criteria than subjective class based upon life-style or influence.

In the remainder of this paper, we examine data pertinent to these issues. A central feature of this data is a group of subjective class identification measures which include, in addition to the standard item, four items intended to correspond to the Weberian distinctions. By construct-

ing a multiple indicator model incorporating these measures, we attempt to determine the dimensionality of subjective class. In addition, an important feature of our approach is that it enables us to assess the degree to which various indicators of subjective class identification are subject to certain types of bias and random errors of measurement.

The Causal Modeling Approach to Measurement Error

Our analysis of the multiple indicators of subjective class identification follows the causal modeling approach to measurement error (cf. Blalock, 1969; Costner, 1969) and involves the application of Jöreskog's (1970; 1971) procedure for the analysis of covariance structures. This approach calls for the construction of hypothesized structural equation models with latent traits to account for the intercorrelations among a set of multiple congeneric measures (or indicators) (Werts and Linn, 1970). A variety of strategies may be applied in arriving at an hypothesized model. The common approach, which we shall use, begins with a model which assumes that measurement error is strictly random. When such a model provides an acceptable fit to the data, coefficients linking latent traits and indicators may be interpreted as validity coefficients and, when squared, as reliabilities (Werts and Linn, 1970; Werts et al., 1974). A comparison of these coefficients then may be used to assess the relative adequacy of the indicators. When the hypothesized model is overidentified, then failure to fit the data may indicate the presence of nonrandom error.

In order to detect certain kinds of nonrandom measurement error involving the subjective class indicators, we construct an overidentified model which includes measures of objective status and political alienation as well as subjective class. These three constructs are all theoretically related. By including an additional latent trait, political alienation, with multiple indicators, we can detect correlated errors and two types of "differential bias" (Costner, 1969).

Data and Method

Data for this research were taken from the 1969 Gary Area Project of the Institute for Social Research at Indiana University. The sample consisted of 800 adults drawn from the city of Gary, Indiana, using a modified probability sample with clusters and quotas (see Sudman, 1966). Trained graduate students and professional interviewers conducted the survey during January, 1969.

The objective status measures were: (a) education—indicated by five categories ranging from less than ninth grade to college graduate; (b) occupation—indicated by two-digit Duncan SEI scores; (c) income—indicated by total family income before taxes in 1967, coded in units of \$2,000 and ranging from under \$2,000 to \$16,000 or more.

All subjective class indicators were structured questions asking respondents to place themselves in one of four class categories: lower, working, middle, or upper. One indicator was a standard, general class identification item; the other four asked respondents to indicate which social class they felt their occupation, income, way of life, and influence were most like. The criteria, in terms of which class self-placements were made, correspond directly to the Weberian dimensions of economic class (occupation and income), status (life-style), and power (influence).

The political alienation indicators consisted of four statements about the national government to which respondents replied on four-point Likert scales. Two of these statements were adapted from Olsen's (1969) Political Incapability/Futility Scale and two were adapted from Neal and Seeman's (1964) Powerlessness Scale.¹

Intercorrelations among the indicators plus their means and standard deviations

¹ These four statements are, respectively: (1) "People like me don't have any say about what the government does." (2) "I don't think public officials care much what people like me think." (3) "The leaders of the federal government don't care what happens to people like me." (4) "This nation is run by the few people in power, and there is not much the little guy can do about it."

are displayed by race in Table 1. Blacks and whites were analyzed separately to control for the possible effects of race.²

Analysis

We assume that the structure of intercorrelations among the twelve measured variables (indicators) can be accounted for by the model in Figure 1. This baseline model (Model I) implicitly assumes that the structure of intercorrelations is completely accounted for by the impact of five latent traits (E, O, I, S, P) on their respective indicators (x_1, x_2, \dots, x_{12})³ and by the correlations among the latent traits. In short, the model assumes that the only errors of measurement are random and that subjective class is unidimensional. Thus, substantively, Model I may be seen as the null hypothesis in testing the alternative that the indicators of subjective class are multidimensional. However, failure to provide an acceptable fit to the data would not necessarily signify multidimensionality but might indicate the presence of nonrandom error.

Before parameter estimates can be meaningfully interpreted, the hypothesized model must provide an acceptable fit to the data. Judgments about the adequacy of the fit are determined by: (a) calculating a χ^2 measure of goodness of fit and (b) observing the residuals matrix, obtained by subtracting the observed correlations (Table 1) from the correlations reproduced by the parameter estimates. The significance of the χ^2 tests the null hypothesis that the observed and reproduced correlations differ within the limits of

chance variation.⁴ Also, the closeness of the χ^2 value to the degrees of freedom is a useful intuitive guide to the adequacy of the fit (Jöreskog, 1971). If the fit is poor, the residuals matrix may be used to draw inferences about how the model should be respecified. Sources of nonrandom error, or invalidity, may then be identified by alternative models which adequately reproduce the matrix of correlations.

By including multiple indicators of more than one latent trait, or unobserved variable, in the model, we can detect nonrandom errors of the following three types (Costner, 1969; Kalleberg, 1974): (1) a direct effect of one trait on indicators of a different trait; (2) an extraneous factor affecting indicators of two or more different traits; (3) a correlation between errors of measurement in indicators of different traits. Where, for example, two unobserved variables are measured by indicators subject to "social desirability" or some other method response set (nonrandom error [2]), a model assuming only random error may consistently underestimate the correlation of indicators across factors. The inclusion of political alienation indicators in the present analysis was based upon the possibility that the similar working of subjective class indicators might imply a response set. Since the alienation items also involve methodological similarities, any common or correlated, method-based response set underlying the two sets of indicators ought to be reflected in the pattern of residuals for Model I.

Maximum likelihood estimates of the parameters of Model I⁵ were obtained through the use of Jöreskog's (1970) general model for the analysis of covariance structures and a corresponding computer program (Jöreskog et al., 1970). Table 2 presents the residual matrices and χ^2 tests

² The decision to examine blacks and whites separately is based upon Jackman and Jackman's (1973) finding of significant race-education and race-occupation interaction effects upon class identification. Among blacks, they found only income to be significantly related to class identification; whereas among whites, class identification was sensitive to education and occupational status, as well as income.

³ Following common practice (cf. Hodge and Treiman, 1968; Jackman and Jackman, 1973), education, occupation, and income have been treated as separate dimensions of objective status, and latent traits corresponding to each dimension have been included in the model.

⁴ Under the assumption of multivariate normality, the test statistic for goodness of fit is distributed as χ^2 with degrees of freedom equal to the number of over-identifying restrictions.

⁵ In estimating model parameters, the values of the paths from each of the objective status latent traits to their respective indicators have been fixed at 1.0. Such a specification is required for purposes of identification and is implicit in analyses with single indicators of constructs.

Table 1. Zero-Order Correlations, Means, and Standard Deviations for Indicators of Objective Class, Subjective Class, and Political Alienation, Whites (below Diagonal) and Blacks (above Diagonal)

Variables	X ₁	X ₂	X ₃	X ₄	X ₅	X ₆	X ₇	X ₈	X ₉	X ₁₀	X ₁₁	X ₁₂	Mean (s.d.)
X ₁ education		.404	.268	.216	.233	.211	.207	.202	-.143	-.283	-.233	-.270	1.274 (1.106)
X ₂ occupation	.495		.220	.277	.183	.270	.157	.142	-.185	-.105	-.137	-.137	23.467 (16.224)
X ₃ income	.398	.292		.268	.424	.325	.282	.238	-.133	-.159	-.113	-.169	4.041 (2.097)
X ₄ s.c.-occupation	.218	.282	.184		.550	.574	.482	.482	-.213	-.123	-.133	-.166	1.288 (0.747)
X ₅ s.c.-income	.299	.166	.383	.386		.647	.517	.472	-.095	-.188	-.160	-.211	1.129 (0.814)
X ₆ s.c.-life style	.272	.161	.321	.396	.553		.647	.622	-.139	-.196	-.154	-.231	1.235 (0.786)
X ₇ s.c.-influence	.269	.169	.191	.382	.456	.534		.632	-.182	-.259	-.186	-.260	1.318 (0.859)
X ₈ s.c.-general	.330	.231	.277	.431	.537	.608	.590		-.142	-.117	-.173	-.191	1.300 (0.784)
X ₉ pol. alienation 1	-.133	-.116	-.182	-.095	-.195	-.148	-.162	-.158		.351	.316	.327	1.264 (0.642)
X ₁₀ pol. alienation 2	-.235	-.175	-.203	-.159	-.231	-.155	-.235	-.192	.401		.372	.423	1.416 (0.729)
X ₁₁ pol. alienation 3	-.248	-.225	-.140	-.152	-.137	-.137	-.194	-.172	.385	.508		.523	1.715 (0.778)
X ₁₂ pol. alienation 4	-.277	-.171	-.239	-.138	-.290	-.154	-.189	-.170	.439	.504	.481		1.490 (0.726)
Mean (s.d.)	1.655 (1.203)	36.698 (21.277)	5.040 (2.198)	1.543 (0.640)	1.548 (0.670)	1.542 (0.623)	1.601 (0.647)	1.554 (0.627)	1.164 (0.668)	1.301 (0.670)	1.507 (0.736)	1.291 (0.624)	

* N (Whites) = 432; N (Blacks) = 368.

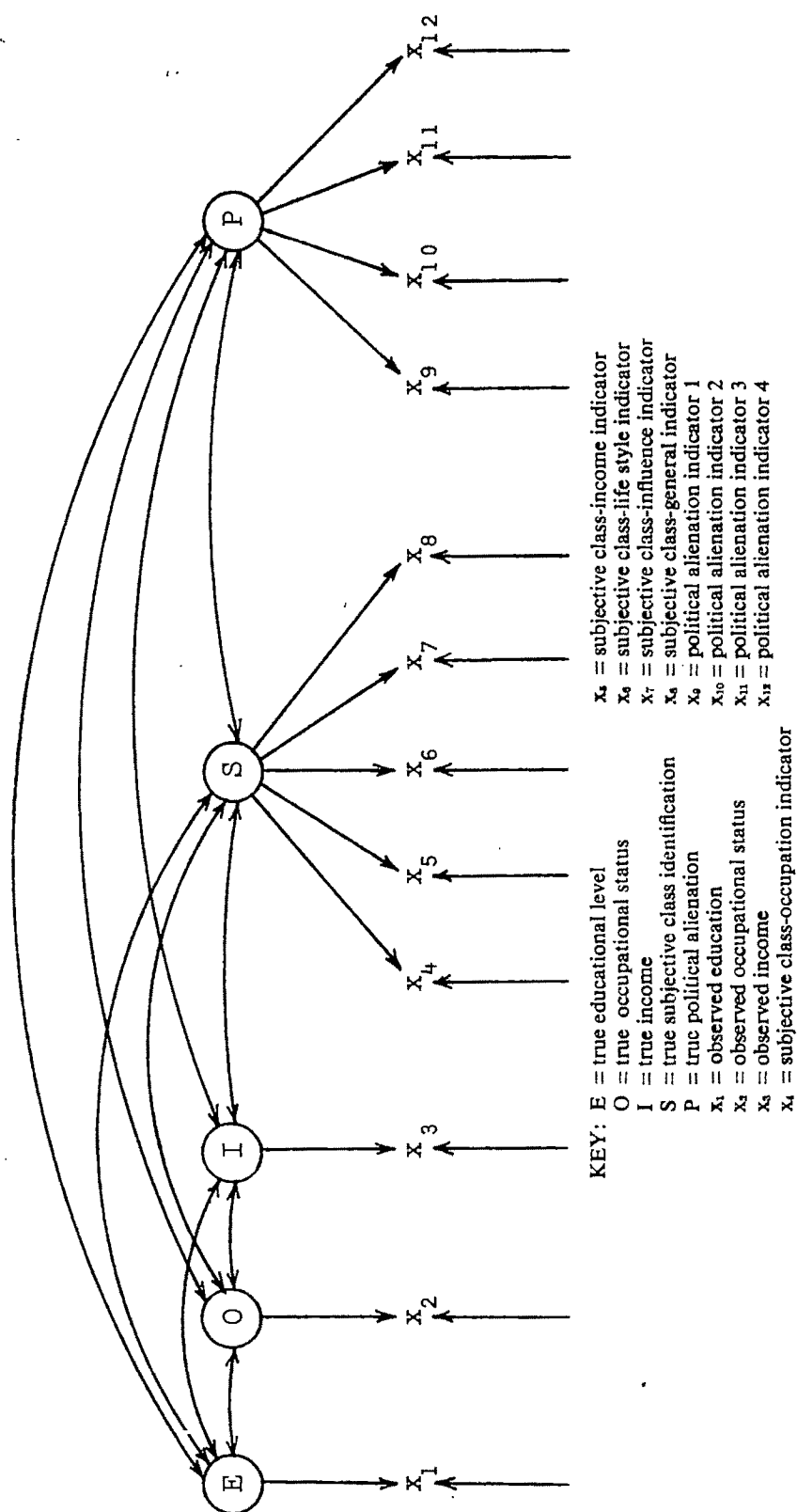


Figure 1. Multiple Indicator Measurement Model (Model I) of Subjective Class Identification

of the Model I estimates for the black and white samples. The χ^2 's indicate a poor fit for both samples. Hence, it is necessary to consider alternative models.

As an initial explanation, we consider the possibility that the poor fit is due to the multidimensionality of subjective class position. Inspection of the pattern of residuals under Model I reveals sizeable discrepancies in the correlations of income (x_3) with the subjective class-income measure (x_6) and of occupation (x_2) with the subjective class-occupation measure (x_4). This suggests an alternative model (Model II) with two separate dimensions of subjective class, one based on income and occupation and the other based on life-style, influence, and general class status. If income and occupational status were more highly correlated with the former dimension than with the later, then the noted deviation in residuals could result.

With 42 degrees of freedom in Model II, the χ^2 for whites = 69.01 ($p = .004$) and for blacks = 85.26 ($p < .001$). Although Model II fits the data better than Model I, as indicated by the reduced χ^2 , the fact that this value is substantially larger than the degrees of freedom indicates that the fit is still inadequate. Further, the hypothesis of multidimensionality loses plausibility in the face of an extremely high correlation between the two hypothesized dimensions of subjective class (for whites, $r = .96$; for blacks, $r = .91$).

On the basis of these findings, we considered various sources of measurement invalidity as explanations for the failure of Model I. An examination of the residuals produced with Model I, plus estimation of models with between-construct (subjective social class and political alienation) error correlations, led to the delineation of the final models (Models IIIa and IIIb) presented in Figures 2a and 2b. Table 3 presents the residuals and χ^2 tests for these respecified models. The χ^2 's indicate a marked improvement in fit in both samples, which leads us to conclude that these models offer an acceptable fit to the observed correlations.⁶

⁶ The decision that Models IIIa and IIIb constitute an acceptable fit (i.e., the decision to stop fitting)

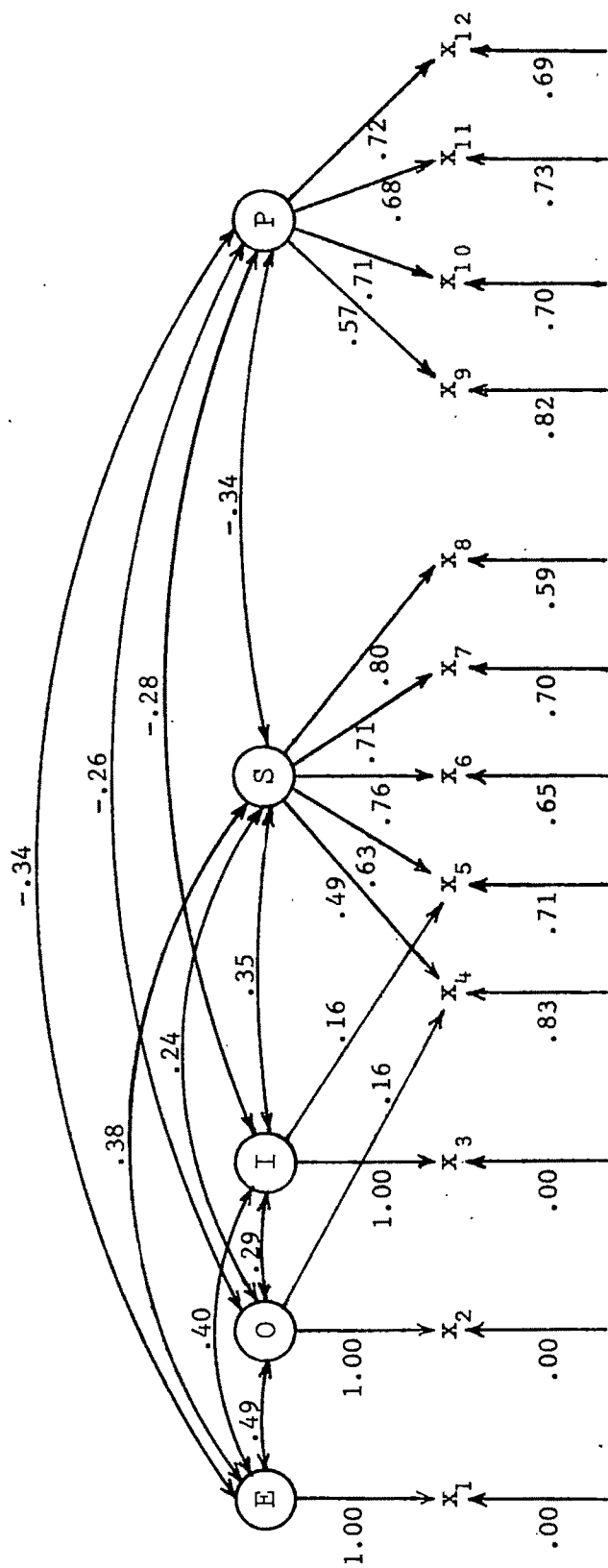
Table 2. Residuals ^a for Model I for Whites (below Diagonal) ^b and Blacks (above Diagonal) ^c

Variables	x_1	x_2	x_3	x_4	x_5	x_6	x_7	x_8	x_9	x_{10}	x_{11}	x_{12}
x_1 education												
x_2 occupation	.000											
x_3 income	.000	.000										
x_4 s.c.-occupation	.005	.137	.000									
x_5 s.c.-income	.022	-.023	.025	.000								
x_6 s.c.-life style	-.029	-.044	.111	.009	.020							
x_7 s.c.-influence	-.009	-.020	.026	-.014	.020	.016						
x_8 s.c.-general	.014	.016	.080	.005	-.035	.000	.004					
x_9 pol. alienation 1	.063	.032	-.032	.001	-.022	-.001	.030	.081				
x_{10} pol. alienation 2	.008	.009	-.019	.015	-.052	.008	-.019	.006	.043			
x_{11} pol. alienation 3	-.015	-.049	.000	-.022	-.053	.038	-.057	.011	.062	.020		
x_{12} pol. alienation 4	-.013	.015	.054	-.021	.034	.048	-.023	.023	.020	-.012	-.011	
				.000	-.110	.042	-.009	.035	.024			

^a All diagonal elements = .000.

^b $\chi^2 = 81.68$; $df = 47$; $p < .001$.

^c $\chi^2 = 119.50$; $df = 47$; $p < .001$.



Note: All variables are labeled in Figure 1.

Figure 2a. Maximum Likelihood Estimates of Revised Final Model for Whites (Model IIIa)

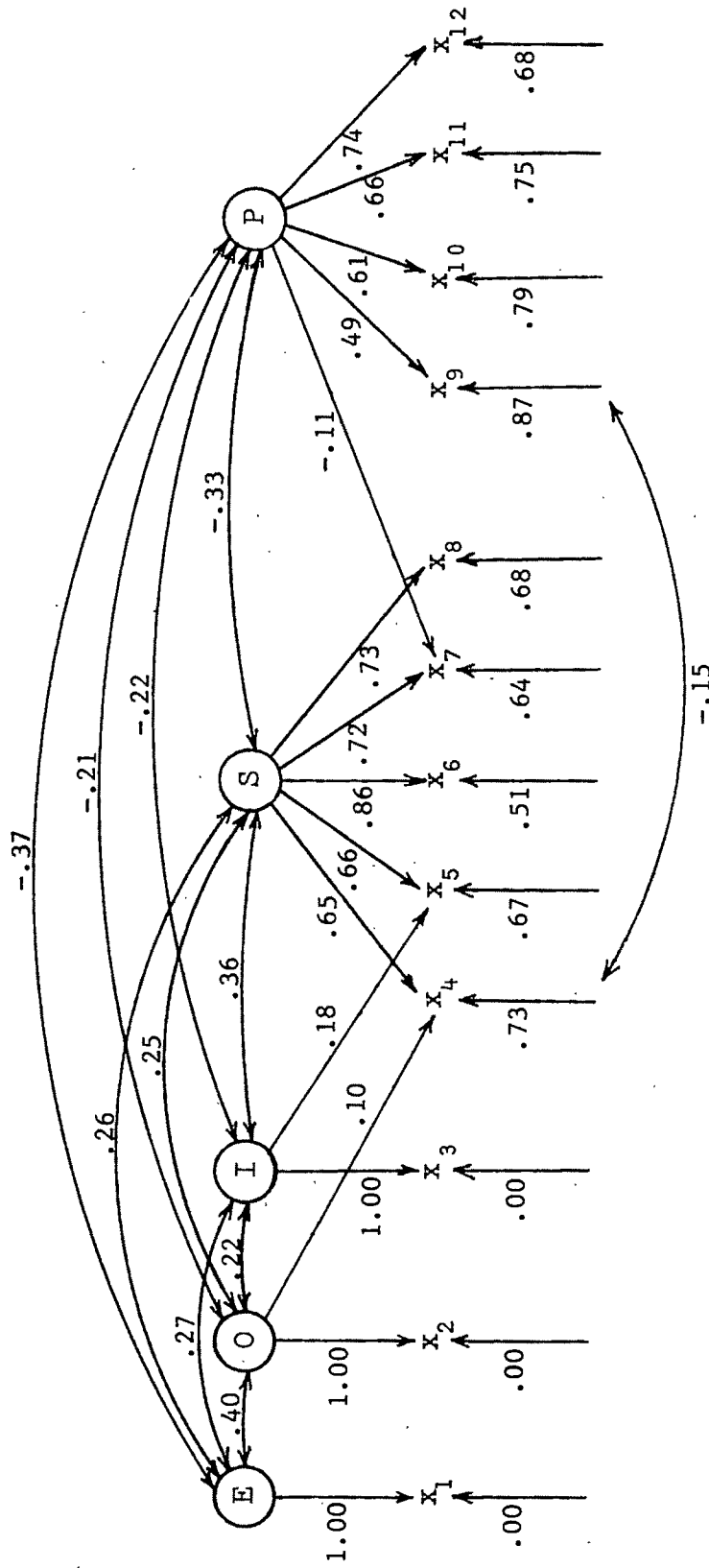


Figure 2b. Maximum Likelihood Estimates of Revised Final Model for Blacks (Model IIIb)

Table 3. Residuals^a for Model IIIa (below Diagonal)^b and Model IIIb (above Diagonal)^c

Variables	X ₁	X ₂	X ₃	X ₄	X ₅	X ₆	X ₇	X ₈	X ₉	X ₁₀	X ₁₁	X ₁₂
X ₁ education												
X ₂ occupation	.000											.004
X ₃ income	.000	.000										.015
X ₄ s.c.-occupation	.048	.000										.004
X ₅ s.c.-income	.003	.034	.000									.009
X ₆ s.c.-life style	.014	.022	.058	.016	.060	.022	.035	.059	.032	.030	.014	.018
X ₇ s.c.-influence	.002	.002	.054	.009	.031	.007	.008	.009	.001	.021	.038	.018
X ₈ s.c.-general	.027	.037	.001	.003	.016	.006	.018	.076	.013	.047	.045	.003
X ₉ pol. alienation 1	.063	.032	.019	.026	.044	.003	.022	.002	.023	.031	.011	.031
X ₁₀ pol. alienation 2	.008	.009	.000	.008	.043	.032	.061	.006	.010	.055	.007	.024
X ₁₁ pol. alienation 3	.015	.049	.054	.008	.043	.042	.027	.017	.008	.019	.031	.034
X ₁₂ pol. alienation 4	.031	.015	.035	.014	.100	.035	.013	.030	.024	.011	.011	

^a All diagonal elements = .000 except X₈ (blacks) = .002 and X₉ (blacks) = .003.

^b $\chi^2 = 54.02$; df = 45; p = .170.

^c $\chi^2 = 68.72$; df = 43; p = .010.

Discussion

As noted above, there are various theoretical reasons for questioning the common practice of treating subjective social class as a unidimensional construct. However, while multidimensional approaches to subjective class have been recommended frequently, empirical research consistently has ignored this issue. A major finding of this study is that a model assuming unidimensionality among a set of indicators of subjective social class provides a better fit to the data than plausible models assuming more than one dimension. Further, although the final models do not fit the data equally well for blacks and whites, this finding holds for both subsamples.⁷ Thus, we have found nothing to disconfirm the assumption of unidimensionality which is presumed by all previous measures of subjective class identification.

Given that subjective class is unidimensional, the various indicators may still be more or less adequate measures of the common factor. That is they may have differential reliabilities and validities as well as being subject to varying sources of nonrandom error. Direct comparison of the indicators of subjective class can be made utilizing the information provided in Figures 2a and 2b. It should be recalled that coefficients linking indicators to the unobserved variable which they are intended to measure may be interpreted as validity estimates and, when squared, as

was made because any further specification would appear to be capitalizing on chance variation. According to Jöreskog (1971:132), the problem of when to stop fitting should be based upon differences between χ^2 values rather than the χ^2 values themselves. If new models yield a reduction in the χ^2 value which is "large compared to the difference in degrees of freedom this is an indication that the changes made in the model represent a real improvement. If, on the other hand, the drop in χ^2 is close to the difference in number of degrees of freedom, this is an indication that the improvement in fit is obtained by 'capitalizing on chance' and the added parameters may not have real significance and meaning." For our data, the drop in χ^2 values would not be large enough to justify the introduction of additional parameters.

⁷ Bielby et al. (1976) have reported a similar difficulty in obtaining equally good fits of measurement models of socioeconomic status measures in nonblack and black samples.

estimated reliabilities.⁸ Comparison of estimated reliabilities and sources of invalidity among the indicators reveals that either the standard general item (x_8) or the item referring to life style (x_9) offers the best single-item measure of subjective social class. Their estimated reliabilities are equally high and no invalidities were detected for either. For blacks, the estimated validity of the standard item is .72; hence, the estimated reliability is .53. For whites, the estimates are .80 and .64, respectively. To our knowledge, these are the first such estimates available in the literature.

Subjective class identifications based upon occupation (x_4) and income (x_5) are the poorest indicators of subjective class, in terms of both their lower estimated reliabilities and their clearly identified sources of invalidity. For blacks and whites, each of these indicators is directly affected by the objective status factor

⁸ It should be noted that our reliability estimates may not apply to other populations. Such generalization assumes that the ratio of true score to error variance is equal across populations. Although it may be reasonable to assume equality of error variance within different populations—since measurement error is often considered to be a property of the measurement instrument (Wiley and Wiley, 1970:112)—it is not always reasonable to assume equality of true score variance across populations. Nor can one assume that increased (or decreased) true score variance will bring a proportional increase (or decrease) in error variance.

The above problem applies to the comparison of estimated reliabilities between corresponding measures from the black and white samples in the present study. The problem may be circumvented, however, by basing comparisons on absolute error variance components rather than standardized coefficients. By fixing one loading of each construct (subjective class and political alienation) at unity and analyzing the variance-covariance matrix of indicators instead of the correlation matrix, the following estimates of error variance components were obtained for the measures of subjective class identification:

	Whites	Blacks
subjective class-occupation	.311	.325
subjective class-income	.269	.376
subjective class-life style	.162	.159
subjective class-influence	.207	.353
subjective class-general	.137	.286

The values of the standardized coefficients can be used legitimately to make comparisons of relative reliabilities within the two samples. However, any comparison of relative reliabilities between samples should be based on the above error variance components and the observed score variances.

upon which the class identification is based (note the paths from 0 to x_4 and from 1 to x_5 in Figures 2a and 2b); and among blacks, the subjective class-occupation item has an additional invalidity due to correlated indicator error across the subjective class and political alienation factors. Apparently, subjects responded to these two items both as questions about class location and as questions about the relative rank of their occupational and income statuses. Also, invalidity in the influence item (x_7), shown in Model IIIb, suggests that blacks may respond to this item on the basis of their perceived political alienation as well as their class placement.

With respect to possible nonrandom errors not detected in the analysis, it should be noted that the pattern of residuals under Model I implies the absence of invalidity due to a generalized response set. Had a common source of variation in the indicators of subjective class and political alienation been present, then a pattern of residuals suggesting the underestimation of correlations between these indicators would have been generated by Model I.

One of the advantages of multiple indicator approaches to measurement is that reliability estimates may be computed and these may be used to correct correlations for attenuation due to random error. In the case of confirmatory factor analysis, the correlations between latent traits represent correlations between "true scores" for the associated constructs (Jöreskog, 1971). In other words, these correlations have been corrected for attenuation due to unreliability of measures. The advantage of the multiple indicator measurement of subjective class over measurement by the standard class identification item may be demonstrated by comparing regressions on the usual measures of objective social class.

For the Gary data, regression of the standard item upon the three objective class measures, education, occupational status, and family income, results in a squared multiple correlation of .079 for the black subsample and .138 for the white subsample. By comparison, a parallel regression for the latent subjective class factor, using multiple indicators, results in

squared multiple correlations of .174 and .189 for blacks and whites, respectively.⁹

Two points are immediately obvious. First, the proportion of subjective class variance explained by the uncorrected regression with the standard item is considerably less than that explained by the corrected regression with measurement by multiple indicators. Since it has been common practice to use the standard class identification item without regard to its reliability, this suggests that prior findings underestimate the degree to which objective and subjective class may be related.¹⁰ Second, however, even with the improved reliability of the composite measure, the relationship between objective and subjective class remains weak. While prior research may have underestimated the relationship by virtue of ignoring unreliability and employing a single indicator, it appears unlikely that increased reliability via multiple indicators will produce a high observed correlation between objective and subjective class. By and large, it remains for theory and substantive research to explain the large residual.

Conclusion

Subjective social class identification has been a central concept in much of the theory and research on social stratification but, in spite of its importance, little attention has been paid to the adequacy of its measurement. By formulating a multiple indicator model, we have been able to

address certain key issues in the measurement of subjective class. Although there are theoretical reasons to question the validity of the conventional single-item measure, our analysis shows that this measure fares quite well in comparison to other plausible indicators. However, as we have demonstrated, the reliability of the conventional measure can be improved by constructing a composite measure.

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⁹ The unstandardized regression coefficients for whites are: Single Item (x_0): Subjective Class = $.119x_1 + .002x_2 + .047x_3$; Latent Subjective Class Factor: Subjective Class = $.110x_1 + .001x_2 + .052x_3$.

The unstandardized regression coefficients for blacks are: Single Item (x_0): Subjective Class = $.092x_1 + .002x_2 + .072x_3$; Latent Subjective Class Factor: Subjective Class = $.070x_1 + .006x_2 + .097x_3$.

¹⁰ The squared multiple correlations (SMCs) in our data, using the conventional measure, are considerably lower than that found by Jackman and Jackman (1973:574) in the 1964 NORC general survey. We suspect that the lower SMC we obtained for the standard measure is due to the nature of our sample, which is restricted to a single urban, highly industrialized city. It seems likely, therefore, that the use of multiple indicators to measure subjective class in a national sample would result in similar increases in the SMC over the single indicator without correction for attenuation.

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DEMOCRACY AND SOCIAL INEQUALITY: A REANALYSIS*

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Papers by Cutright and Jackman test the hypothesis that democratization reduces inequality in national societies. Cutright finds a negative effect of democratization on inequality, while Jackman finds no effect. In this paper, we reanalyze and extend these analyses. We consider five issues and our findings are the following: (1) the two different indexes of democratization used are not the likely cause of the discrepant findings; (2) the different samples are not the likely cause; (3) the probable cause of the different results seems to be in the data used or coding procedures that Cutright and Jackman employed in constructing their measure of sectoral income inequality; (4) substituting personal income for sectoral income in the analysis, we find that both the Cutright and Jackman indexes have negative effects on inequality; (5) we test the alternative specification which hypothesizes that inequality has a negative effect on democratization, and then test these two different hypotheses. This analysis supports the hypothesis that inequality has a negative effect on democratization, but there is less empirical support for the original hypothesis that democratization negatively affects inequality.

In a recent paper, Jackman (1974) addressed the issue of the relationship between political democracy and social inequality. Jackman's analysis was based on an hypothesis of Lenski (1966) which argued that the rise of democracy was one of the reasons for the decline of the degree of inequality in industrial societies relative to agrarian societies. Jackman's empirical analysis compared the Lenski hypothesis that political democracy reduces inequality with an alternative hypothesis that the relationship of democracy to inequality is spurious, once the level of a country's economic development is controlled. His

empirical results supported the hypothesis of a spurious relationship between democracy and inequality. These results, however, contradict the findings of an earlier study by Cutright (1967). Cutright investigated the same set of relations, and his empirical results showed that there was a significant negative effect of democracy on inequality, even controlling for the level of economic development. Thus, two similar studies investigating the same hypothesis reach different conclusions. Because the relationships among the three variables of democracy, inequality and economic development are of continuing concern in comparative politics (Banks, 1972; Cnudde, 1972; Lipset, 1960; Moul, 1974), it is important to reanalyze these findings.

Cutright's study explicitly tested the hypothesis that political democracy re-

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duces inequality in nations. He tested this hypothesis through a cross-sectional regression analysis, using a sample of 52 nations. Inequality was measured by the Gini index of *sectoral* income inequality, and democracy was measured by the political representation index (PRI) developed by Cutright (1963). Economic development was measured by an economic development index (EDI). Cutright (1967) used a stepwise regression procedure in which the economic development index was introduced first and then followed by the measure of democracy, PRI. He found that democracy exerted an independent negative effect on sectoral income inequality.¹ He concluded that political democracy does reduce inequality, a finding that he interpreted as supporting the Lenski hypothesis.

Jackman's (1974) analysis also tested this hypothesis through a cross-sectional regression analysis, using a sample of 60 nations. He used three indicators of social equality, the Schutz coefficient of *sectoral* income inequality, SIPE and a social welfare index.² He measured democracy by an index of democratic performance (DP). Jackman performed three separate regression analyses in which each indicator of social equality was regressed on the ln-energy consumption per capita (KWH) and the index of democratic performance. In all three regressions, the measure of democracy had no significant effect on the indicators of social equality.³ From these results, Jackman concluded that there was no independent effect of democratization on inequality.

¹ The stepwise regression procedure performed by Cutright could only have underestimated the effect of PRI, since this index of democracy was entered after the economic development index.

² While Cutright summarized sectoral income distribution by the Gini coefficient and Jackman summarized this distribution by the Schutz coefficient, these different summary coefficients cannot account for the discrepant findings since Jackman (1975:17) reports that Alker and Russett found that the two coefficients, if computed from the same distributions, correlate at .95.

³ The fact that Jackman used kilowatt-hours of electricity consumed and Cutright, at times, used an economic development index does not seem to be a source of the discrepant findings, since such various measures of economic development all tend to correlate very highly.

In trying to understand these divergent findings, we focus on four issues raised by these analyses: the different indexes of democratization used, the different samples of countries used, the relationship between sectoral and personal income inequality, and the important issue of the direction of the causal relationship between democratization and inequality.

We also should point out that Cutright and Jackman have extracted only one very small hypothesis from the very rich and complex theoretical model developed by Lenski. Their results and those we will report therefore should not be interpreted as confirming or disconfirming Lenski's theoretical model.

Comparison of the Measures of Democratization

In discussing the fact that his results do not agree with those of Cutright, Jackman focuses on the issue of the measurement validity of the indexes of democratization. Jackman argues that his index of democratic performance more accurately measures what most theories mean by democracy because it emphasizes political participation and freedom of competition. Cutright's index, he argues, measures political stability more than democracy because of its emphasis on the existence of parliamentary forms of government (Jackman, 1974:43).

The operational procedures used in constructing the two measures are, in fact, quite different. Jackman's index consists of four items of (1) the ratio of the number of adults voting to the total voting age population, (2) the competitiveness of the party voting system as coded by Rummel (1966), (3) the degree of press freedom as presented in Taylor and Hudson (1972) and (4) the degree of electoral irregularity as presented in Taylor and Hudson. These four components were equally weighted and summed to give the score on democratic performance. Cutright's (1963:256) political representation index is constructed in the following way: a nation is given a score of two points for each year that "a parliament existed in which the lower or the only chamber contained representatives of two or more political

parties and the minority party or parties had at least 30% of the seats." One point was given to those countries that complied with the above criteria but lacked the 30% criterion. Nations for that year that had no parliament or had the parliament abolished by executive fiat received no points. Countries received an additional point if the chief executive was in power by virtue of a direct vote or was selected by the party in power in parliament, if the parliament satisfied the above two criteria. One-half point was given to nations that selected their chief executive by other means, but no additional points were given if the nation was ruled by an hereditary ruler or a foreign power. The index consisted of the total points for each year from 1945-1954. Thus, in contrast to Jackman's, Cutright's index stresses the formal structure of the regime and its stability, and it is for these reasons that Jackman asserts that his measure is a more valid measure of democratization while Cutright's is a measure of stability.

While there have been critiques of the conceptualization and operationalization of indicators of democratization (Burrowes, 1972; Moul, 1974), our concern here is with a more narrow question: Do these two indexes measure different aspects of a nation's political structure, as Jackman claims, or are they empirically indistinguishable? In order to answer this question, we inspected the correlation matrix and performed a factor analysis of these two indexes and a third index of democratization, Dahl's (1971) polyarchy measure. We included this third index because Jackman states that his measure of democratization is conceptually similar to Dahl's polyarchy measure (Jackman, 1975:64). Two different samples of countries were used in this analysis—the first sample ($N=47$) consisting of all nations which have scores on all three indexes of democratization, and the second sample ($N=32$) consisting of countries which have scores on the democracy indicators and have data on personal income distribution.⁴

⁴ The scoring for Jackman's Democratic Performance (DP) index is taken directly from Jackman (1975:216-8). Cutright's Political Representation Index (PRI) is coded as five-year averages, for the

We found that the correlations among the three indicators of democratization are all relatively high. In the larger sample, the correlation between the Jackman and Cutright indexes is .8182; and both of these indexes have essentially the same correlation with Dahl's index, .7201 for Jackman and .7330 for Cutright. In the smaller sample, the correlation between the Jackman and Cutright indexes is .6783; and the two correlate at .6063 and .6038, respectively, with Dahl's index. The relatively high magnitude of these correlations implies, to some extent, that it is not possible to simply assert (as does Jackman) that the different effects found by Jackman and Cutright are explained by the fact that the two indexes measure different political dimensions.

In the factor analyses, a principal factor solution was applied to the correlation matrix of indicators (Van de Geer, 1971:128-55). A strong indication that the three measures of democratization are empirically indistinguishable would be if the first factor to emerge were to account for a disproportionately large share of the variance of the three measures and the two subsequent factors accounted for a relatively small share of the variance. This is what we found. In the first sample of 47 nations, the first factor to emerge accounted for 83.8% of the variance in the democratization indexes; the second factor accounted for 10.1% of the variance; and the third factor accounted for 6% of the variance. The eigenvalue for the first factor was 2.282. Further, the factor loadings on this factor are all quite similar. Cutright's measure has the highest loading, .9118; Jackman's measure has the next highest loading, .8967; and Dahl's measure has the lowest loading, .8038.⁵

five years closest to the income inequality measure which will be discussed later.

⁵ The lower loading of the Dahl polyarchy index is probably due to the fact that we could not use Dahl's full two-dimensional measure because we knew of no way to weight the two dimensions (which Dahl presents separately) and combine them into one index. We used only the dimension of the "degree of competitiveness of a nation's political system," and did not use his dimension of inclusion, the extent to which all citizens are included in the political process. For this reason we do not include Dahl's index in later analyses.

The smaller sample showed essentially the same pattern of results.

In both samples, the first factor accounts for very substantial proportions of the variance and the subsequent factors account for very little of the variance. Also, the loadings of the indexes are quite consistent across the two samples. These results cast doubt upon the assertion that these measures of democratization are capturing distinctly different dimensions of a nation's political structure and that this is the reason for the different findings of Jackman and Cutright. Rather, these results cast doubt on Jackman's claim that the difference between his findings and Cutright's findings are due solely to the fact that their indexes of democratization are empirically measuring different dimensions of political structure.⁶

Comparison of the Samples and of Sectoral Income Inequality

A second reason why the two studies reach different conclusions may arise from different samples. Both Jackman and Cutright used only those countries for which there was complete data on their various indicators. Jackman's sample had 60 countries, while Cutright's sample had 52 countries. However, comparing the samples shows that they had 44 cases in common. Since Jackman's index of democratic performance was constructed from secondary sources, it is impossible to create identical samples. We suspect that because of the high proportion of common cases the different samples alone cannot explain the discrepant findings.

However, another factor which may explain the discrepant findings did appear. This factor concerns the measures of sectoral income inequality. Both Jackman and Cutright used sectoral income in-

equality as a measure of inequality. Cutright constructed his sectoral income measures himself, while Jackman took his sectoral income measure from Taylor and Hudson (1972). While they both used the same conceptualization of sectoral income inequality, it is possible that errors resulting from coding or from using different sources for determining sector income and workers per sector could be responsible for the discrepant findings. The two scorings of sectoral income inequality obviously should correlate very highly.⁷ When we correlated the two measures for the 44 common cases, however, we obtained a correlation of only .645. Although we cannot determine exactly the source of this low correlation, we can conclude that the difference in findings between Cutright and Jackman may be due to the fact that the two measures of sectoral income inequality do not correlate highly.⁸

Reanalysis Using Personal Income Distribution

If the difference in the findings is due to differences in the scoring of sectoral income inequality, we still cannot determine which result is correct. Both Jackman and Cutright assert, however, that sectoral income inequality is not as good a measure of social inequality as is actual personal income distribution (Cutright, 1967:563; Jackman, 1975:19-21). Both Jackman and Cutright were forced to use this measure because they did not have measures of personal income distribution. Sectoral income inequality is a measure originally developed by Kuznets (1963). It is computed by dividing the per-

⁷ See footnote 2.

⁶ Strictly speaking, without more analysis, we cannot conclude that because the correlations among these indexes can be reproduced by a common factor that these variables are measuring the same dimension, since the variables may be spuriously related. However, our aim is merely to show that the Jackman and Cutright indexes are so empirically interrelated that the difference between them is not sufficient to account for the different empirical results in their studies.

⁸ Another factor that could produce different results stems from the fact that the two studies used somewhat different time periods. Cutright's study seems to pertain to the mid-1950s, while Jackman's study seems to pertain to the mid-1960s. We say "seems to" because it is not possible to ascertain from the sources exactly what are the actual time periods for the data. And, in fact, Jackman (1974:33) notes that he uses some data from Cutright's original study. Our analyses in the next section, however, show that both Jackman's and Cutright's democratization indexes have the same effects on personal income distribution, so we feel that the somewhat different time periods are not causing the problem.

centage of total domestic product produced in eight economic sectors (agriculture, mining, manufacturing, etc.) by the number of workers in each sector. While this computation only gives the distribution of per worker product across economic sectors, both Cutright and Jackman use it as a proxy for personal income. Using sectoral income for this purpose, however, assumes that the income within each production sector is equally distributed among all economic actors within the sector. This assumption is unrealistic (Husbands and Money, 1970). A better test of the Lenski hypothesis of the effect of democratization on inequality is to use personal rather than sectoral income inequality. To do this, we take income distribution data from Paukert (1973), who presents data showing the share of pre-tax personal income accruing to the five quintiles of households in 56 countries. We use this data to reanalyze the relationship between democratization and inequality.⁹

To test the hypothesized relationship using the Gini index of *personal income inequality* (computed by Paukert, 1973) as the dependent variable, we use the exact functional form of the regression equation used by Jackman:

$$Y_1 = a + b_1X_1 + b_2X_2 + e \quad (1)$$

where: Y_1 = Gini index of personal income inequality

X_1 = Jackman or Cutright democracy index

X_2 = ln kilowatt-hours of energy per capita (KWH).

The results from the estimation of equation (1), with t-scores in parentheses, are:

$$\begin{array}{ll} \text{Gini Index} = .0003 \text{ KWH} - .033 \text{ Jackman}^* & \\ \quad (.237) & (2.39) \\ R^2 = .188, N = 32 & \end{array}$$

$$\begin{array}{ll} \text{Gini Index} = .0005 \text{ KWH} - .006 \text{ Cutright}^* & \\ \quad (.434) & (1.88) \\ R^2 = .155, N = 32. & \end{array}$$

These results show that the effect of Jackman on the Gini index of personal income inequality is $-.033$, with a significant t-statistic of 2.39.¹⁰ Other results, not presented here, also show that Jackman has significant effects on increasing the income of the bottom 20% of the population and the middle 40–60% and decreasing the relative share of the income of the top 20% of the population. Thus, when we substitute personal income as the measure of social inequality, a measure that Jackman himself claims is superior to his own, we find that his indicator of democratic performance does have significant effects on reducing income inequality, even controlling for the level of economic development.¹¹

These results also show that the effect of Cutright on the Gini index is $-.006$, with a significant t-statistic of 1.88. Other results, not presented here, also show that Cutright has a significant positive effect on the share of income occurring to the bottom 20% of the population and a significant negative effect on the share of income of the top 20% of the population. Thus, these results parallel those using Jackman's measure of democracy. They show that Cutright's political representation index has significant negative effects on personal income inequality.

From this reanalysis, we can conclude that the two indicators of democratization both have negative effects on social inequality. These results tend to support our earlier analyses which showed that both indicators of democracy were empirically very similar. At this point, the analysis seems to support the hypothesis that democratization has a negative effect on inequality.

⁹ Our data for personal income distribution are for different time periods, with most of the data being from the mid-1960s. Therefore, we matched the other variables to the exact year of the income inequality data. Cutright's index was constructed as five-year averages for the five years closest to the income inequality measure. Kilowatt-hours was coded for the exact year of the inequality measure. Since Jackman's index cannot be replicated (as can Cutright's we had to use the original scores presented by Jackman (1975).

¹⁰ We use a significance level of .05 indicated by an asterisk (*) with a one-tailed test throughout the paper.

¹¹ The number of cases for which we could get data on personal income inequality, kilowatt-hours, and the Jackman and Cutright indexes is 32. Though this number is smaller than that used in the other studies, the results from this and subsequent analyses are so consistent that we have confidence in the findings.

An Alternative Specification

If we were to end the analysis at this point, we would conclude that political democracy does have a significant negative effect on inequality. However, we now consider an issue which was not addressed by either Jackman or Cutright, the issue of alternative specifications. Both Jackman and Cutright model the relationship between democracy and inequality as a one-way causal relationship in which democracy affects inequality. Neither entertains the possibility that inequality affects democracy.

Although neither Jackman nor Cutright discuss this alternative causal ordering, there is a large body of research which leads to the implication that inequality has an important effect on democratization. The reasoning behind such an assertion is the following. Observation of inter-country differences in income inequality shows that most of the variation occurs in the middle of the income distribution. The countries which exhibit the greatest income *equality* are those which have a greater share of income going to the third and fourth quintiles of the population and a smaller share of income going to the top 20% of the population. Countries with the most unequal income distributions are those in which the top 20% of the population has a relatively large share of the income and the middle quintiles have a relatively small share (Paukert, 1973). That is, the empirical evidence shows that inter-country differences in inequality are largely a function of the relative strength of the middle-class groups in a country. These observations lead to the conclusion that the shape of the income distribution is primarily a function of the relative strength of economic groups in a country, with the lowest amount of inequality being associated with large and economically powerful middle-class groups. Thus, when we compare countries on inequality, basically we are comparing their class structures and, particularly, the degree to which they are dominated by a middle class. It is interpreting inequality as an indicator of class structure that leads to the hypothesis that social inequality affects democratization.

There are a number of historical studies which point to the hypothesis that the class structure of a country has an important effect on democratization. Such a perspective immediately suggests Marx's analysis which argued that the rise and institutionalization of the bourgeoisie lay behind the shift to parliamentary democracy in European countries. This relationship between the bourgeoisie and democratization arose because the rising capitalist classes used parliamentary mechanisms to wrest control of the state from the Crown and the traditional landed elite (Marx, 1926). Similarly, Moore (1966:413-32), in his famous study of political regimes, argues that the urban bourgeoisie has been the crucial group in the major democratic revolutions. It is the existence of a strong and organized urban bourgeoisie, he concludes, which has been the necessary condition for the institutionalization of democratic forms of government. Soboul (1975), in his analysis of the French Revolution, emphasizes that the triumph of legal equality (the classic conception of democracy) over other visions of equality was established by the eventual control of the French state by the urban bourgeoisie.

Such studies lead to the implication that the degree of social inequality, because it is an indicator of class structure, affects democratization. This hypothesis can be tested by estimating equation (2) below. Economic development also is included in this equation as one of the determinants of democratization (Cutright, 1963; Jackman, 1975; Neubauer, 1967).

$$X_1 = a + b_1 Y_1 + b_2 X_2 + e \quad (2)$$

where: X_1 = Jackman or Cutright democracy index

Y_1 = personal income inequality, Gini index or 3rd Quintile

X_2 = 1n kilowatt-hours of energy consumption per capita (KWH).

This equation differs from equation (1) because the causal relationship between democracy and inequality is reversed. Besides presenting the effects of the Gini index on democratization, we also present the effects of the 3rd quintile (the middle

40–60%) of the population here because it represents the proportion of income which accrues to the middle of the income distribution and, thus, is one indicator of the economic strength of the middle-class groups in the countries. According to the works cited above, we expect that this variable should have *positive* effects on democratization and that the Gini index should have *negative* effects. While it is not necessary that the larger the income share going to the 3rd quintile, the less the inequality, it is empirically true in the data on income distribution presented by Paukert (1973) and analyzed by Robinson (1976). For example, the correlation between the relative proportion of income accruing to the 3rd quintile and the Gini index is $-.889$ in the sample including Cutright, and this correlation is $-.875$ in the sample including Jackman. The results from the estimation of equation (2), using the two indexes of democratization and the two measures of inequality, are presented below with the t-scores in parentheses:

$$\text{Jackman} = .035\text{KWH}^* - 56.41 \text{ Gini index}^* \\ (2.65) \quad (2.39)$$

$$R^2 = .345, N = 32$$

$$\text{Cutright} = .024\text{KWH}^* - 17.11 \text{ Gini index}^* \\ (5.45) \quad (1.88)$$

$$R^2 = .580, N = 32$$

$$\text{Jackman} = .023\text{KWH} + 2.08 \text{ Quintile 3}^* \\ (1.68) \quad (3.08)$$

$$R^2 = .409, N = 32$$

$$\text{Cutright} = .022\text{KWH}^* + .529 \text{ Quintile 3}^* \\ (4.49) \quad (1.94)$$

$$R^2 = .583, N = 32.$$

The results of these equations show that the Gini index has significant negative effects on both the Jackman and Cutright indexes of democratization. These results also show that there are significant positive effects of the 3rd quintile on both these measures of democratization. Both sets of results are consistent with the hypothesis outlined above.

The results from equation (1) support the hypothesis that democratization has a significant negative effect on inequality, and the results from equation (2) support the alternative specification that inequality has a significant negative effect on democratization. These results, however,

arise from the independent estimation of each equation by ordinary least squares (OLS), and therefore it is difficult to determine the relative validity of each specification. It seems reasonable now to attempt to make a preliminary determination about the relative validity of each specification by testing a model which allows for reciprocal effects between democratization and inequality. Since in their present form equations (1) and (2) cannot be estimated by OLS, we will estimate this system of equations through the method of instrumental variables (IV).

Before proceeding with the instrumental variables analyses, we caution the reader on two issues. First, our analysis is intended to be exploratory. We have found that there is theoretical and empirical support both for the assertion that democratization affects inequality and for the assertion that inequality (interpreted as class structure) affects democratization. The empirical cross-national research that we have been discussing has concentrated exclusively on evaluating the first assertion. We have shown, using the same type of data, sample and methods, that there is equal support for the second assertion. It seems necessary, therefore, to begin to make some assessment of the relative merits of each assertion, since the issues raised here are central for the development of an adequate model of the relationships between economic and political structure. Our analysis, therefore, should be seen as only a first step in this task, and our results should be considered tentative until there is much more research on this issue.

Second, we alert the reader to the complicated issue of the choice of instrumental variables. The two requirements for an instrumental variable are that it is uncorrelated with the error in the equation and that it is correlated with the regressor (Wonnacott and Wonnacott, 1970:159). Given these requirements, a very large range of variables may be acceptable, even variables which are not theoretically relevant. Consequently, it is often stated as a "conservative" guide that instruments should be chosen from within a fully developed structural model (Wonnacott and Wonnacott, 1970:160). Unfor-

tunately, such a model has yet to be developed for the interrelationships we are studying. Our analysis, in fact, is one part of an attempt to develop such a model. We proceed, then, to construct a slightly larger model in which we add one theoretically relevant variable to each equation.

Two variables, an index of Horizontal Power Distribution (POWCON) and government revenue as a percentage of gross domestic product (GOVREV) will be added to the model. If we refer to GOVREV as X_3 and POWCON as X_4 , and retain the symbols used previously for the remaining variables, the specification of the model is:

$$Y_1 = a_1 + b_1X_1 + b_2X_2 + b_3X_3 + e_1 \quad (3)$$

$$X_1 = a_2 + b_4Y_1 + b_5X_2 + b_6X_4 + e_2. \quad (4)$$

This model produces a just-identified system of equations that satisfies the assumption that the errors are uncorrelated with the independent variables in the equations when the system is estimated by the method of instrumental variables (Wonnacott and Wonnacott, 1970:152-63).¹²

The Horizontal Power Distribution index (POWCON), created by Banks and Textor (1963:106), measures the degree to which the different branches of govern-

ment (whatever they may be) exercise the functions normally attributed to them, or whether they are dominated by another branch of government or extra-governmental agency. This variable is scored 0, 1 and 2, with 0 representing a more equal power distribution among governmental branches and 2 representing a more unequal or concentrated power distribution. The Horizontal Power Distribution index captures the idea of countervailing governmental power or "checks and balances" which is so often postulated to be a necessary condition for democratization (Dahl, 1971; Lipset, 1960). According to our perspective, it provides the institutional conditions that allow societal competition to be politically organized and legitimated within the government. Therefore, we expect that this variable will have a negative relationship to the measures of democratization, and that horizontal power distribution does not directly affect a society's class structure.

Our reasoning for including government revenue as a percentage of gross domestic product in equation (3) is that government revenue is a measure of state strength, and the strength of the state is one of the most important causes of inter-country variations in class formation and inequality (Wallerstein, 1972). State strength has this effect because the state is one of the primary mechanisms for controlling the world-economy to the advantage and disadvantage of various economic groups. We expect that state strength only affects democratization through its effects on a society's class structure. A fully developed explanation for these assertions is presented in Robinson (1976). We expect that this variable will have negative effects on the Gini index and positive effects on the share of income accruing to the 3rd quintile. Government revenue has been coded from the International Financial Statistics Supplement (IMF, 1972).

The results of the instrumental variables estimation are presented in Tables 1 and 2 under columns 2 and 4. The OLS estimates of the same equations are presented for comparison in columns 1 and 3 of these tables. Column 2 of these tables shows the regression estimates of

¹² One way to state the statistical requirements for the instruments is to see if they are correlated with the regressor and have no statistically significant direct effect on the dependent variable when the other relevant variables are controlled (Namboodiri et al., 1975:552-5; Heise, 1975:160-5). In the sample using the Jackman index, the correlation of POWCON and Jackman is -.556, and the correlations between GOVREV and the Gini index and the 3rd quintile are -.297 and .436. In the sample using the Cutright index, the correlation of POWCON and Cutright is -.802 and the correlations between GOVREV and the Gini index and the 3rd quintile are -.387 and .523. We then ran eight OLS equations in which we regressed the two measures of inequality on the two measures of democratization, KWH, and POWCON, and in which we regressed the two measures of democratization on the two measures of inequality, KWH, and GOVREV. In seven of the eight equations, there was no significant effect of the instruments on the dependent variable. Only in the case of the equation with the 3rd quintile with Jackman was there such an effect. However, because of the consistency of the results in the eight sections on the IV analysis, this is not too problematic. Because of these tests we feel that POWCON and GOVREV are adequate instruments.

Table 1. Analysis Using Jackman's Index: Ordinary Least Squares (OLS) and Instrumental Variables (IV) Regression Estimates

Variables	(1)	(2)	(3)	(4)
	Gini Index		Jackman	
	OLS	IV	OLS	IV
POWCON	*	°	-10.145*	-11.07*
			(2.99)	(4.15)
KWH	.0007	.0004	.014	.005
	(.560)	(.279)	(1.04)	(.433)
GOVREV	-.003	-.006*	°	°
	(.776)	(1.80)		
Gini Index			-60.741*	-143.35*
			(2.90)	(2.34)
Jackman	-.042*	.017		
	(2.03)	(.450)		
R ²	.205	.117	.503	.706
N = 32				
	3rd Quintile		Jackman	
	OLS	IV	OLS	IV
POWCON	*	°	-10.817*	-11.84*
			(3.48)	(4.61)
KWH	.002	.007	-.0008	.018
	(.629)	(1.57)	(.060)	(.980)
GOVREV	.074	.220*	°	°
	(.729)	(2.01)		
3rd Quintile			2.30*	4.03*
			(3.99)	(2.62)
Jackman	.110*	-.082		
	(2.69)	(.703)		
R ²	.389	.112	.588	.765
N = 32				

* Variable is excluded from this equation.

° Variable is the dependent variable in this equation.

° Variable is used as the instrumental variable in this equation.

t-statistics in parentheses.

the effects of democratization in inequality, the regression estimates of the effects of inequality on democratization are shown in column 4. The comparisons among these coefficients represent the main focus of interest in these analyses. Before discussing these results, however, we briefly consider the other effects in these tables.

First, we see that the instrumental variables operate as expected. POWCON has significant negative effects in all equations. GOVREV has its expected negative effects on the Gini index and its expected positive effects on the 3rd quintile. These effects are statistically significant for the IV solution (in column 2) except for its effect on the 3rd quintile in Table 2. We also see that there is no statistically sig-

nificant effect of KWH on the Gini index or on the 3rd quintile. (Recall that there was also no effect of KWH in the previous analysis of equation [2].) While these results may seem surprising to some, they are not unique to this analysis. Robinson (1976), in a larger sample of countries, found that there was no significant effect of KWH on the Gini index when GOVREV was controlled. Jackman (1975:196) also found that there was no statistically significant effect of KWH on the Schutz index of *sectoral* income inequality when civilian government expenditures (a measure similar to GOVREV) was controlled. Thus, these effects in Tables 1 and 2 correspond to other sets of findings.

We now consider the effects between the indicators of democratization and in-

Table 2. Analysis Using Cutright's Index: Ordinary Least Squares (OLS) and Instrumental Variables (IV) Regression Estimates

Variables	(1)	(2)	(3)	(4)
	Gini Index		Cutright	
	OLS	IV	OLS	IV
POWCON	^a	^c	-5.237* (3.89)	-4.29* (3.51)
KWH	.0001 (.992)	.0002 (1.29)	.012* (2.41)	.011* (1.84)
GOVREV	-.005 (1.53)	-.005* (1.80)		
Gini Index			-11.64 (1.53)	-40.74* (2.14)
Cutright	-.007* (1.91)	-.006 (1.34)		
R ²	.220	.239	.727	.784
N = 32				
	3rd Quintile		Cutright	
	OLS	IV	OLS	IV
POWCON	^a	^c	-5.39* (4.19)	-5.03* (4.95)
KWH	-.0001 (.035)	.001 (.259)	.009* (1.80)	.004 (.666)
GOVREV	.183 (1.65)	.193 (1.75)		
3rd Quintile			.452* (2.06)	1.20* (2.27)
Cutright	.180 (1.63)	.125 (.739)		
R ²	.363	.345	.743	.784
N = 32				

^a Variable is excluded from this equation.^b Variable is the dependent variable in this equation.^c Variable is used as the instrumental variable in this equation.

t-statistics in parentheses.

equality. Inspecting the upper half of Table 2, column 1, we see that Jackman has an effect of .017 on the Gini index, with a nonsignificant t-statistic of .450. Comparing this effect with the estimate from the OLS model (-.042, with a significant t-statistic of 2.03), we see that IV estimation changes the sign of the coefficient from its hypothesized negative to positive and, also, changes the size of the coefficient from -.042 to .017. Column 4 shows that the effect of the Gini index on Jackman is -143.35, with a significant t-statistic of 2.34. Comparing this effect with the estimate from the OLS model (-60.741, with a significant t-statistic of 2.90), we see that the IV estimation has increased the size of this coefficient by 136%, while retaining its hypothesized

negative sign. Inspection of the standard errors for these variables (not reported in the table) reveals that the changes in statistical significance are not just a function of differences in the inflation of standard errors, which is often a consequence of the IV solution. The standard errors of Jackman and the Gini index both double in size. From this half of Table 1, then, we see that the instrumental variables estimation increases the size of the negative effects of the Gini index on Jackman and changes the sign of the effect of Jackman on the Gini index from the expected negative to positive.

Inspecting the bottom half of Table 1, we see the same pattern of results. In Column 2, the effect of Jackman on the 3rd quintile is -.082, with a nonsignificant

t-statistic of .703. While the IV estimation has changed the size of this coefficient from the OLS estimate (.110, with a significant t-statistic of 2.69), it is again the wrong sign for the hypothesis. In column 4, we see that the effect of the 3rd quintile on Jackman is 4.03, with a significant t-statistic of 2.62. The size of this coefficient is 75% greater than the OLS estimate, and it retains its hypothesized positive effect. Inspection of the standard errors also reveals that the changes in the statistical significance of these effects is not due to relative differences in the changes of the standard errors, for both standard errors increase about 2½ times. From the bottom half of Table 1, we see that the instrumental variable estimation has increased the size of the effect of the Gini index on Jackman, while altering the sign of the effect of Jackman on the 3rd quintile in a direction contrary to the hypothesis.

The estimation in Table 1, then, presents a consistent set of results. In each case, the effect of the measures of inequality on Jackman has increased and remained statistically significant, while the effects of Jackman on inequality have changed in sign contrary to the hypothesis and are no longer statistically significant.

Table 2 shows a similar pattern of results. Inspecting the upper half of this table, we see in column 2 that the effect of Cutright on the Gini index is -.006, with a nonsignificant t-statistic of 1.34. The size of this effect is only slightly reduced from the OLS estimate of -.007, with a significant t-statistic of 1.91. In column 4, however, we see that the effect of the Gini index on Cutright is -40.74, with a significant t-statistic of 2.14. The increase in the size of this effect is 250% over the OLS estimate of -11.64. Again, inspection of the standard errors reveals that the changes in the significance levels are not due to relative differences in changes in the standard errors, since the standard error for the Gini index increases even more than the standard error for Cutright. From this portion of Table 2, we see that the size of the effect of the Gini index on Cutright increases significantly and is also statistically significant, while the size of the effect of Cutright on the Gini index

decreases slightly and is not statistically significant.

The bottom half of Table 2 shows the same pattern. In column 2, we see that the effect of Cutright on the Gini index is .125, with a nonsignificant t-statistic of .739. This represents a reduction of about 30% in the size of this coefficient from the OLS estimate (.180, with a nonsignificant t-statistic of 1.63). In column 4, however, we see that the effect of the 3rd quintile on Cutright is 1.20, with a significant t-statistic of 2.27. This effect represents an increase of 165% in terms of the size of this coefficient relative to the OLS estimate of .452, with a significant t-statistic of 2.06. Again, the difference in significance levels cannot be attributed to relative changes in the standard errors of Cutright and the 3rd quintile, since the standard error of the 3rd quintile increases more than the standard error of Cutright. From this half of Table 2, we see that the instrumental variable estimation substantially increases the size of the effect of inequality on Cutright and somewhat decreases the size of the effect of Cutright on inequality.

In Tables 1 and 2, we find that in the instrumental variables model the effects of democratization on inequality decrease in size or are in the wrong direction and, in no case, are they statistically significant. The effects of inequality on democratization, however, always increase in size, are in the right direction and are, in each case, statistically significant. This same pattern of results has occurred for two different indicators of democratization and for two different indicators of inequality. These results lend more empirical support to the assertion that inequality has an effect on democratization, while giving less support to the assertion that democratization has an important effect on inequality.

In summary, we find that both hypothesized factors help to explain inter-country differences in the degree of democratization: (1) a class structure characterized by greater economic power of middle-income groups and (2) a governmental structure characterized by a more equal distribution of power across governmental branches. We also find that the strength of the state is an important determinant of inter-

country variations in class structure, or inequality; and while there is some effect of democratization on class structure, this effect is considerably reduced when these other variables are included in the model.

Conclusion

We feel it is important to emphasize the tentative nature of these findings. We realize that the sample size is not large (and small sample size can affect the consistency of IV estimates), nor is the sample random (although we have used the only data available). However, the aim of this paper has been replication and reanalysis. Whatever biases there may be in this analysis due to the sample or specification also are present in the empirical research that motivated this analysis. We believe that it is important to replicate first and then to proceed further. For it has become commonplace in the empirical cross-national literature to consider only that democratization affects inequality, and this assertion has been included within a great many models of political development. It would not be correct to conclude that there is no effect of democratization on inequality, since some of these effects (those in Table 2) were not reduced by major proportions in the instrumental variables analysis. Our aim here has been to raise the issue of the alternative specification and to present an analysis of the interrelationships between democratization and inequality.

The primary question with which this paper has been concerned is the relationship between democratization and inequality. In contrast to much of the empirical research which motivated our reanalysis, we make two points in conclusion. First, inequality within countries should be seen basically as a consequence of the class structure of countries. Second, it is this very process of class formation and class dynamics which should be seen as the basic determinant of democratization. In broader theoretical terms, we are raising the issue of the relationship between class structure and competition (and their interactions with the state) to the formation of political regimes.

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SOCIAL STATUS, STATUS INCONSISTENCY AND PSYCHOLOGICAL STRESS*

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A method for measuring status inconsistency is presented that permits a comprehensive test of inconsistency theory without the complicated statistical problems that have plagued previous research. An incremental modeling strategy is employed to test for the effects of social status, the effects of status inconsistency, and the effects of statistical interaction between vertical status and inconsistency. The analysis demonstrates the presence of status inconsistency effects. More importantly, however, the results confirm that the effects of status inconsistency are conditional upon location in the stratification hierarchy.

A substantial quantity of research has accumulated involving what Taylor (1973) has termed "theories of social consistency." Under this label, Taylor included certain formulations of social mobility (cf. Sorokin, 1927), status crystallization (Lenski, 1954) and status consistency (Jackson, 1962), the theory of status integration (Gibbs and Martin, 1964; Berry, 1966), and the idea of structural or com-

positional effects offered by Blau (1960) and Davis (Davis et al., 1961). To this list may be added various social exchange formulations such as distributive justice (Homans, 1961), social equity (Adams, 1965) and the theory of status value offered by Berger and his associates (Berger et al., 1972).

Several writers have pointed out formal as well as substantive similarities between the various social consistency theories. Along theoretical lines, Sampson (1963), Geschwender (1967), Mills (1975) and Taylor (1971) have noted that the theories of social consistency parallel the cognitive consistency theories of social psychology (e.g., Osgood's attitude congruity principle, Heider's structural balance principle, and Festinger's dissonance principle) in that both classes of theories predict that an inconsistency or incongruity between

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attributes (e.g., statuses or attitudes) constitutes a source of psychological stress for the individual involved and that this stress in turn leads to some form of adaptive or stress-reducing response. On the methodological side, Blalock (1967) has shown that theories of social mobility, status consistency, status integration and structural effects can be expressed in the same multiple regression format:

$$Y = \beta_1 X_1 + \beta_2 X_2 + \beta_3 (X_1, X_2) + e_y$$

wherein, X_1 and X_2 are statuses occupied by an individual at the same point in time in the case of status consistency and status integration; or the same status occupied at two different points in time in the case of social mobility; or an attitude measured at the individual and aggregate levels, respectively, in the case of structural effects. The third term in the equation denotes some difference (i.e., inconsistency, malintegration, etc.) between X_1 and X_2 that is presumed to affect the dependent variable net of the effects of the statuses or attitudes themselves. Taylor (1973), while demonstrating the utility of the general linear model for assessing and locating inadequacies in theories of consistency, has shown that Blalock's regression strategy is but a limited application requiring interval variables.

Theories of cognitive consistency have received considerable empirical support and occupy a prominent position in social psychology, whereas social consistency theories have not fared as well when subjected to empirical test. The confusing and sometimes contradictory results may be due to simplistic conceptualization and measurement of inconsistency coupled with analysis strategies that are insufficient for the complicated task of disentangling inconsistency effects from the effects of the statuses themselves.

This paper examines the relationship between status inconsistency and psychological stress and involves an examination of assumptions about the effects of inconsistency that have been unchallenged in previous research. This is accomplished by comparing the results obtained with a series of regression models. The most parsimonious of these models involves the linear main effects of education, occupa-

tion and income. More complex models are developed that incorporate terms for type of status inconsistency and then terms for statistical interaction effects between type of inconsistency and vertical status.

Theories of Social Consistency

Theories of social consistency maintain that statuses that are defined as inconsistent (malintegrated or inequitable) are psychologically stressful and that this "socially induced stress" (Martin, 1965:65) leads to an adaptive or stress-reducing behavioral or attitudinal response. Many of the ideas that find expression in the various formulations of social consistency are contained in Hughes' (1945:353-6) classic article "Dilemmas and Contradictions of Status" in which he noted that:

In the struggle for achievement, individual traits of the person stand out as separate entities. And they occur in peculiar combinations which make for confusion, contradictions, and dilemmas of status There tends to grow up about a status, in addition to its specifically determining traits, a complex of auxiliary characteristics which come to be expected of its incumbents If we think especially of occupational status, it is in the colleague-group or fellow-workers group that the expectations concerning appropriate auxiliary characteristics are worked most intricately into sentiment and conduct

According to Hughes, a "specific status" characteristic is the foundation from which expectations arise about the nature of an individual's "auxiliary status" characteristics. The auxiliary characteristics that come to be expected of occupants of a specific status are those auxiliary statuses that occur in frequent combination among individuals possessing the specific characteristic. Conversely, combinations of auxiliary status characteristics that are "peculiar" among incumbents of a specific status are defined as inconsistent and constitute a source of psychological stress for the individuals involved. Thus, for example, the female aerodynamic engineer and the black physician that Hughes talked about occu-

pied inconsistent statuses, because traditionally aerodynamic engineers were male and physicians were white. These inconsistencies were a source of stress for them and for others with whom they interacted.

The definition and measurement of inconsistency that underlies most previous research has not distinguished between specific and auxiliary status characteristics but has assumed, instead, that positions in different status dimensions each give rise to symmetric expectations about other status characteristics of the individual. However, as suggested by Hughes, expectations about relationships between specific and auxiliary status characteristics are asymmetric and involve anticipatory and normative components. Expected status relationships are anticipatory in that if Person believes Other occupies position i in the specific status dimension S (call it $S^{(i)}$), then Person anticipates Other to occupy positions j and k in auxiliary status dimensions A_1 and A_2 (call them $A_1^{(j)}$ and $A_2^{(k)}$).¹ Expectations are normative in that both Person and Other come to believe that $A_1^{(j)}$ and $A_2^{(k)}$ *should* be occupied by individuals possessing $S^{(i)}$. Furthermore, the expected relationships between specific and auxiliary status characteristics are asymmetric in that $A_1^{(j)}$ and $A_2^{(k)}$ are expected of individuals occupying $S^{(i)}$, but a particular category or level of a specific status dimension is not necessarily expected of individuals on the basis of their positions in A_1 and/or A_2 . (For a more detailed discussion of asymmetric status expectations, see Berger et al., 1972:129-31.) Thus, while aerodynamic engineers are expected to be male and physicians are expected to be white, males and whites are not necessarily expected to be engineers or physicians.

Following Hughes, occupational status, which is a salient element of the self-concept (Mulford and Salisbury, 1964) and the single best indicator of social position (Blau and Duncan, 1967), is a specific status characteristic for which combinations of auxiliary statuses such as educa-

tion and income come to be defined as consistent or inconsistent (i.e., "natural" or "peculiar"). On the basis of occupation, individuals are expected (in many instances, legally required) to have attained a certain level of education and to maintain a certain style of life. In this respect, individuals in the same or a similar occupation constitute an equity group (Kemper, 1968) or referential structure which determines the investments the individual legitimately can be expected to make and the rewards he or she legitimately can expect to receive. Levels of investments such as educational attainment or rewards such as income that are higher or lower than expected for a specific occupational status violate this distribution rule and result in psychological stress such as dissatisfaction with current status positions (Hyman, 1967; Hornung, 1972).

Data

The data for this analysis were collected as part of the Princeton Fertility Study (Westoff et al., 1961). Of the 1,265 couples in the first panel of the study, 941 husbands completed questionnaires that included a number of measures of satisfaction with current job and financial situation.

Measures of Psychological Stress

Occupation stress (OS) is conceptualized as a perception of a lack of intrinsic rewards in the occupational role. No pretense is made concerning the exact nature or relative importance of the various components of occupation dissatisfaction which include wages, amount and type of supervision, degree of autonomy and self-expression, physical conditions, hours, etc. (Kalleberg, 1977). Further, no reference to the internalization of the work ethos is intended. Rather, the reference for occupation stress is essentially noneconomic in quality and pertains to the individual's preference for engaging in activities other than his present occupational role.

Financial stress (FS) is conceptualized as the degree to which the individual per-

¹ In the case of nominal status attributes "i," "j," and "k" denote categories, while in the case of hierarchical status dimensions "i," "j" and "k" denote ranks.

ceives his present income as being insufficient to afford the style of life he feels he deserves.

Measures of these stress variables were obtained from a factor analysis of thirteen dichotomous items contained in the husband's questionnaire. Factor scales for occupation stress and financial stress were computed and adjusted to have means of 2.50 and standard deviations of 1.25.

Development of Regression Models

Additive main effects model (Model 1.1). The first regression model involves the additive linear main effects of education, occupation and income status and is written as:

$$Y = \beta_1 E + \beta_2 O + \beta_3 I + e_y \quad (1.1)$$

where E, O and I denote education, measured as the number of years of formal schooling completed, occupation, and income measured as the total salaries and wages received by the individual in the year preceding the date of the interview.² The ability of the linear main effects of status to account for levels of occupation and financial stress experienced by individuals is one criterion for judging the merits of more complex models involving measures of status inconsistency.

The working hypotheses that occupation stress and financial stress are inversely related to education, occupation and income status will be refined as regression models involving status inconsistency variables are developed.

Type of status inconsistency (Models 2.1 and 2.2). Types of status inconsistency are defined in terms of combinations of

education and income that are expected for different occupations. The procedure for measuring inconsistency employed in this analysis was introduced by Goffman (1957) and has at least three important advantages over measurement strategies that have dominated status inconsistency research. First, the procedure used here does not rest on an assumption of a strong or even a moderately strong linear correlation between status hierarchies. The linearity assumption which underlies commonly used measures of inconsistency (cf. Lenski, 1954; House and Harkins, 1975) introduces error into the measurement of inconsistency to the extent that departures from linear relationships between status dimensions are structured in the status expectations held by individuals. Second, the procedure used here takes into account the asymmetric nature of the status expectation process whereas previous measures have assumed the process to be symmetric. Third, nominal status characteristics such as sex or marital status can be used, whereas previous measures have been restricted to status dimensions that can be rank-ordered.

Although the procedure is used here with occupation as the specific status and education and income as auxiliary statuses, alternative analyses may be undertaken in which, for example, race, ethnicity, sex or education is defined as the specific status and occupational prestige, income or other status characteristics of individuals are conceptualized as auxiliary statuses. Further, the measurement procedure is easily generalized for use with four or more status characteristics (see Ploch, 1968, for an alternative methodology).

To determine types of status inconsistency, education and income are cross-tabulated within each of the census categories of occupation. Levels of education and income between the 30th and 70th percentiles of their respective distribution within categories of occupation are defined as "consistent" for that occupation, while levels of education and income below the 30th percentile are "inconsistent-low" and values above the 70th percentile are "inconsistent-high."

² It should be noted that the measure of income status pertains to a different point in time (i.e., the year preceding the date of interview) than the measures of education and occupation status (i.e., status at time of interview). This temporal difference is unlikely to introduce "differential" measurement error into the main effects of the status variables, given that the differences apply across all cases. However, distortions of indeterminant magnitude may be introduced in the inconsistency effects, given that mobility between the two points in time to which the status measures apply will mean that individuals are scored "consistent" while at time of interview they are "inconsistent" and vice versa.

		Income		
		Low	Consistent	High
Education	High	Education-High Income-Low Type 1	Education-High Income-Consistent Type 2	Education-High Income-High or Occupation-Low Type 3
	Consistent	Education-Consistent Income-Low Type 4	STATUS CONSISTENT Type 5	Education-Consistent Income-High Type 6
	Low	Education-Low Income-Low or Occupation-High Type 7	Education-Low Income-Consistent Type 8	Education-Low Income-High Type 9
		30th Percentile		70th Percentile
		30th Percentile		70th Percentile

Figure 1. Type of Inconsistency

This yields nine combinations for each occupation.³ Eight of these combinations are mutually exclusive types of status inconsistency (see Figure 1). The intersection of consistent education and consistent in-

come defines the status consistent type (type 5).

Model (2.1) isolates the effects of type of inconsistency controlling for education, occupation and income status:

$$Y = aU + b_1E + b_2O + b_3I + \sum_{i=1}^{k-1} c_i T^{(i)} + e_y \quad (2.1)$$

where $T^{(i)}$ are binary coded terms denoting the eight types of inconsistency. A model for determining the effects of type of inconsistency ignoring the main effects of the status variables is:

$$Y = aU + \sum_{i=1}^{k-1} c_i T^{(i)} + e_y \quad (2.2)$$

In both of these models, the vector for the status-consistent type is deleted to eliminate exact linear dependence. Consequently, the coefficients c_i measure the increase or decrease in Y associated with

³ An examination of the education and income distributions among professionals revealed that nearly 25 percent were in the two highest income categories and over 43 percent were in the highest category of education. In view of the skewness of these distributions, inconsistent high categories of income and education were not defined for professionals. There is some theoretical basis for this. It is questionable whether individuals in the highest occupation could be considered by others to have attained too much education or too high an income. In contrast, education and income low values were defined for the lowest occupation meaning that even laborers are expected to have attained some formal education and earn a minimal income.

The nine categories of type of inconsistency formed in this manner are exhaustive and mutually exclusive. This avoids important logical and statistical problems which have been encountered in previous inconsistency research using two factor comparisons for types of inconsistency (cf. Berry and Martin, 1972).

the i^{th} type of status inconsistency relative to the mean level of stress among status consistents (for alternatives to a +1,0 coding scheme, see Melichar, 1965; Cohen, 1968; Lyons, 1971; MacDonald, 1973; Kerlinger and Pedhazur, 1973; Wolf and Cartwright, 1974).

Equity theory provides a convenient rationale for predicting certain types of status inconsistency to be more psychologically stressful than others. Stress is predicted to be greatest in conditions of *inequity-disadvantage*, which exists when the rewards received by an individual are less than he or she could legitimately expect. Lower stress is expected in conditions of *inequity-advantage*, when rewards received are greater than can be legitimately expected. The lowest stress (i.e., an absence of stress) is expected in a condition where the rewards received are equitable for the investments and costs incurred (Cook, 1975).

Considering years of education as an investment and occupation as a reward, types 1, 2 and 3 of inconsistency are conditions of inequity-disadvantage; types 7, 8 and 9 are conditions of inequity-advantage; and types 4, 5 and 6 are conditions of equity. Considering the occupational role as a cost and income as a reward, inconsistency types 1, 4 and 7 are conditions of inequity-disadvantage; types 3, 6 and 9 are conditions of inequity-advantage; and types 2, 5 and 8 are conditions of equity. Further, the equity or inequity between educational investments and income rewards can be derived from the equity relationships between education and occupation and between occupation and income. In this way, inconsistency types 3, 5 and 7 are derived to be conditions of education-income equity; types 1, 2 and 4 are derived conditions of inequity-disadvantage; and types 6, 8 and 9 are derived conditions of inequity-advantage.

The combinations of these equity-inequity conditions generate predictions about the rank-order effects of types of inconsistency. Specifically, in the regression of occupation stress on types of inconsistency, the following relationship between coefficients is predicted:

$$c_1 > c_2 = c_4 > c_3 = c_7 > c_6 = c_8 > c_9 > 0.$$

The regression of financial stress on types of inconsistency is predicted to show the following:

$$c_1 > c_4 > c_2 = c_7 > c_3 = c_8 > c_6 > c_9 > 0.$$

Note that all types of status inconsistency are predicted to be more stressful than consistency (i.e., all c_i coefficients are predicted to be positive); that type 1 inconsistency is predicted to lead to the highest occupation and financial stress; that education-low, income-high (type 9) is predicted to be the least stressful type of inconsistency; and finally, that inconsistency types 2 through 8 are predicted to have different consequences for occupation and financial stress.

Vertical status and type of inconsistency (Models 3.1, 3.2 and 3.3). The third set of models in this analysis explores the possibility of statistical interaction between type of inconsistency and location in the vertical stratification hierarchy. Models (2.1) and (2.2) assume, as has previous inconsistency research, that the effects of inconsistency are uniform at all points in the vertical hierarchy. In other words, the effects of education-low, income-high have been assumed to be the same for clerical workers as for skilled trades workers. Although there is some evidence that the effect of inconsistency is conditional upon vertical status (cf. Goffman, 1957; Bauman 1968; Hornung and McCullough, 1977), there has been no systematic attempt to locate specific interaction effects. If interaction effects are documented, then the assumption of uniform effects of inconsistency breaks down and a potential source of the confusing and sometimes contradictory results of previous inconsistency research (cf. Jackson and Curtis, 1972) has been identified.

Model (3.1) yields the total effects of vertical status (measured as occupation) and type of inconsistency:

$$Y = aU + \sum_{i=1}^{k-1} \sum_{j=1}^{m-1} b_{ij} 0^{(i)} T^{(j)} + e_y \quad (3.1)$$

where $0^{(i)}$ and $T^{(j)}$ are binary-coded occupation and type of inconsistency variables, respectively. It can be shown (Taylor, 1973:1210) that this equation is

identical to a subclass equation written as:

$$Y = \sum_{i=1}^k b_i C^{(i)} + e_y$$

where $C^{(i)}$ are unique combinations of occupation and type of inconsistency.

Model (3.1) and the subclass equation are "total effects" equations in the sense that they yield the maximum explained variation in Y that can be achieved with vertical status and type of inconsistency. However, neither of these equations partition explained variation into that due to the additive effects of occupation and type of inconsistency and that due to their statistical interaction. To accomplish this partitioning and to isolate the proportion of variation in stress explained by the conditional relationship between type of inconsistency and vertical status, Model (3.1) is compared to an equation that assumes the effects of vertical status and inconsistency to be additive. Model (2.1) is not suitable as an additive effects baseline because it includes terms for the effects of education and income in addition to those of occupation and type of inconsistency. An equation suitable for our purposes is written as:

$$Y = aU + b_1 O + \sum_{j=1}^{m-1} c_j T^{(j)} + e_y \quad (3.2)$$

where the effects of vertical status are assumed to be linear. Because of the restrictiveness of this assumption, Model (3.2) may produce inflated estimates of interaction effects when compared to Model (3.1). A more conservative estimate of the interaction sums of squares is obtained by comparing Model (3.1) with an equation that relaxes the linearity assumption, thereby enabling vertical status to explain a larger proportion of the variation in Y . Such a model is written as:

$$Y = aU + \sum_{i=1}^{k-1} b_i O^{(i)} + \sum_{j=1}^{m-1} c_j T^{(j)} + e_y \quad (3.3)$$

If Model (3.1) explains significantly more variation in stress than the conservative Model (3.3), then there is strong evidence of interaction effects between type of status inconsistency and location in the stratification hierarchy.

Type of Inconsistency and Stress

The results are summarized in several tables beginning with Table 1 which presents the unstandardized regression coefficients and proportion variation explained for the first two sets of models. Model (1.1) assumes additivity and linearity and shows that years of education, occupation, and total salary and wages are, as predicted, inversely related to both occupation and financial stress. While each of the main effects is significantly related to occupation stress with the single best predictor being years of education, only occupation and income are significantly related to financial stress. The linearity and additivity assumptions restrict the main effects to explaining a modest 12.9 and 11.2 percent of the variation in occupation stress and financial stress, respectively. These results are one criterion for assessing the utility of less parsimonious models which incorporate terms for status inconsistency.

There is some debate over what constitutes an appropriate test of consistency theory (cf. Hope, 1975). There are those who maintain that theories of consistency have scientific value if measures of inconsistency are capable of explaining variation in the dependent variable left unexplained by the additive effects of the statuses (cf. Olsen and Tully, 1972). The appropriate test of status consistency according to this position is a comparison of the proportions of variation explained by Model (2.1) and Model (1.1). However, it also can be argued that theories of consistency are scientifically useful if they yield accurate predictions of the effects of inconsistency without reference to the main effects of the statuses. In this case, the appropriate test of status consistency is a comparison of the predicted and observed coefficients for the effects of type of inconsistency obtained with Model (2.2).

Model (2.1) finds type of status inconsistency adding only 2.1 percent to the explanation of occupation stress and even less to the explanation of financial stress. The poor explanatory power of type of inconsistency also occurs in Model (2.2) which ignores the main effects and explains less than 2 percent in occupation

Table 1. Unstandardized Regression Coefficients Models 1.1, 2.1 and 2.2

Model	Main Effects						Inconsistency Effects									
	Multiple R ²	Y	aU	E	O	I (x10 ⁻³)	T ⁽¹⁾	T ⁽²⁾	T ⁽³⁾	T ⁽⁴⁾	T ⁽⁵⁾	T ⁽⁶⁾	T ⁽⁷⁾	T ⁽⁸⁾	T ⁽⁹⁾	T ⁽¹⁰⁾
(1.1)	.129 ^a	OS	4.28	-.078 ^a	-.088 ^a	-.040 ^a	.215	.239 ^a	.394 ^a	.113	.014	-.089	-.714 ^a	-.475 ^b	-.332	-.408 ^a
	.112 ^a	FS	3.62	-.022	-.041 ^b	-.100 ^a	.352 ^a	.128	.356 ^a	aU	aU	.106	.075	-.119		
(2.1)	.150 ^a	OS	5.24	-.194 ^a	.010	-.040 ^a	.311 ^a	.419 ^b	.184	.349 ^b	aU	.285 ^c	.279 ^d	.297 ^c	.513 ^c	
	.126 ^a	FS	3.80	-.062 ^a	-.018	-.070 ^a	.645 ^b	.329 ^c	-.082	.707 ^a	aU	.029	.640 ^a	.210 ^d	-.158	
(2.2)	.016 ^d	OS	2.27													
	.057 ^a	FS	2.26													

^a p < .000.^b p < .05.^c p < .01.^d p < .10.

stress but 5.7 percent in financial stress. Accordingly, in terms of the first criterion for assessing the theory, compared to the explanatory power of the main effects, type of status inconsistency does not appear to have much scientific value. However, Model (2.2) does yield results that provide support for the theory, at least in terms of the second criterion.

Table 2 shows that the mean stress levels for each type of inconsistency conform quite closely to their predicted rank-order. Seven of the eight types of inconsistent individuals report significantly higher occupation stress and five report significantly higher financial stress than status-consistents. Occupation stress is high for types 9, 2 and 4, moderate for types 1, 8 and 6, low for types 7 and 3, and lowest among status-consistents (type 5). Financial stress is high for types 4, 1 and 7, moderate for types 2 and 8, and about equally low for types 6, 5, 3 and 9. A finding that does not conform to the predictions is that education-low, income-high inconsistency (type 9), which was predicted to be the least stressful of the inconsistency types, has the lowest level of financial stress but the highest occupation stress.

Types of inconsistency that involve inequity-disadvantage between education and income (types 1, 2 and 4) have high means on both occupation stress and financial stress. The high financial stress reported by these types of inconsistent is a function of the low financial returns for the level of investments made. Kalleberg's (1977:138) analysis of job satisfaction would suggest that the observed high occupation stress results from highly educated workers being overtrained for their jobs and therefore not deriving intrinsic satisfactions from their occupational role. Our results generally support Kalleberg's argument but suggest further that the relationship between educational investments and occupation stress is conditional upon the level of financial rewards received. Specifically, *over*-education leads to occupation stress only when the financial returns are less than equitable for the investments made. Type 3 inconsistent report low financial stress and low occupation stress suggesting that financial re-

Table 2. Stress by Type of Status Inconsistency

Type	N	Occupation Stress		Financial Stress	
		Mean	Rank	Mean	Rank
1. Education-High, Income-Low	39	2.58*	6	2.90*	8
2. Education-High, Income-Consistent	102	2.69*	8	2.59*	6
3. Education-High, Income-High	70	2.45	2	2.18	2
4. Education-Consistent, Income-Low	145	2.62*	7	2.97*	9
5. Status Consistent	253	2.27	1	2.26	3
6. Education-Consistent, Income-High	117	2.55*	4	2.29	4
7. Education-Low, Income-Low	80	2.54*	3	2.90*	7
8. Education-Low, Income-Consistent	102	2.56*	5	2.47*	5
9. Education-Low, Income-High	30	2.79*	9	2.10	1
		$\bar{Y}_o=2.50$ $s=1.25$		$\bar{Y}_f=2.50$ $s=1.25$	

* Significantly different from the mean for status consistent.

turns that are above what the individual can legitimately expect on the basis of his or her occupation reduce the stress produced by overtraining if the income received is equitable for the higher than expected education. In this respect, equity between education and income apparently mollifies the stresses of inequity-disadvantage between education and occupation and inequity-advantage between occupation and income. In contrast, *under*-education combined with higher than expected income (i.e., inequity-advantage as in type 9) leads to high occupation stress. This probably results from undertrained individuals working harder at their jobs and thereby deriving less satisfaction from the occupational role per se and from being resented by their co-workers for having an income above a level defined as just (Homans, 1961).

This analysis confirms the presence of status inconsistency effects and demonstrates that types of inconsistency cannot be assumed to have the same consequences for all measures of stress or, for that matter, all attitudes and behaviors. However, the theoretical importance of these findings cannot be assessed without considering the results obtained with the third set of regression models which isolate the effects of types of inconsistency at different points in the stratification hierarchy.

Model (3.1) accounts for 24.7 and 17.3 percent of the variation in occupation and financial stress, respectively. More impor-

tant, assuming linear effects of vertical status finds 14.4 and 8.2 percent of the variation accounted for by the interaction of type of inconsistency and vertical status. When the assumption of linear vertical status effects is relaxed, 6.2 percent of the variation in the occupation stress measure and 5.4 percent in financial stress remain attributable to statistical interaction between vertical status and inconsistency. These results are strong evidence that the effects of types of status inconsistency are conditional upon the individual's position in the occupation hierarchy.

Table 4 presents the mean occupation and financial stress by type of status inconsistency and vertical status. It must be mentioned that the frequencies in many of the cells are small and therefore the results must be interpreted with caution because of the sensitivity of the mean to sampling error.

Table 4 reveals that the effects of status consistency as well as inconsistency are conditional upon vertical status. No combination of education and income status is found to have the same consequences at all points in the stratification hierarchy. Instead, the level of both occupation and financial stress is a function of vertical status and type of status inconsistency.

The means in each column of Table 4 are graphed in Figure 2 with the X axis divided into four segments corresponding to the semi-permeable boundaries between lower blue-collar, upper blue-collar, lower white-collar and upper

Table 3. Vertical Status and Type of Inconsistency: Additive and Interaction Effects (Models 3.1, 3.2, 3.3)

Model	Source	Occupational Stress		Est. of Variance	F Ratio	p	% Variation Explained
		Sums of Squares	df				
3.2	Total	1463.97	937				
	Linear Vertical Status and Type	150.40	9	16.71	11.81	.001	10.3
3.3	Nonlinear Vertical Status and Type	271.03	15	18.07	13.96	.001	18.5
3.3-3.2	Explained by Nonlinearity	120.63	6	20.11	15.59	.001	8.2
	Nonlinear Error	1192.94	922	1.29			
3.1	Subclass	361.48	64	5.65	4.48	.001	24.7
3.1-3.2	Interaction (Linear)	211.08	49	4.31	3.42	.001	14.4
3.1-3.3	Interaction (Nonlinear)	90.45	43	2.10	1.67	.01	6.2
	Subclass Error	1102.49	873	1.26			
Financial Stress							
3.2	Total	1463.54	937				
	Linear Vertical Status and Type	132.88	9	14.76	10.29	.001	9.1
3.3	Nonlinear Vertical Status and Type	174.33	15	11.62	8.31	.001	11.9
3.3-3.2	Explained by Nonlinearity	41.45	6	6.91	4.97	.001	2.8
	Nonlinear Error	1289.82	922	1.39			
3.1	Subclass	252.72	64	3.95	2.84	.001	17.3
3.1-3.2	Interaction (Linear)	119.84	49	2.45	1.76	.001	8.2
3.1-3.3	Interaction (Nonlinear)	78.39	43	1.82	1.31	.05	5.4
	Subclass Error	1210.82	873	1.39			

white-collar *classes*. Each class is, in turn, divided into a lower and upper *stratum*.

If the consequences of type of inconsistency were independent of vertical status, then the mean stress levels could be connected by straight lines parallel to the X axis (i.e., $b_1 = b_j = \dots = b_m = 0$, where i is type of status inconsistency) and the effects of types of inconsistency would be indicated by different Y intercepts ($a_1 \neq a_j \neq \dots \neq a_m$). If there were effects of vertical status but no effects of inconsistency, the slopes would be equal and non-zero and the Y intercepts would be equal. Additive effects of vertical status and type of inconsistency would produce equal non-zero slopes and different Y intercepts, while unequal slopes and different intercepts would indicate additive and interaction effects between vertical status and inconsistency. The interaction effects of inconsistency and stratum are indicated

by line segments with non-zero slopes, while interaction between inconsistency and class are indicated by line segments of differing (average) heights above the X axis or significantly different slopes.

The lines plotting occupation stress and those for financial stress covary in a manner that would indicate that the psychological consequences of status consistency and inconsistency are conditional upon vertical status and that the results are not simply an artifact of small Ns. This is substantiated by the pattern of findings for status-consistents in which there are a sufficient number of cases in each occupation to be reasonably confident in the stability of the means of the stress measures.

The pattern of results suggests that if we think in general terms about education as an investment of human capital that is a requisite for an occupational role and of income as an expected perquisite, then the

Table 4. (Continued)

Vertical Status	Types of Status Inconsistency									Row Means and Standard Deviations
	1	2	3	4	5	6	7	8	9	
Semi-Skilled	2.62	3.21	3.60	2.99	3.10	2.79	2.40	3.45	3.56	3.12
	2.76	2.84	2.55	3.18	2.89	3.18	3.24	2.89	1.93	1.15
(11)	(48)	(13)	(13)	(13)	(30)	(9)	(12)	(21)	(4)	2.88
										1.18
(161)										
Service		1.34	1.70	2.24	3.17	2.27	3.20	2.38	1.47	2.42
	(0)	1.03	1.38	2.70	2.81	3.27	3.36	2.17	2.90	1.03
		(2)	(3)	(4)	(6)	(10)	(5)	(2)	(2)	2.75
										1.21
(34)										
Laborers	2.71	2.83	3.27	3.30	2.44	2.96	1.27	2.93	3.18	2.95
	1.74	2.68	3.28	2.89	2.84	2.90	3.57	3.31	3.15	1.07
	(2)	(3)	(6)	(5)	(4)	(5)	(1)	(2)	(3)	2.94
										1.21
(31)										
Column Means and Standard Deviations	2.58	2.69	2.45	2.62	2.27	2.55	2.54	2.56	2.79	2.50
	1.18	1.28	1.30	1.32	1.28	1.17	1.19	1.20	1.05	1.25
	2.90	2.59	2.18	2.97	2.26	2.29	2.90	2.47	2.10	2.50
	1.16	1.26	1.32	1.15	1.23	1.21	1.23	1.19	1.28	1.25
	(39)	(102)	(70)	(145)	(253)	(117)	(80)	(102)	(30)	
(938)										

* Occupational stress in upper left corner and financial stress in lower right corner of each cell.

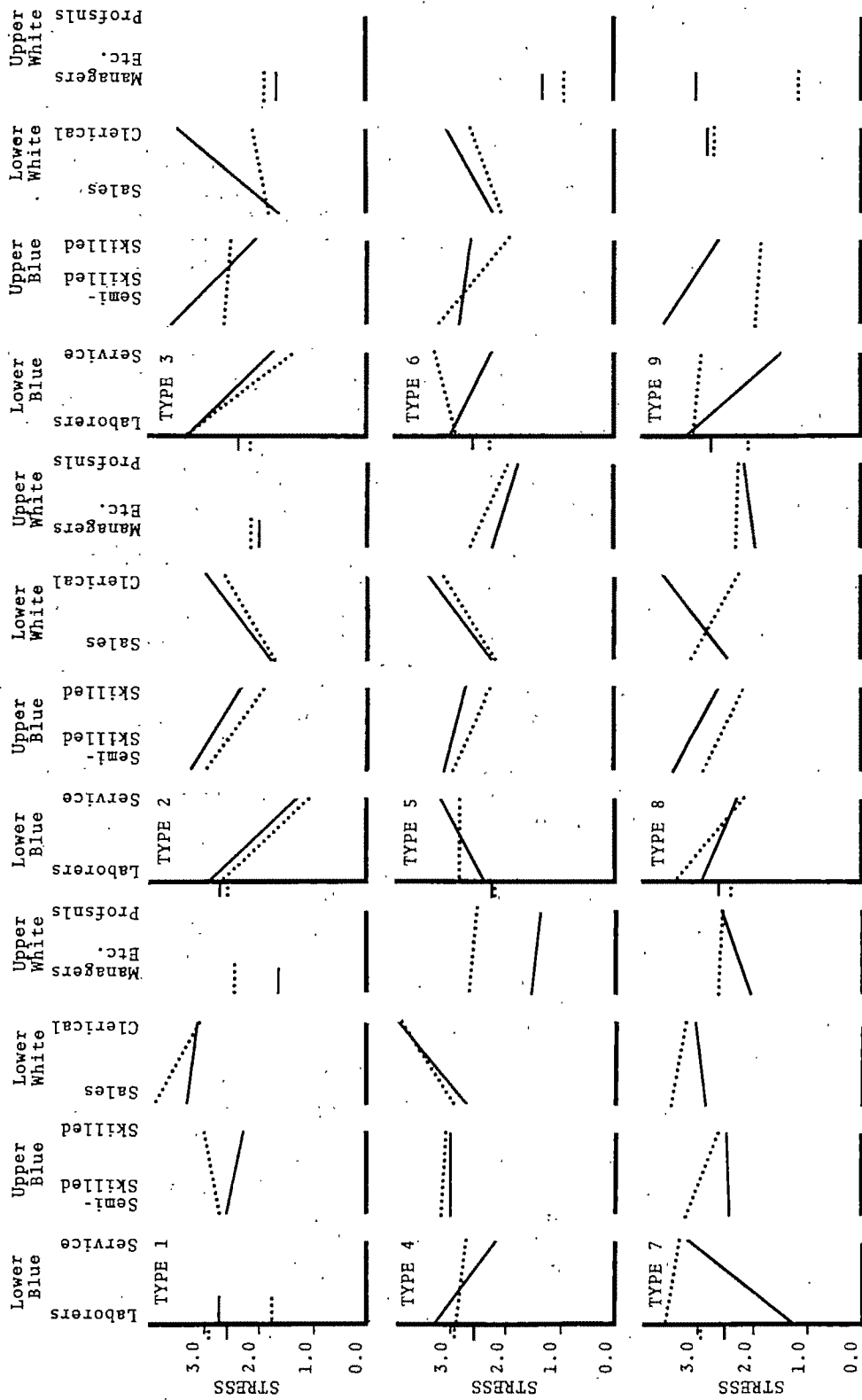


Figure 2. Stress by Type of Inconsistency by Vertical Status Financial Stress

psychological consequences of inconsistency (i.e., inequity) are conditioned by position in a vertical hierarchy of occupations. The observation that status consistency is not always associated with low stress is particularly significant since the cornerstone of previous research has been the assumption that status consistency was not stressful regardless of vertical status. On the contrary, status-consistents with jobs in the top stratum of the lower blue-collar (i.e., service workers) and lower white-collar (i.e., clerical) classes as well as status-consistents with jobs located in the bottom stratum of the upper blue-collar (i.e., semi-skilled) and upper white-collar (i.e., managers, etc.) classes report *both* high occupation stress and high financial stress. It is noteworthy that status-consistents in these strata experience greater stress than many status-inconsistent. One explanation for this is that status consistency is stressful when consistency across status dimensions means that future mobility is unlikely. For example, clerical workers who have attained the expected level of education and earn the expected income for their occupation are likely to lack the financial resources and security to enter the ranks of the self-employed or the educational attainment to become professionals. In this respect, status-consistents in these strata may come to regard their occupations as mobility traps offering little opportunity to improve their financial situations or to move into other lines of work.

The graphs for the eight types of status inconsistency provide some indication of the psychological consequences of status inconsistency at different points in the hierarchy of occupations. Both occupation stress and financial stress are functions of class position and relative amount of income received among individuals with more than the expected level of education for their occupation. When income rewards are relatively low (type 1), occupation stress decreases within each class; but when income rewards are consistent (type 2) or higher than expected (type 3), occupation stress decreases within the blue-collar classes but increases within the lower white-collar class. Among these overtrained individuals, financial stress

increases within the upper blue-collar class and decreases within the lower white-collar class when the level of income rewards received is less than expected. However, this pattern reverses when income is consistent or higher than expected so that financial stress decreases within the blue-collar classes and increases within the white-collar class. In contrast, the results for types 7, 8 and 9 which involve inconsistent-low education show that financial stress decreases within each class, independent of whether the income rewards received are lower than expected (type 7), consistent (type 8) or higher than expected (type 9) for the occupation. In other words, the level of financial stress experienced by undertrained individuals is a function of whether they are in a high or low stratum job within an occupational class. However, occupation stress experienced by undertrained individuals is a function of both position within a class and the relative amount of income rewards received. Individuals in upper stratum jobs with less than the expected level of education experience high stress unless they receive the expected or higher than expected income rewards. Jobs in the upper stratum of a class place more emphasis on training and education than lower stratum jobs in the same class and, as a result, undertrained individuals experience high stress in upper stratum jobs when their income rewards do not testify to their ability to satisfactorily perform the occupational role (i.e., when income rewards are not consistent or higher than expected).

Conclusions

The discontinuity of the lines in Figure 2 suggests that there are divisions in the hierarchy of occupations that affect the psychological and, perhaps, other consequences of status consistency and inconsistency. This implies that these divisions are meaningful for the way individuals perceive and evaluate the equity of their educational investments and income rewards. Distribution rules that define the level of investments and rewards that individuals can legitimately be expected to make and receive appear to be based on

position within an occupational class. Moreover, position within a class affects the kinds of stress experienced by individuals in different conditions of inequity. Individuals in low stratum jobs within classes generally react similarly to each type of status inconsistency while individuals in high stratum jobs also react similarly, but the level of occupation stress and financial stress they experience differs from that experienced by those in lower stratum jobs.

The theory and measurement of status consistency presented here was found to predict accurately higher levels of stress among status-inconsistent than among individuals occupying status combinations that were defined as consistent. This finding by itself, however, is not sufficient to claim that social consistency theories in general, or status consistency theories in particular, are as sound as cognitive consistency theories. However, the finding that type of status inconsistency interacts with vertical status indicates that the assumption of additive effects of status and status inconsistency is misleading and is a potential source of the confusing findings reported in previous research. In the future, adequate tests of social consistency theories will require the use of analysis strategies that partition explained variation into that accounted for by the main effects, the effects of inconsistency and higher-order statistical interactions between the main effect variables and measures of inconsistency.

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DISTRIBUTIVE JUSTICE AND EARNED INCOME*

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This is an empirical exploration of the normative criteria for just distributions of earned income. The results indicate that the fairness of earnings is judged relative to a set of criteria which includes (but perhaps is not limited to) formal educational attainment, occupational attainment, sex, marital status and knowledge of family earnings. That is, both merit and need factors combine to produce judgments of fairness and unfairness. These criteria for just earnings appear to be held consensually and are largely independent of the raters' observable demographic attributes.

Introduction

Empirical research on distributive justice has focused largely on the consequences for subjects of observing and/or participating in situations defined by an experimenter as "unjust." This large and impressive body of literature (whose most recent review appears in Adams and Freedman, 1976) has focused primarily on the proper specification of the determinants of "inequity effects" (thought to be anger in the under-reward case, guilt in the over-reward case) and certain not-yet-well-understood redressing behaviors. The favorite research design involves observing the reactions of subjects whose expectations, furnished by the investigator, are violated. For example: Walster and Austin (1974) measured the contentment and distress of subjects who, led to expect "normal \$2 pay" for performing a proofreading task, were paid varying amounts; McCranie and Kimberly (1973) constructed a referential standard whose "three ranks were always described as consistent (i.e., all high) so that the Ss

would have a highly stable referent with which to compare ego"; and Cook (1975:382) supplied subjects with the implicit distribution rule that "pay should be allocated on the basis of ability."

The research designs described above as archetypical bypass the critical questions of the justice problem: (1) Is there a general standard (or standards) that guides judgments concerning distributive justice issues in such crucial areas as earnings? (2) How much consensus is there concerning such standards of justice? Such research designs also ignore the basic question about the etiology of the standards of justice, the major puzzle being whether they are derived from actual distributions of resources—an existential referent—as Homans (1974; 1976) has suggested, or whether they are plucked from some primordial image of "what ought to be"—a utopian referent—as Berger et al. (1972) have suggested.

Laboratory experiments undoubtedly have pursued the right tactic in attempting to fix experimentally the normative framework into which subjects are to be placed. The existence of consensually held standards of justice is so critical to the experiments that it would be damaging to assume that they are brought to the experimental setting by subjects. Yet from the viewpoint of attempting to assess the importance to real-world behavior of distributive justice processes, the existence and prevalence of such frameworks must be taken as problematic.

Nor is this question merely one of con-

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cern to the social scientist interested in real-world applications of the concepts and theories of distributive justice. The two major theoretical formulations decidedly regard as problematic the content of the justice rules. The Berger et al. (1972) model defines a situation as unjust if a real-world actor's level of a goal-object differs from the level of the goal-object received by his/her counterpart in a "referential structure." Note that this formulation is amenable to both the utopian and the existential conceptions of the sources of justice. The Homans (1974) model defines justice as equality of actors' profits/investments ratios. Both models are essentially multivariate and both recognize the multiplicity of dimensions deemed relevant to the justice evaluation of shares of social resources. Both require a common metric such that the value or "worth" of levels of characteristics or inputs and levels of outcomes can be established unambiguously (cf. Homans, 1976:232).

However, the research tradition has avoided the question of the just association between levels of actors' attributes and levels of outcomes, apparently preferring to relegate it to the givens of the theory. Thus, in a well-formalized recent piece, Cook (1975:374) consigns to the scope conditions of a theory of the emergence of justice concerns in social systems: (1) the existence of a distribution rule that "specifies how the dimensions of evaluation in a social system (e.g., skill level, seniority, etc.) are related to the allocation of valued outcomes" and (2) the perception of the legitimacy of the distribution rule, that is, of "the extent to which the distribution rule is supported normatively in the social system." Cook (1975:373) writes, "It would be interesting, of course, to address questions concerning the legitimacy of the distribution rules (e.g., that workers with more seniority should receive higher salaries)." Current empirical studies in distributive justice thus expend their major efforts in attempts to predict reactions to various forms of inequity, the latter being defined within the context of the research setting.

But one investigator's assumption is another's research question. Convinced

as we are that soup should come before salad—that legitimate distribution rules¹ should be endogenous to an empirical theory of distributive justice—we undertook to animate the Berger et al. (1972) referential-structure "persons," to quicken the terms in the Homans ratios, and to do so quantitatively.

But suppose that selection of referential standards, relevant inputs or distribution rules is idiosyncratic, suppose that the world is as Walster et al. (1976:4–5) observe:

Ultimately, equity is in the eye of the beholder. An individual's perception of how equitable a relationship is will depend on *his* assessment of the value and relevance of the various participant's input and outcomes If participants do calculate inputs and outcomes differently—and it is likely that they will—it is inevitable that participants will differ in their perceptions of whether or not a given relationship is equitable. Moreover, "objective" outside observers are likely to evaluate the equitableness of a relationship quite differently than do participants.

We suggest that if referential standards turn out to be idiosyncratic, then an empirical theory of distributive justice is but a drollery. Deprived of agreement on what is just and unjust, our models would be unable to predict anything social (a point implied by Homans [1976:232] and well understood by Cook, as the scope conditions show).

Prediction is not the sole impetus behind our search for consensual referential standards. These referential standards or distribution rules are part of what Rawls (1971:4–5) had in mind in his normative theory of justice:

A society is well-ordered when it is not only designed to advance the good of its members but when it is also effectively regulated by a public conception of justice. That is, it is a

¹ We consider that a distribution rule may be a set of guidelines, such as a triage policy, or that it may be an empirically-derived function, such as earnings returns to specific characteristics. In any case, these distribution rules describe "what is." Their legitimacy refers to "what ought to be." Manipulation of these two variables may lead to alternative research designs. For example, subjects could be asked to evaluate alternative distribution rules or they could be asked to devise such a rule.

society in which (1) everyone accepts and knows that the others accept the same principles of justice, and (2) the basic social institutions generally satisfy and are known to satisfy these principles.

We consider important a research program aimed at documenting empirically the standards thought just for the distribution of the social primary goods, chief among them the Weberian hierarchies of wealth, power and prestige, but also the sub-hierarchies, such as educational attainment. Empirical work of the past ten years has made notable advances in documenting the *actual* distributions of earnings, educational attainment, occupational attainment and occupational prestige. A parallel assessment of *just* distributions would permit preliminary answers to the theoretical question of the sources of justice and to the practical question of everyday discourse, "What is fair?"

In the empirical inquiry reported here, we restrict attention to the earnings distribution. Earnings are of overwhelming importance because, for most persons, they are the sole source of monetary income, which is critical to so many aspects of life in an industrial state. Moreover, money as a variable possesses that (blessed) intrinsic quantifiability.

There are many possibilities for the just distribution of earnings. One is equality, that is, zero variance, with equal shares for all workers. A second possibility combines dispersion with random criteria for allocation. In this case, the distribution of earnings thought just may have a given variance, but allocation of differently-sized shares would be on a random basis (e.g., by lottery). A third possibility combines dispersion with systematic criteria for allocation of the differently-sized earnings shares. These criteria could be any of a large number of personal or family characteristics, such as age, sex, native ability, effort, need or even seemingly capricious criteria such as number of lines in the right palm.²

² Choice of a just distribution of earnings may be considered as a branching problem. First, a decision is made on the proposition that all shares ought to be equal. If that decision is negative, then there are two additional decisions to be made, one on the amount

This third possibility may be regarded as rational selection of a just distribution out of all the possible unequal distributions. The criteria chosen reflect alternative conceptions of just bases or claims for earnings allocation. For example, earnings may be viewed as payment for the performance of a job, in which case the criteria would be work-related; or earnings may be viewed as support for a family, in which case certain indicators of need would be salient criteria.

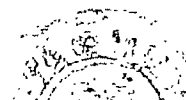
There are many reasons to believe *a priori* that our society represents a particular manifestation of the third form of justified inequality. Common wisdom stresses that there should be some sort of fit between a person's qualifications and performance and his/her earnings. All the tell-tale traces of distribution rules suggest achievement through personal qualities as the basic justification for income differentials with some modifications to take into account the varied income needs of persons and households under certain conditions (e.g., young children, disabling illnesses). The details of the underlying structure of justice rules are not as obvious, however. Whether educational attainment, for example, overrides the justification for income that may be generated by responsibilities for dependents is not at all clear.

The general aims of the research reported below are to uncover some of the features of the normative structure underlying distributive justice judgments concerning earnings. We take as problematic, in the first place, the existence of such a structure and, in the second place, the specific provisions within such an existing structure.

Method

Our research strategy is to elicit from a reasonable sample of respondents a set of judgments about the justice of the earnings enjoyed by a variety of fictitious individuals and families. These judgments are

of dispersion to be allowed and the other on whether the criteria for allocation ought to be random or systematic. These possibilities suggest a variety of research designs, thus making possible the reliability testing of procedures.



obtained in household interviews by presenting to the respondents a set of descriptions of individuals and families (called vignettes). The set of vignettes presented to each respondent has certain important features: the distribution of earnings across such vignettes is rectangular, which maximizes the earnings variance. Earnings are random with respect to all other characteristics, thus assuring a test of the relative importance of the various justice rules. Finally, the overall patterning of judgments about such a set of vignettes allows inferences concerning collectively preferred earnings distributions.

The respondent sample. Block quota sampling methods were used to choose a respondent sample of 200 white adults residing in private households in Baltimore City. Sex quotas were fixed to ensure that equal numbers of adult females and males were interviewed. Although block quota sampling methods have known biases (cf. Kish, 1965), their considerably lower costs offset the greater precision afforded by more rigorous area probability sampling methods for studies in which population estimates are not critical.³ As will be shown below, the fact that there are few subgroup differences within our sample on crucial findings leads us to have some confidence in the external validity of this particular sampling strategy.

The vignette sample. Each fictitious person was described in terms of a combination of characteristics, as follows:⁴

Sex

Marital status—Single or married.

Number of children—Zero to six, in increments of one.

Education— Years of formal education, ranging from completion of

seventh grade to college graduation, in increments of one year.

Occupation—Ninety-six occupational titles whose prestige scores, standardized by Bose (1973) for the Baltimore area, ranged from 8.0 to 95.8, plus the designation "Unemployed and looking for a job" and the sex-restricted title "Housewife."

Earnings— Fifteen levels of annual earnings ranging from \$2,000 to \$40,000; thus, a married couple may have combined earnings ranging from \$2,000 to \$80,000.⁵

Half the vignettes described one adult person designated either married or single but providing no information about the spouse, if married. These vignettes mirror some real-world situations, either of single persons living alone or of married persons about whose spouses one has no knowledge. The other half of the vignettes describe married couples, with all information given for both husband and wife. The vignettes were generated by computer and printed on IBM cards, the program randomly selecting characteristics from within each list for each person described in a vignette. Thus, for a one-person-single vignette, sex was randomly selected, then occupation, an educational level and finally an earnings amount were selected. Restrictions to eliminate nonsensical combinations (e.g., a physician with an eighth-grade education) were imposed *a priori*. To eliminate the possibility of zero earnings on a vignette, the Unemployed and Housewife designations were restricted to one such title per married couple.

The vignettes thus generated constitute an almost completely crossed experimental design. Obviously, the characteristics of vignettes do not have the same intercorrelations as one would find among concrete individuals or families. Indeed, this is one of the virtues of the vignette

³ Comparisons with census (U.S. Bureau of the Census, 1970) estimates for whites in Baltimore City show that our sample is similar to the parent population in mean years of school completed (12.3), but somewhat younger (43.8 versus 48.4 years) and more married (67% versus 56%), a result perhaps of our setting sex quotas at parity. The respondents' tract median incomes ranged from \$5,442 to \$16,685.

⁴ We decided to restrict vignette characteristics to a very small set because each additional factor would considerably lessen the efficiency of the vignette samples. However, race, ethnicity, length of employment and other variables excluded from this study will be added to future studies.

⁵ The levels of individual earnings are in increments of \$1,000 between \$2,000 and \$8,000, of \$2,000 between \$8,000 and \$14,000, of \$4,000 between \$14,000 and \$22,000, and of \$6,000 between \$22,000 and \$40,000.

technique. Because correlations are much lower than obtained in the real world, respondents are forced to make discriminations that involve taking into account disparate attainments and characteristics of individuals and families.

Ten samples of sixty vignettes each were drawn, for a total of 600 distinctly different vignettes.⁶ The vignette samples were drawn so that each contained five vignettes describing single males, five describing single females, ten each of vignettes describing married males and married females (with no information provided about a spouse) and thirty vignettes describing married couples. The ten vignette samples were systematically distributed with equal probability across respondents, each sample being given to twenty respondents, for a possible total of 12,000 judgments. To preclude order effects, each vignette set was well-shuffled.

Interviews lasting about half an hour were conducted by experienced professional interviewers.⁷ Each respondent was handed a set of vignettes and a box with nine slots, each labeled with a number and a description corresponding to a level of the justice evaluation variable, ranging from "-4 Extremely Underpaid," through "0 Fairly Paid," to "+4 Extremely Overpaid." The task given to the respondent was to place each vignette in the slot that corresponded to the respondent's justice evaluation of each vignette.⁸ The interviewers reported

⁶ Each of the ten vignette samples may be regarded as a random sample of a population composed of all the permissible combinations of the personal characteristics. Since each person in each one-person vignette could take on one of two sexes, one of two marital statuses, one of 99 occupational titles, one of ten educational levels and have from zero to six children (if married), the population of possible vignettes for the one-person vignettes was very large. Of course, the married-couple vignettes have a much larger population of possible combinations. Because the vignette samples were drawn at random, each sample has the same statistical properties as the parent population and varies from it only within the limits of sampling error. Hence, each sample is a replication of an incompletely crossed factorial design and has most of the desirable statistical properties of such designs.

⁷ Sidney Hollander and Associates, a well-known Baltimore survey firm, collected the interviews in early 1974.

⁸ The actual wording of the item was as follows: "People and their jobs differ in a lot of ways. We

that the respondents generally approached the rating task with seriousness, a view borne out by the number of ratings made, a total of 11,806, or over 98% of the possible ratings.

Special measurement considerations. While most of the variables are measured and coded conventionally,⁹ the fairness rating deserves special comment. The nine-category fairness-rating scale represents an attempt to break into the underlying continuum of perceived relative unfairness with a minimum of bias and a maximum of field utility. The scale works very well in the personal household interview situation. Its major weakness is that the discrete category values reduce the fineness that respondents may want to make in their justice discriminations, forcing them to stay within a set range and to round off their judgments. We assume that rounding errors are unbiased, i.e., have a mean of zero. The scale's major strength is that the combined use of verbal and numerical labels may evoke a sense of intervalness. Our hope that we have attained a better-than-ordinal scale hinges on those labels (and on the large number of ratings).

have made up descriptions of different kinds of people and jobs. Here is a box with nine different slots in it. Here is a card with a description of a person and job.

- a. Please put the card in the slot labeled Fairly Paid if you think that the salary is about right for the person, that is, if you think that the salary on the card is fair payment.
- b. Put the card in the slot labeled Extremely Overpaid if you think that the money is way too much payment for these people, that is, if, taking everything into account, you think that they're very much overpaid.
- c. Put the card in the slot labeled Extremely Underpaid if you think that the money is way too little payment for these people, that is, if, taking everything into account, you think that they're very much underpaid.
- d. If you think that the salary is a bit too much or a bit too little, that is, if you think that it belongs somewhere between the most overpaid and the most underpaid, then just put the card in the slot that you think matches the fairness or unfairness of the salary.

⁹ The quantitative variables were coded with their raw values, except for earnings, which was coded in thousands of dollars. Occupation was coded with the prestige scores standardized for the Baltimore area by Bose (1973). Sex and marital status entered the regressions as dummy variables, with the categories "single" and "male" coded zero.

Table 1. Distributions of Income-Fairness Ratings by Type of Vignette

Fairness Rating	Type of Vignette					
	Single Female	Single Male	Married Female	Married Male	Married Couples	All Vignettes
Extremely Underpaid (-4)	14%	18%	14%	19%	7%	12%
Much Underpaid (-3)	8	12	8	12	9	9
Somewhat Underpaid (-2)	7	10	10	12	10	10
Slightly Underpaid (-1)	11	10	11	11	11	11
Fairly Paid (0)	26	19	26	21	24	23
Slightly Overpaid (+1)	10	8	9	8	10	10
Somewhat Overpaid (+2)	7	8	9	8	10	9
Much Overpaid (+3)	7	7	6	5	10	8
Extremely Overpaid (+4)	10	8	6	5	9	8
100% =	[996]	[998]	[1991]	[1995]	[5826]	[11806]
Average Rating =	-.20	-.61	-.42	-.88	+.10	-.23

Our research design, then, assumes that persons, in fact, make judgments about the fairness or relative unfairness of resource allocation. Happily, this is a short-lived assumption, since data collection soon confirms it. We further assume that the individual's justice conceptual space is a continuum with a mid-point of fairness and two extreme poles of unfairness.¹⁰ Since the zero point represents fairness, we regard the fairness measure as a continuous scale of degrees of unfair payment ranging from underpayment through a midpoint of fair payment to extreme overpayment.

Results

The distribution of ratings received by the approximately 12,000 vignettes rated

by our respondents is shown in Table 1. If respondents were simply endorsing an extreme *laissez-faire* outlook, we would expect that the ratings would pile up in the "Fairly Paid" category expressing approval of the entire range of earnings levels for persons and couples of widely varying characteristics. This outcome is certainly not the case in Table 1. As shown in the last column, about 23% of the ratings are in the "Fairly Paid" category, but the remainder are spread quite widely across the other ratings. Thus, it is clear that the ratings were not randomly distributed among vignettes.

Table 1 may be regarded as a harbinger of tendencies that will emerge illuminated from the regression analyses. For example, the one-person vignettes, which had lower absolute earnings than married couple vignettes, exhibit a strong minor mode at ratings of -4 (Extremely Underpaid) and lower mean ratings, indicating the importance of earnings amount per se. Further, the minor mode is stronger and the mean rating lower for males than for

¹⁰ Following Aristotle (1952: Books II and V), we consider justice to be a mean, a state intermediate to two kinds of injustice—one of excess and the other of deficiency—that are contrary both to each other and to the mean and that are susceptible of degree variation.

Table 2. Regressions of Income Fairness Ratings on Vignette Characteristics: One-Person Vignettes

A. Correlation Matrix, Means and Standard Deviations of All One-Person Vignettes:

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fairness Rating (1)		-.089	-.042	-.079	-.093	-.131	.721	.619
Sex (F=1, M=0) (2)			.000	-.041	.032	.057	.008	.000
Marital Status (3)				.643	.059	.014	.049	.042
Number of Children (4)					.066	.058	-.010	-.036
Education (Years) (5)						.295	.083	.104
Occupational Prestige (6)							.092	.082
Income (Thousands) (7)								.964
Income Squared (8)								
Mean=	-.562	.500	.666	1.982	12.05	45.89	13.31	299.2
Standard Deviation=	2.398	.500	.472	2.181	3.01	21.65	11.05	437.5

B. Regression Analyses:

	Unstandardized Regression Coefficients		
	(1)—All	(2)—Single	(3)—Married
Sex	.3177	.4544	.2229
Marital Status	-.1743		
Number of Children	-.0735		-.0682
Education (Years)	-.0538	-.0691	-.0433
Occupational Prestige	-.0197	-.0175	-.0202
Income (Thousands)	.3903	.4531	.3689
Income Squared	-.0060	-.0076	-.0055
Intercept	-1.9928	-2.6351	-2.4278
R ² =	.6616	.6873	.6513
N=	5980	1994	3986

All statistics significant beyond the .001 level.

females, regardless of marital status, suggesting that the sex credit is not related to breadwinning.

The structure of judgments about the fairness of earnings. The gross distributions of ratings shown in Table 1 reflect the activity of a number of justice rules. In order to uncover the structure of the justice rules that guided our respondents, we resort to multiple regression as an analytic tool, regressing the ratings on the characteristics displayed in the vignette descriptions. This approach, of course, assumes that a linear, additive relationship exists, which we will test later.

Tables 2 and 3 report the results of these regressions separately for the one-person and the married-couple vignettes. Note that the unit of analysis is the vignette, not the respondent; there are nearly 12,000 vignettes, each of which has been rated by a respondent.

The top panels of Tables 2 and 3 contain the zero-order correlations among the variables used in the regressions reported

in the bottom panels. With the exception of those in the top rows, the correlations are of little interest since they are artifacts of the design of the study, in which vignette characteristics were almost randomly associated with each other. Thus, these correlations are consistently low, a property which enhances the stability of the regression coefficients.

The unstandardized regression coefficients may be regarded as measures of the extent to which each of the vignette characteristics affect the fairness ratings achieved by a vignette. Thus, the coefficient for education (-.0538), in the case of all the one-person vignettes, has a direct interpretation, as follows: for each additional year of schooling attributed to a vignette person, the fairness rating moves .05 of a unit toward the unfairly underpaid end of the rating scale.

A positive coefficient may be interpreted as indicating an increment in the direction of overpayment and a negative coefficient as indicating the direction of

underpayment. Thus, depending on the activity of the whole equation, an increment may equally well indicate decreasing underpayment, a leap to fairness or increasing overpayment. Moreover, the sign of the coefficient also affects the meaning imputed to its magnitude. The larger a positive coefficient, the greater the "penalty," but the larger a negative coefficient the greater the "credit." Thus, as we shall see when we compare raw weights for husband's and wife's characteristics, the larger of a pair of negative coefficients has substantive implications in the same direction as the smaller of a pair of positive coefficients.¹¹

For a variety of reasons, including the claims of economists that the utility of income varies at the margins, we expected to find a nonlinear effect of earnings. Similar reasons led us to entertain the possibility of nonlinearity in the other vignette characteristics. The possibility of nonlinearity in all the independent variables was tested by entering quadratic terms for all the variables into a polynomial regression. Inclusion of second-order terms for all variables except earnings produces increases in the R^2 s ranging from zero to .003, clearly negligible.¹² Quadratic terms for the earnings variables, however, as expected produce increases in the R^2 s that range from .0234 to .0826.

A further search for specific nonlinearities was performed, based on the notion that the income squared term might be masking interactions between earnings and each of the other vignette characteristics. Substantively, such product terms would indicate "separate" evaluations of earnings relative to each attribute. New models were estimated, for all the one-person vignettes together as well as disaggregated by marital status and for all the married-couple vignettes, in which the

fairness rating was regressed on a set of independent variables including all first-order characteristics and all two-way interactions involving earnings. The entire set of product terms increased the resultant R^2 s (from the purely linear models) from .6004 to .6028 in the one-person-single case, from .5720 to .5836 in the one-person-married case, from .5790 to .5887 for all the one-person vignettes together, and from .4758 to .4799 for the married-couples vignettes.

Clearly, the net effects of the product terms are negligible. Such models fail to account for the seven or eight percentage points explained by the income-squared terms (cf. Tables 2 and 3). Moreover, the improvement of one percentage point in the one-person-married case is far outweighed by the costs in interpretation. The product terms are considerably well correlated both with their own constituent terms and with each other (since they share the earnings term).

Thus, we favor a simple additive model of the fairness rating with a single nonlinearity in the earnings term. This model suggests that the basic justice evaluation process implicitly involves three analytic steps: first, the worth of the two earnings terms is calculated; second, the amounts of just remuneration to a set of claims are calculated and summed; third, the sum of the claims is subtracted from the earnings effects to produce the justice evaluation. If the remainder is zero, the individual is judged fairly paid. If the remainder is a positive number, then the individual is judged unfairly overpaid. And, finally, if the remainder is a negative number, then the individual is judged underpaid.

In other words, the best and simplest of these models consistent with the data specifies a straightforward comparison between what an individual of such-and-such characteristics ought to earn and what he/she in fact does earn. This is precisely the model of justice evaluations suggested by Berger et al. (1972).

As Tables 2 and 3 indicate, the vignette characteristics (all of whose coefficients are statistically significant) account for a considerable portion of the variance in the income fairness ratings, ranging from .49 to almost .69. Since the fairness of earn-

¹¹ This activity of the regression coefficients parallels in statistical terms Aristotle's (1952: Book V) observation that "more of the good and less of the evil are gain, and the contrary is loss."

¹² Because of the high degree of freedom associated with the large number of ratings, even these minuscule increases in R^2 produce highly significant F-tests, suggesting rejection of the hypotheses that inclusion of quadratic terms adds nothing to the prediction. However, we consider increases of magnitude less than .01 to be substantively trivial.

ings is being judged, variation in earnings can be expected to consume most of the variation in the fairness ratings. The high earnings effect indicates that, net of everything else, the higher a person's earnings the more likely he/she is to be perceived as overpaid. High earnings per se lead to overpayment judgments, regardless of the income source. The negative coefficients for the earnings quadratic terms indicate decreasing increments in judgments of overpayment, the larger the earned income involved.¹³

The respondents clearly accepted the characteristics we used to describe the fictitious persons as just criteria in earnings allocation. Increments in education, occupation and number of children, as well as maleness and the married state, move judgments toward the underpaid side of the fairness rating scale, suggesting that the justice of earnings is judged not only relative to education and occupation but also to factors extraneous to the occupational sphere.

Looking now at the disaggregated one-person vignettes,¹⁴ the increment in the fairness rating due to the effects of the two earnings terms (first- and second-order) is slightly greater for single than for married persons. The penalty for being female is much higher for singles than for marrieds. Males are viewed as justified in earning more than females, but marriage, and specifically the possibility that the earner supports a household alone, considerably temper this differential.

Turning now to the married-couple vignettes (Table 3), the results indicate that the husband's education and occupation

buy more underpayment than the wife's. When husband's and wife's earnings enter the regression separately, the increment in the fairness rating due to the two earnings terms (first- and second-order) is larger for the husband than for the wife; thus, his earnings contribute a bit more than hers to a judgment in the direction of overpayment. This constitutes the only benefit to women we discern in these data. And, strictly speaking, it is not a benefit "to" women, but rather "through" women to their households.¹⁵

A further search for interactions in the married-couple vignettes revealed one important effect. The product term for the husband's and wife's earnings is highly significant and improves R^2 over two percentage points, or as much as the two earnings-squared terms (cf. Table 3, models [2] and [3]). The coefficient of $-.0029$ indicates a greater justification of the same combined earnings amount when both spouses are employed. For example, the family in which the two spouses each earn \$10,000 has an extra credit of $-.29$ on the fairness rating scale, a credit not granted to the family in which one spouse earns \$20,000 and the other is unemployed. Remember that this effect is net of the effects of absolute level of earnings.

Moreover, this interaction indicates approval of equality of earnings between spouses. For the product term varies inversely with the size of the gap between the factors and is maximized when the two factors are equal. For example, while the spouses who each earn \$10,000 would enjoy a credit of $-.29$ on the fairness rating scale, the family in which one spouse earns \$15,000 and the other earns \$5,000 would have a credit of $-.22$.

Note that the interaction between the two earnings suggests only justification of higher earnings when both spouses are employed and still higher earnings when their earnings are equal. It tells us nothing

¹³ The second-order polynomial is a reasonable, albeit imperfect, approximation of the effect of earnings on the fairness rating. For example, in the married-couple vignettes, the highest mean fairness rating, 2.949 occurs at the combined-earnings value of \$58,000, whereupon the mean fairness rating dips to 1.718 at \$68,000 (mean fairness ratings close to 1.7 also occur at values of 39, 40, 45 and 52 thousand dollars), then recovers to 2.525 at the final combined-earnings value of \$74,000. This activity is represented in our equation by a parabola that peaks at \$64,000 and descends to a combined-earnings effect at \$74,000 which is close to that at \$52,000.

¹⁴ The difference in R^2 due to separate b weights is only .0057 (but statistically significant, as indicated in footnote 12); examination of the differences in the slopes is our main interest here.

¹⁵ As Quevedo y Villegas (1963), the great poet of the Spanish baroque, observed in *Letrilla satirica*: "Poderoso caballero es don dinero," money engenders almost sacred respect. Its presence doesn't ever escape our notice. A family may not get increased prestige for having a wife-mother physician, but it certainly gets judged overpaid if the wife-mother has sizable earnings.

about whether, if there is a gap, it is more tolerable when one spouse rather than the other has the higher earnings.

The Question of Consensus

Although the analyses presented so far have indicated that the linear model is a good fit to the fairness ratings, the R^2 s do not exceed .69, suggesting that there is still some individual variation, some of which may be regarded as "error." Now we consider that consensus exists in a population when there are no significant amounts of structured disagreement among socially recognizable subgroups of the population in question.

In order to search for such structured disagreement, we entered respondent characteristics into the analyses. It should be recalled that since each respondent had an equal chance of receiving each of the vignette samples, the respondent characteristics correlate essentially zero with any of the vignette characteristics. Hence, whatever respondent characteristics affect the ratings do so independently of the analyses presented earlier.

The results of these regressions indicate that the respondent characteristics account for remarkably little of the variance in the fairness ratings. The R^2 values, while statistically significant, are substantively tiny. In the case of the one-person vignettes, all the respondent characteristics together account for only .0045 of the variation in the ratings and, in the case of the two-person vignettes, only .0077. This is strong indication that the determinants of income fairness evaluations are not rooted to any great extent in raters' demographic characteristics.¹⁶

To test the possibility that respondent characteristics interacted with vignette characteristics, we computed all regressions with interaction terms added to the basic set of terms. In no case did the increase in R^2 due to the entire set of interaction terms reach a magnitude of .01.

To explore further a possible effect of sex of respondent on the fairness ratings,

¹⁶ Recall that the ratings were produced by 200 respondents. Hence, the regression results are inflated by intra-individual correlations. So the tiny respondent effects themselves are inflated.

we correlated the mean ratings given by males with the mean ratings given by females for each of the six-hundred distinctly different vignettes. This procedure yielded a correlation coefficient of .9393, highly significant, indicating that the relative orderings given to vignettes by males and by females are virtually the same.

Thus, we conclude that the judgments made by our respondents were guided by consensually held justice rules. The variance left unexplained in Tables 2 and 3 may be attributable to "error" or to characteristics of the respondents on which we have no measures.

Estimating Fair Earnings

We now use the regressions reported in Tables 2 and 3 to estimate the earnings amounts for persons and married couples of varying characteristics that would be most likely to be judged "Fairly Paid." Table 4 reports a number of such estimates for single persons, married persons about whose spouse nothing is known, and married couples. The figures are for individual earnings in the first two cases (the first four columns) and for combined earnings of the married couples.¹⁷

The lowest estimate, \$7,125, is for single females with seven years of education and jobs such as "chambermaid." The highest estimate, \$34,593, is for the combined earnings of a married couple, both college graduates and with jobs such as "lawyer." We suspect that the former estimate exceeds the typical wage for such persons and that the latter figure is less than actual typical earnings for such married couples. These extreme estimates suggest a tighter just distribution of earnings than exists in the empirical world.

Variation within rows reflects the sex and marital status variables. Variation within columns reflects the education and occupation variables. It is clear that

¹⁷ Estimates of fair earnings are obtained by solving according to the quadratic formula the regressions (2) and (3) of Table 2 and (5) of Table 3. The solution sets for each equation contain two real numbers, only one of which lies in the domain of the earnings terms used in this research ($2 \leq x \leq 40$ in the one-person case and $2 \leq x \leq 80$ in the two-person case). It is this one root out of each solution set that constitutes a valid estimate of fair earnings (cf. Kerlinger and Pedhazur, 1973:213-4).

Table 4. Estimated Fair Earnings for Individuals and Married Couples of Varying Personal Characteristics

Education (Years)	Occ. Prestige	Single Male	Single Female	Married Male	Married Female	Married Couple
7	10	\$8,473.70	\$7,125.00	\$9,217.00	\$8,397.50	\$13,187.50
8	10	8,710.50	7,315.80	9,379.40	8,554.60	13,562.50
8	20	9,250.00	7,835.50	10,151.90	9,300.80	15,281.30
8	30	9,815.80	8,375.00	10,950.80	10,070.80	16,937.50
10	20	9,697.40	8,250.00	10,491.00	9,627.90	16,093.80
10	30	10,276.30	8,802.60	11,302.10	10,408.70	17,906.30
10	40	10,868.40	9,355.30	12,144.00	11,216.90	19,781.30
12	30	10,743.40	9,236.80	11,659.10	10,751.70	18,875.00
12	40	11,355.30	9,802.60	12,515.20	11,572.40	20,687.50
12	50	11,986.80	10,388.20	13,407.80	12,425.00	22,687.50
12	60	12,644.70	10,986.80	14,341.80	13,313.60	24,812.50
14	40	11,855.30	10,263.20	12,893.20	11,933.80	21,750.00
14	50	12,506.60	10,861.80	13,802.80	12,801.30	23,812.50
14	60	13,184.20	11,473.70	14,756.40	13,706.70	25,843.80
14	70	13,901.30	12,111.80	15,761.10	14,655.50	28,125.00
16	50	13,039.50	11,342.10	14,205.80	13,184.50	24,812.50
16	60	13,736.80	11,973.70	15,180.40	14,107.80	27,031.30
16	70	14,480.30	12,631.60	16,209.70	15,077.20	29,281.30
16	80	15,250.00	13,328.90	17,304.10	16,100.30	31,812.50
16	90	16,059.20	14,039.50	18,447.80	17,187.40	34,593.80

See footnote 17.

intra-column differences are greater than intra-row differences, suggesting the larger effects of occupationally relevant factors.

The estimates for fair combined earnings are lower than the sum of the earnings of two comparable single persons at the lower attainment levels, but larger at the higher attainment levels. This pattern suggests special credits for low-earnings singles who have to maintain a household alone and also, as noted earlier, for high-earning, both-employed married couples.

The one-person-married condition maximizes earnings across the full spectrum of educational and occupational attainment. We interpret this as a presumed-need differential.

We regard Table 4 as a summary of the region of justice in earnings allocation. Entries in the top row are minimum amounts, and entries in the bottom row are maximum amounts. Differentials between minimum earnings and maximum earnings reflect activity of justly-perceived need and merit factors.

Summary and Discussion

This research shows that the fairness of earnings is judged relative to a set of criteria which includes (but may not be

limited to) education, occupation, sex, marital status and knowledge of family earnings. These criteria appear to be held consensually and are largely independent of the raters' observable demographic attributes.

This means that an equal distribution of earnings would be considered unjust. The justly-perceived dependence of just earnings on earner characteristics suggests a certain inappropriateness of methods for assessing income inequality that have absolute equality as the ideal to be approximated (e.g., Gini's coefficient of concentration and the variance). On the other hand, we certainly found approval for a very restricted variance in the earnings distribution, but this variance must be structured and not random.

One of the more fascinating findings of this research is approval not only of two-earner families, but of a money credit to spouses whose earnings are equal. This suggests acceptance of marital equality as an ideal, and also suggests recognition that married individuals whose spouses are employed work harder and have less leisure than married individuals with unemployed spouses. We think it is time that the tax structure adjusts to the new realities.

There are many questions for future re-

search, but a most important one is to assess the consequences for social systems and for their members of actual earnings distributions that exhibit varying degrees of deviation from the consensually-held principles of justice in earnings allocation.

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COMMENTS

PROOF? NO. EVIDENCE? NO. A SKEPTIC'S COMMENT ON INVERARITY'S USE OF STATISTICAL INFERENCE*

(COMMENT ON INVERARITY,
ASR APRIL, 1976)

Inverarity's (1976) selective use of significance tests in his recent study of populism and lynching in Louisiana would seem to require far more justification than provided.¹ In his cross-sectional analysis by parish, goodness-of-fit chi-square tests are applied to entire causal models, but no attempt is made to assess the importance of stochastic processes for individual coefficients. This pattern is all the more curious since significance tests are applied to individual coefficients in the OLS model reported on page 276. Moreover, important substantive conclusions are drawn from the time series shown in Figure 1 with no apparent concern for the alternative explanation of chance. Consequently, even if, as Inverarity argues, the Winch and Campbell (1969) article justifies using goodness-of-fit tests, it is still unclear how it supports Inverarity's decision to consider stochastic processes in some contexts and not others. Therefore, the purpose of this brief comment is twofold: to provide some groundwork for Inverarity's response and to call attention to special issues regarding statistical inference for historical

* Beneath this necessarily brief comment lie a variety of difficult substantive and mathematical issues. To the degree that I have been able to correctly articulate their relevance for historical data, much of the credit belongs to Marilyn Brewer from UCSB's Psychology Department, Thomas Cooley from UCSB's Economics Department, George Bohrstedt from Indiana University's Department of Sociology and an anonymous reader. In addition, much of the background reading for this comment was accomplished over the past year in which my research was, in part, supported by a grant from the Russell Sage Foundation to study methodology for Evaluation Research. Finally, Sarah Fenstermaker Berk deserves credit for the irreverent parts of the comment's title.

¹ Throughout this comment, I will assume that Inverarity means to use a "Classical" approach to statistical inference based on a frequency interpretation of probability. There is certainly no evidence that a Bayesian view was intended: no discussion of prior probability distributions, likelihoods, degrees of belief, and so on (Box and Tiao, 1973).

data (cf. for example, Snyder and Tilly, 1972; Feierabend and Feierabend, 1966; Gurr, 1968).

In an unusually lucid discussion of inferential reasoning, the mathematician Polya (1954:12) characterizes "generalization" as "passing from the consideration of a given set of objects to that of a larger set, containing the given set." This process may be properly undertaken through a wide variety of analytical devices in which chance, as "the ever present rival conjecture," is but one of the many hazards to overcome. In this context, Edgington (1966:485) distinguishes between "statistical" and "non-statistical" inference; the former approached through estimation or hypothesis testing, the latter through "logical" considerations.² My point, therefore, is that Inverarity's selective use of statistical inference per se is not the issue; inferences often may rest on theoretical arguments. However, in the absence of some persuasive rationale, the selective use of statistical inference looks rather like the haphazard use of statistical inference.

Perhaps the most common justification in sociology for statistical inference rests on a hypothetical model in which a very large number of probability samples are independently drawn from a population of *fixed* values and a sampling distribution of the resulting population estimates is constructed (Kish, 1965:9-11). Statistical inference is mandated by a special kind of stochastic "distortion" or "error" introduced by the researcher when only part of a population of fixed values is studied. Each *observation* can be seen as a random variable (Kmenta, 1971:104) and, building on the assumption of a very large number of independent repeated drawings, mathematical-statisticians have provided the techniques with which to estimate from a single sample, the long-run consequences of the sampling process.³

² Under the rubric of what one might call "analytical" or "theoretical" generalization, nonstatistical inferences are made routinely not only in sociological case studies, but in survey research when investigators generalize to units outside the sampled population, both in space and time (Kish, 1965:7-8).

³ I do not mean to imply that the step from an estimated sampling distribution to significance tests and/or confidence intervals is universally accepted. Indeed, there are many lively controversies sur-

However, as Hollis and Nell (1975:76) have observed "statistics does not become a calculus of probabilities either merely by being mathematically sound or merely by being christened a calculus of probabilities." A particular probability calculus must fit the proposed stochastic process and accurately describe the outcomes of its repeated "realizations" (see also Malinvaud, 1970:69-73). Perhaps responding to such considerations, Inverarity's footnote 19 characterizes his data as a "population" (implicitly discarding the random sampling model) and the Winch and Campbell (1969) article is cited as justification for using chi-square goodness-of-fit tests. Unfortunately, this still leaves serious ambiguities.

The random sampling model formally addresses *external validity*: inferences from a probability sample to a population of fixed observations. In contrast, the Winch-Campbell model addresses *internal validity* (Campbell and Stanley, 1966): the degree to which alternative explanations for an assumed causal effect can be ruled out.⁴ Their perspective draws heavily on the randomization process in true experiments by which a researcher attempts to statistically equate groups of subjects (or other units) before the treatment is applied. Subjects are randomly assigned to treatment and control groups so that preexisting systematic differences between individuals are transformed into random *group* differences. While the "luck of the draw" during the assignment process may result in actual nonequivalence (thus confounding treatment effects with "selection" effects), the probability of this occurrence can be gauged with significance tests. Put another way, significance tests reflect the probability over repeated random assignments of the entire subject pool, that what one takes as a treatment effect really results

from chance group differences between experimental and controls (Fisher, 1971:13-4; Lindquist, 1953:11-2).⁵

While the relevance of the randomization model for Inverarity's goodness-of-fit tests may not be immediately apparent, Winch and Campbell (1969) provide the necessary links. To freely translate their somewhat informal discussion, they suggest that regardless of the causal model being entertained, one should also consider a world in which any associations between observed variables are the result of some random assignment process. In other words, social forces replace the researcher as randomizer, and just as one must rule out chance in randomized experiments, one must rule out chance in substantive models. Note that the problem is not random sampling error as described earlier under the random sampling perspective and, thus, not external validity. Rather, the problem is internal validity and ruling out a particular kind of alternative (stochastic) explanation for observed associations.

How well does this model fit Inverarity's cross-sectional data? To begin, footnote 19 is, in fact, broadly consistent with what appear to be Winch and Campbell's central arguments. However, why the model justifies goodness-of-fit tests and not tests of individual coefficients would seem to need further clarification. A random assignment world would appear to warrant the null hypothesis that each of the coefficients are zero. (For some important consequences of this, see Bohrnstedt's adjacent comment.) Then, significance tests would suggest which observed relationships might be understood as random error and which could be attributed to other (deterministic) processes.

More fundamentally, one must also consider how sensible the hypothetical random assignment world really is. Winch and Campbell would seem to require a world that first attaches to each county a value for an endogenous variable and then randomly pairs with it

rounding these inferential procedures which seem a very long way from resolution (Hogben, 1957; Morrison and Henkel, 1970; Barnett, 1973; Bartlett, 1975; Ferguson, 1967).

⁴ This requires some qualification. In the Winch and Campbell (1969) article, the authors were still operating under the Campbell and Stanley (1966) typology. "Instability" was added as a "new" threat to internal validity and resulted from such things as sampling, randomization and unreliable measures. Tests of statistical significance only addressed instability. The revisionist perspective enunciated by Cook and Campbell (1975) treats instability separately as "conclusion validity." In other words, while the issue remains alternative explanations for assumed causal effects, internal validity is reserved for systematic biases while conclusion validity refers to chance factors. And again, tests of statistical significance are only appropriate for the latter.

⁵ While there is wide agreement on the particulars of the randomization models, there is a healthy debate about the appropriate tests when the model is applied to an actual experiment. Subjects and their attached scores on a dependent variable(s) are commonly viewed as randomly shuffled between experimental and control groups (e.g., Siegel, 1956:88-9), but the choice between nonparametric, component-randomization tests and parametric tests is controversial. (Siegel, 1956; Edgington, 1966; Alf and Abrahams, 1972). Regardless of which tests are used, however, all address internal validity and provide no information about generalizations beyond the pool of subjects actually assigned to treatment and control groups.

values for each of the exogenous variables. This entire process is then repeated a very large number of times to generate a "population of realizations" of which the observed data is a single realization. Without such repeated assignments of the full pool of observations, the required sampling distribution on which probabilistic statements necessarily rest is at best imaginary. And while concepts for imaginary things (such as mass in physics) can be useful, they should not be introduced without careful thought and discussion.⁶

Further complications immediately surface when one remembers that, according to the Winch-Campbell model, *the pool of potential observations on all variables is fixed, all marginal distributions remain unchanged in repeated random assignments of the entire pool, and the values of endogenous variables attached to particular counties are constant.* These are substantive statements about a very special kind of randomizing world or, alternatively, about particular historical processes. Why not allow new values on any of the variables to appear in different realizations? Why not allow the marginals to vary? Why not allow values of the endogenous variables attached to particular counties to change? Moreover, the Winch-Campbell model seems only to apply to models with a *single* endogenous variable and thus provides no formal guidance for situations in which the covariance between multiple endogenous variables varies in repeated realizations. In short, the use of the Winch-Campbell model would seem to require far greater justification and may well be inappropriate. Should this be the case, the real meaning of Inverarity's goodness-of-fit tests become obscure. The stochastic processes Inverarity may want to consider are not the ones described by the model.⁷ Exaggerating somewhat, it is a bit like trying to calculate the probability of obtaining a head or tail with a model based on a six-sided die.

⁶ A full-blown Bayesian critique would go a good deal further. Rather than resorting to the mental gymnastics required to conjure up the necessary sampling distribution, the relative frequency approach to statistical inference in this case would be discarded in favor of statistical inference based on "degree of belief" (Barnett, 1973).

⁷ I suspect a better stochastic model could have been developed from econometric traditions (e.g., Malinvaud, 1970:59-61). However, while there is not space here to detail my uneasiness with such models, they in essence seem to define the important problems away through assumptions made about the stochastic processes involved in the "error term" (Berk and Brewer, 1977). The discussion below of time-series models will provide a brief exposure to some of these difficulties.

Turning to the time-series data shown in Figure 1, recall that several important substantive conclusions were reached with no considerations of random processes. Is this reasonable? In fact, stochastic models of longitudinal data may be usefully viewed as another class of randomization models. Each observation is treated as a random variable, the set of observations are treated as a family of random variables, and one can speak about the probability distribution for a given variable or the joint probability distribution for the family of random variables. In addition, when sequential numbers are affixed to each random variable, they represent an equal interval scale describing each observation's location in time. Therefore, each random variable can be treated as a function of time and a wide variety of stochastic models for the relationship between time and the observations may be postulated and tested (Cramer and Leadbetter, 1967; Cinlar, 1975).

Again, internal validity, not external validity is the issue. Each set of observations can be treated as a realization of an underlying stochastic process, not as a random sample from a larger group of time-dependent processes. The stochastic process is typically assumed to be invariant with respect to the displacement of time: the models are assumed to be "stationary" or are transformed into stationary models (Pindyck and Rubinfeld, 1976:435-6).⁸ Each number reflecting an observation's temporal location has meaning relative only to an arbitrary starting point. Thus, one might say that for these models, time has no "historical" interpretation. Or, to quote Nelson (1973:26), "in a probabilistic sense, history repeats itself in a stationary time series."⁹

⁸ At least two qualifications are required here. First, a few kinds of nonstationary models such as the "random walk" can be estimated but, by and large, nonstationarity is removed in social science applications. Second, there are actually many different kinds of stationarity. For example, "strict" stationarity requires that the joint probability distribution be invariant with respect to the displacement of time. "Weak" stationarity only requires that in repeated realizations the expected value of the series is constant, the expected value of squared deviation scores (the variance) is constant and, for any lag, the expected cross-product between random variables (the covariance) is constant. The former implies the latter, but not the reverse (Cramer and Leadbetter, 1967:120-3). For present purposes, however, such distinctions are not critical.

⁹ Technically, one could still speak about external validity to a population of all possible realizations of the stationary series. However, this is, by definition, almost always a substantively trivial population. Thus, interesting generalizations will typically flow from theoretical considerations.

To illustrate: in their introductory chapter to *Stationary and Related Stochastic Processes*, Cramer and Leadbetter (1967) concretize their discussion of stationary processes with an example involving the background "noise" associated with "phase tracking" of satellite transmissions. They display graphs showing that while the temporal series varies from observation to observation, it seems to oscillate around a constant level with fluctuations that seem on the average to be about the same size. Thus, while one requires that the observations be arrayed with real numbers indicating temporal location (e.g., to be able to graph the pattern), one could begin that series at any arbitrary point and expect to find the same patterns were the series to begin at any other arbitrary point. The ordering and spacing of observations is what matters, not the starting point.¹⁰ Consequently, one could take a very large number of time-series samples with each replication treated as an independent realization of the same underlying stationary stochastic process, *even though each realization would necessarily have a different starting point*. This leads to the critical result that a meaningful sampling distribution could be produced from which probabilistic inferences could be drawn.

What happens if the empirical series is, in fact, not stationary? While there is some disagreement on how best to handle nonstationarity (Glass et al., 1975; Hibbs, 1974), there is currently consensus that, for social science applications, one usually interprets its causes as deterministic and attempts to remove them before stochastic models are applied.¹¹ Box and Jenkins (1970) emphasize "differencing" to remove longitudinal trends (e.g., a series shows a gradual increase); econometricians such as Kmenta (1971) and Johnston (1972) emphasize regression techniques. Nevertheless, the statistical inference is still meant to address internal, not external validity.

The implications for Inverarity's time series should now be apparent. If the longitudinal pattern of lynching is viewed as having no stochastic component, conclusions may be drawn with no reference to chance. If the series is viewed as at least partly stochastic and

stationary, then formal estimation procedures may be appropriate. Finally, if the series is viewed as at least partly stochastic and nonstationary, attempts could be made to remove the nonstationarity before estimation. However, such decisions must be made in the context of what numerous replications (or "realizations") *mean* for historical data. The fit between the model of a stationary time series and radio waves, industrial processes and the like seems quite good. The case for patterns of lynchings or other historical events over time would seem far more difficult to make.

In summary, mathematical-statisticians have blessed us with a wide variety of stochastic models which may, in theory, be applied to all manner of social science data. Moreover, these models are often amazingly robust when certain assumptions are violated. However, robustness must not be confused with infallibility if the probability assessments which emerge are to be taken seriously. This means that selection of a particular stochastic model must be carefully made and that one also must determine more generally if stochastic processes, as currently defined, are applicable for the data on hand.

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¹⁰ In their introductory chapter, Box and Jenkins (1970) draw on similar illustrations of stationary processes. The most important point for our discussion is that while observations are taken over time, the starting point is irrelevant.

¹¹ This consensus may be breaking down, since models with stochastic parameters are becoming more common. See, for example, Cooley and Prescott (1973).

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USE OF THE MULTIPLE INDICATORS-MULTIPLE CAUSES (MIMIC) MODEL*

(COMMENT ON INVERARITY,
ASR APRIL, 1976)

In a recent issue of this journal, Inverarity (1976) applies Jöreskog's technique for the analysis of covariance structures (Jöreskog, 1970; 1973) to a Multiple Indicators-Multiple Causes (MIMIC) model of lynchings which occurred in Louisiana in the late nineteenth century. The purpose of this comment is to show that a re-estimation of his parameters indicates that the model for which he suggests support exists probably, in fact, should be rejected.

The basic three-indicator, three-cause model proposed by Inverarity to explain lynchings in Louisiana from 1889-1896 is shown in Figure 1. The structural equation linking the unobserved variable, Mechanical Solidarity (η), to the three exogenous variables, Percent Black (ϵ_1), Urbanization (ϵ_2) and Religious Homogeneity (ϵ_3), is given by

$$\eta = \alpha_1\epsilon_1 + \alpha_2\epsilon_2 + \alpha_3\epsilon_3 + \epsilon, \quad (1)$$

* The author is grateful to Richard Berk, William Bielby, Peter Burke, Arthur Goldberger, Robert Hauser and Michael Hout for helpful comments on an earlier draft of this comment. As usual, the author takes sole responsibility for any errors. Special thanks to Debbie Adams for typing the manuscript.

and the measurement model linking the three indicators, Percent Democratic Vote, 1892 Presidential Election (y_1), Percent Democratic Vote, 1896 Gubernatorial Election (y_2) and Lynchings, 1889–1896, to Mechanical Solidarity are

$$\begin{aligned} y_1 &= \beta_1 \eta + u_1 \\ y_2 &= \beta_2 \eta + u_2 \\ y_3 &= \beta_3 \eta + u_3 \end{aligned} \quad (2)$$

where α_1 , α_2 , α_3 , β_1 , β_2 and β_3 are the primary parameters of interest to be estimated.¹ Using the more compact matrix notation where y and x are 3×1 vector of observables, α and β 3×1 vectors of coefficients and u a 3×1 vector of error terms, Hauser and Goldberger (1971) and Jöreskog and Goldberger (1975) show that substituting (1) into (2) yields

$$\begin{aligned} y &= \beta(\alpha'x + \epsilon) + u \\ &= \pi'x + v \end{aligned} \quad (3)$$

which is the reduced form where $\Pi = \alpha\beta'$ is the coefficient matrix and

$$v = \beta\epsilon + u \quad (4)$$

is the reduced form disturbance vector. The covariance matrix of v is

$$\Omega = E(vv') = \sigma_\epsilon^2 \beta\beta' + \Theta^2, \quad (5)$$

where Θ^2 is diagonal. Jöreskog and Goldberger (1975:632) point out that the reduced form parameters remain unchanged if β is multiplied by a scalar and α and σ_ϵ^2 are divided by the same scalar. Some decision must be made about how to resolve this indeterminacy. Jöreskog and Goldberger chose to assume that the variance of ϵ is unity, i.e., $\sigma_\epsilon^2 = 1.0$ although, as we shall see, other decisions which fix the metric of the unobserved variable are also possible.

In attempting to reproduce the results Inverarity presented in his Figure 3, we first fixed σ_ϵ^2 to unity, but the estimates obtained varied from those presented by Inverarity in some unexpected ways.² Table 1 shows the results of

Table 1. Comparing Estimates for Inverarity's Figure 3

Parameters	Parameter Estimates		
	For Figure 1	Original	Re-estimates
α_1		.804	1.085
α_2		.128	.166
α_3		.130	.165
β_1		.501	.371
β_2		.770	.575
β_3		.283	.208
σ_{u_1}		.910	.831
σ_{u_2}		.715	.505
σ_{u_3}		.974	.950
σ_{u_4}		.743	1.000
			Ratio
			.741
			.771
			.798
			1.350
			1.349
			1.361
			†
			†
			†
			.743

† See text for an explanation of differences between Inverarity's and our estimates.

both Inverarity's and our analyses. First, note that Inverarity's estimates of the α_i are roughly .743 smaller than ours, and the β_i roughly 1.346 times larger than ours. Notice that .743 is Inverarity's estimate of the variance of ϵ and that if one multiplies our β_i by .743 and the α_i and σ_ϵ^2 are divided by .743 that the two sets of estimates are roughly equal, as one would expect given the indeterminacy discussed earlier. Apparently, Inverarity attempted to estimate σ_ϵ^2 rather than fixing it *a priori* and ACOVS, as it will do, provided an answer even though the model is not identified.³ Somewhat curiously, he has the correct degrees of freedom—six, instead of the five one would expect if he indeed tried to estimate σ_ϵ^2 . In reporting the standard deviations of the u_i , Inverarity apparently mistook the standard deviations the program printed for variances, since his estimates are the square root of ours except for rounding error.

By themselves, these differences would hardly be worth the fuss. Much more important is Inverarity's failure to report the standard errors of estimates.⁴ As Malinvaud (1970:60)

³ In trying to replicate Inverarity's results, we estimated σ_ϵ^2 instead of fixing it at unity. While we failed to replicate his results, we were able to generate yet another set of estimates for the α_i and β_i which satisfy the data equally well, further illustrating the indeterminacy of any solution without making some assumption about σ_ϵ^2 . We also thought that he might have used a procedure described by Werts et al. (1973) which combines maximum likelihood and least squares estimates, but we could not reproduce his results with this technique either.

⁴ In fairness to Inverarity, it must be pointed out that some versions of ACOVS, in particular ACOVSS and ACOVSL, do not compute the information matrix necessary to obtain the standard er-

¹ The notation used in this comment is that of Jöreskog and Goldberger (1975) rather than that of Inverarity.

² All of the results presented in this comment were estimated using LISREL (Jöreskog and van Thillo, 1972). However, the results shown in Figure 1 were reproduced exactly using ACOVSF (Jöreskog et al., 1970), guaranteeing that our results do not differ from those of Inverarity as a function of having used LISREL instead of ACOVS. We, personally, find MIMIC models of this sort easier to set up using LISREL.

points out, the use of tests of significance is justified on one of two bases:

On the one hand, the phenomenon to be studied may be considered to resemble a process entailing random determination of certain quantities which are in this case assumed to be random in the universe as well as in the observed sample. On the other hand, the individual observations may be selected at random in which case the sample is random in composition and therefore the data are also random even if they refer to non-random quantities.

We shall always adopt the first position. The stochastic model will be supposed to represent the process which generates the quantities studied.

On the assumption that Inverarity views the relationship between boundary crises, Mechanical Solidarity and repressive justice as a general social process, the use of significance tests with his sample of 59 Louisiana parishes seems appropriate even though these cases are not a random sample from some larger population.⁵

In Figure 1, we present re-estimates of Inverarity's Figure 3 in which we have rescaled the variance of η , the unmeasured variable, to equal unity. This rescaling means the entire solution is standardized and, hence, simplifies interpretability. In addition, the standard errors are presented in parentheses alongside the coefficients. Notice that neither the coefficient linking Urbanization (ξ_2) to Mechanical Solidarity (η) nor that linking Religious Homogeneity (ξ_3) to it is large enough to reject the null hypotheses, calling into question whether Urbanization and Religious Homogeneity properly can be thought of as causes of Mechanical Solidarity, even though a chi-square test suggests that the overall fit of the data to the model is reasonably good.

To further test the adequacy of this model, Inverarity (1976:276) poses an alternative model where Black Population Size is allowed to affect lynchings directly, in effect to "test whether the estimates of the first model are spurious." The results obtained lead Inverarity to conclude that the inclusion of Black Population Size in the model does, in fact, reduce the

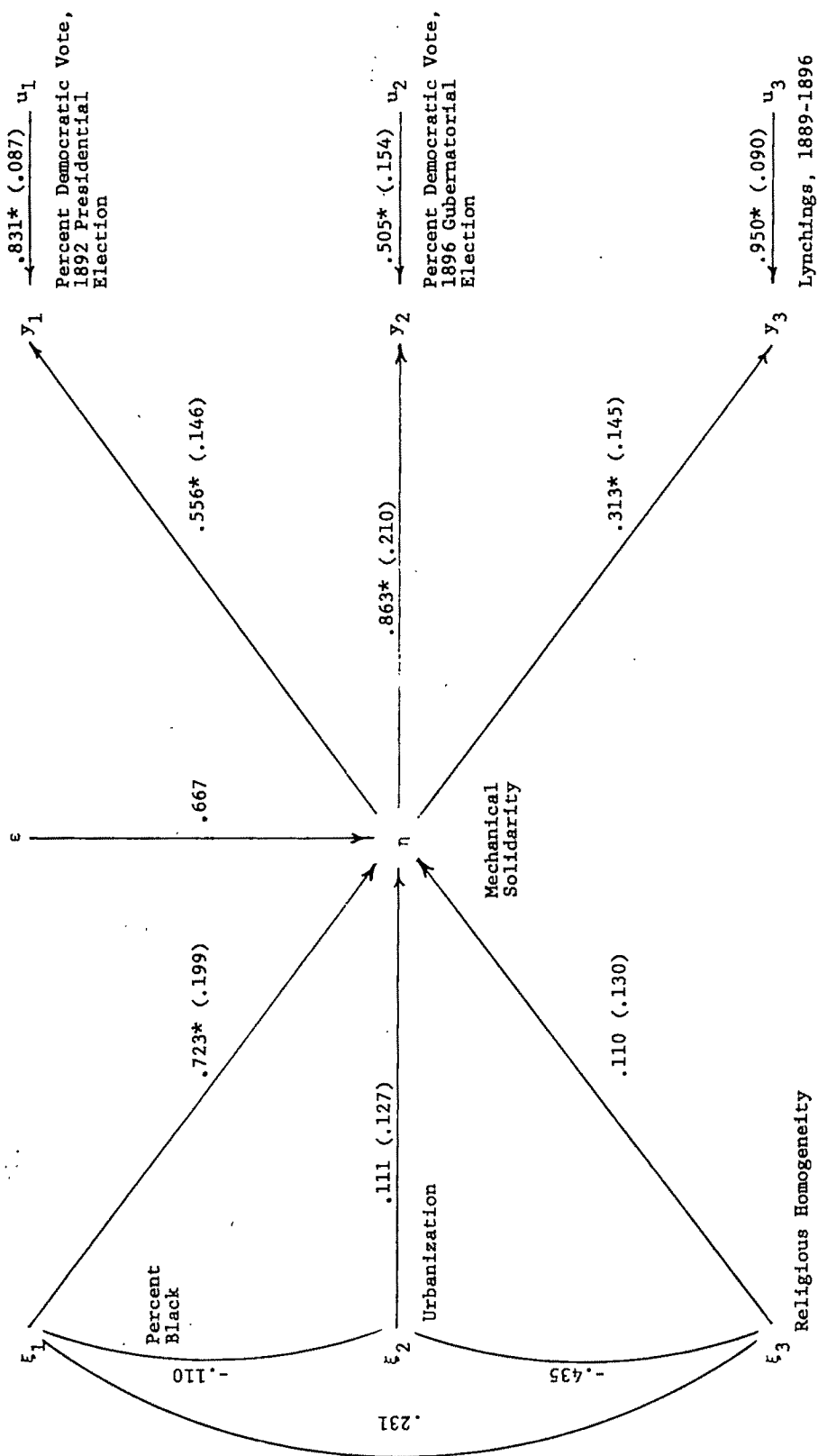
effect of Mechanical Solidarity on lynchings, but since some effects due to Mechanical Solidarity remain, Inverarity accepts the results of the analysis as providing additional evidence for the hypothesis that the degree of repressive justice is affected by the level of Mechanical Solidarity. However, our reanalysis of Inverarity's alternative model seriously calls this conclusion into question, since the path from Mechanical Solidarity is not statistically significant (see Figure 2).

What model might adequately explain the data? One alternative which utilizes the findings of the models already re-estimated is shown in Figure 3. While Inverarity's alternative model (his Figure 4) included Black Population Size, we have omitted this variable since it is also the numerator of Percent Black which guarantees a conceptual and mathematical dependency. A discussion of this problem can be found in Fuguitt and Lieberman (1974) and Schuessler (1974). Overall, the model shown in Figure 3 fits the data reasonably well ($\chi^2_2 = 3.68$, $p = .16$) although the correlation between Percent Black and Lynchings implied by the model is .202 whereas the observed correlation is .295 leaving an unexplained residual of -.093. Note that Mechanical Solidarity does have a small, but statistically significant coefficient linking it to Lynchings.

A natural, plausible alternative to the immediately previous model posits a direct path from Percent Black to Lynching. The data Spilerman (1970) presents on racial disturbances in the 1960s certainly suggests the plausibility of this alternative model. The results for this alternative model, shown in Figure 4, are unfortunately equivocal. The direct path from Percent Black is much larger than the path from Mechanical Solidarity to Lynchings. Specifically, the direct effects are .267 and the indirect effects through Mechanical Solidarity are only .028, seriously calling into question the need for Mechanical Solidarity as an intervening variable. But the standard error associated with the direct path is too large (.168) to allow one to reject the hypothesis that this parameter is in fact zero. Alternatively, the improvement in fit as assessed by a χ^2 test with one degree of freedom is marginal since $\chi^2_1 = 1.84$, significant at only the .20 level. But there is no question that *all* of the analyses suggest that Mechanical Solidarity as measured in this study, at best, has no more than a small effect on lynchings in Louisiana in the late nineteenth century. Indeed, the most obvious alternative is that Percent Black is the only variable one needs to explain lynching in these data, although it is our preference to let those who are

rors. We do not know which version was available to him. It is also the case that if a model is not identified, then the inverse of the information matrix cannot be taken and the standard errors would not be estimable (Wiley, 1974), a condition which will obtain if one attempted to estimate σ^2_η , instead of fixing it.

⁵ For a more detailed discussion of the issue of statistical significance using historical data, see the adjoining comment by Berk.



* Estimate is at least twice the size of its standard error. Standard errors in parentheses.

Figure 1. Maximum Likelihood Estimates of Invariance's Figure 3

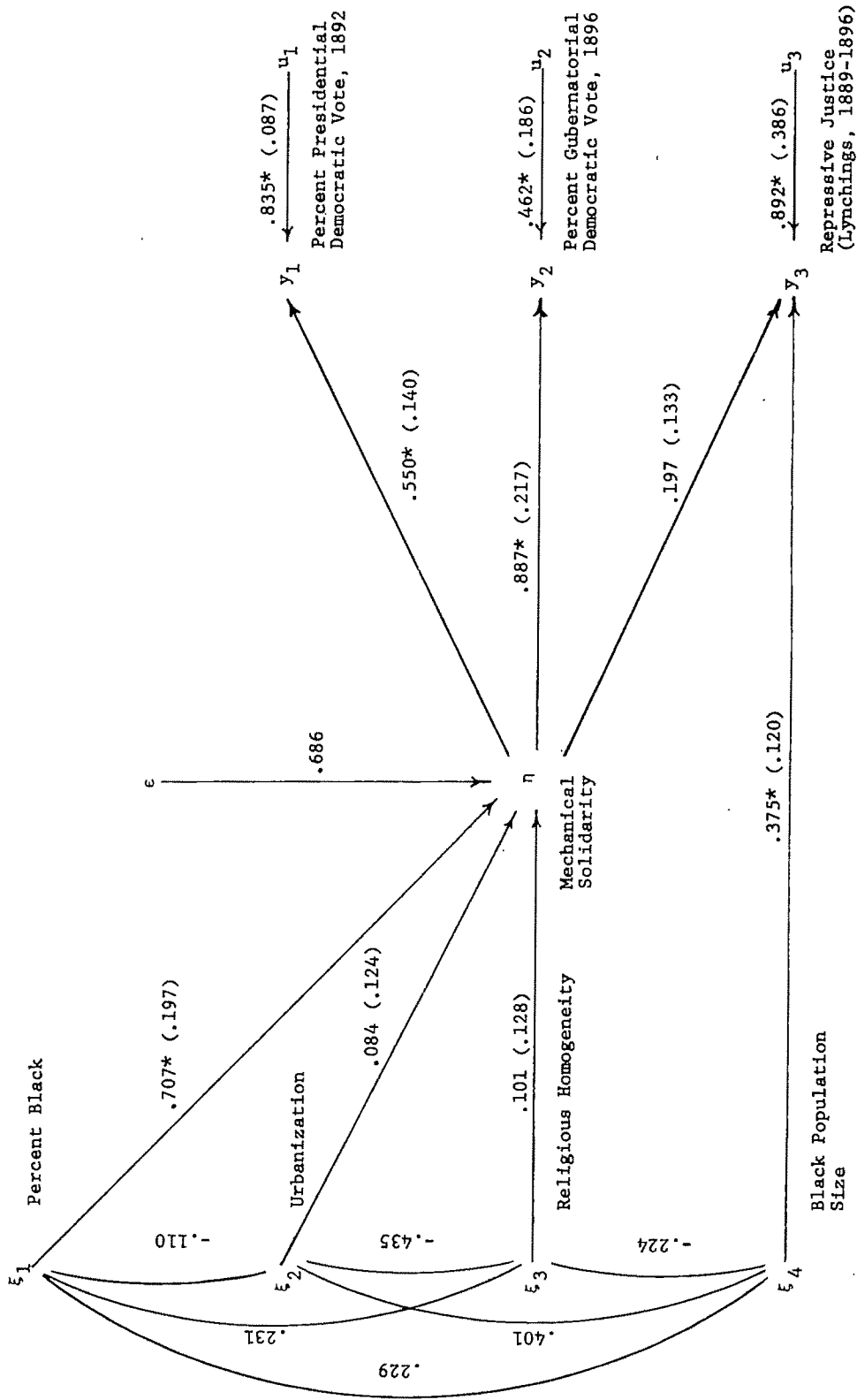
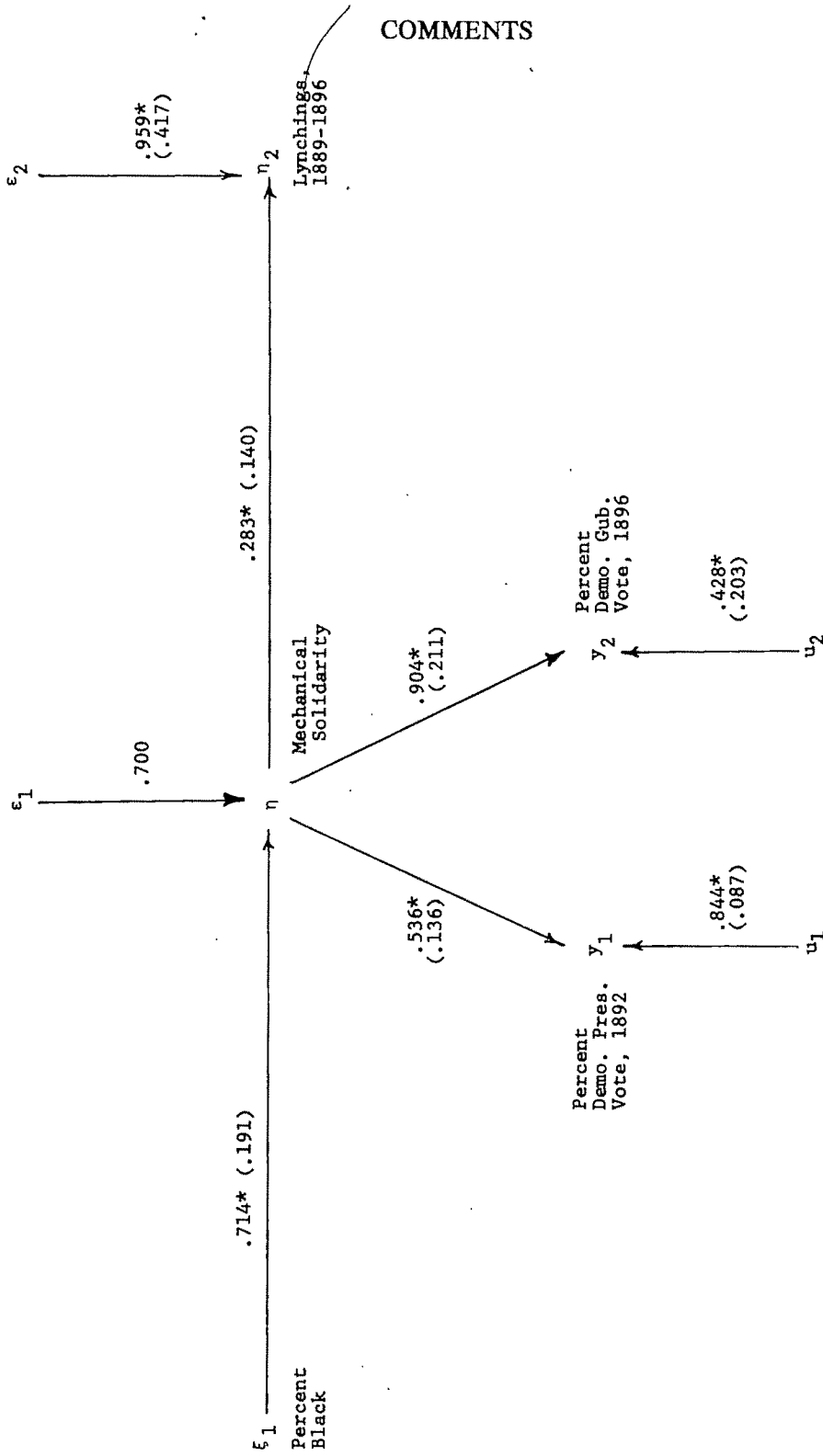
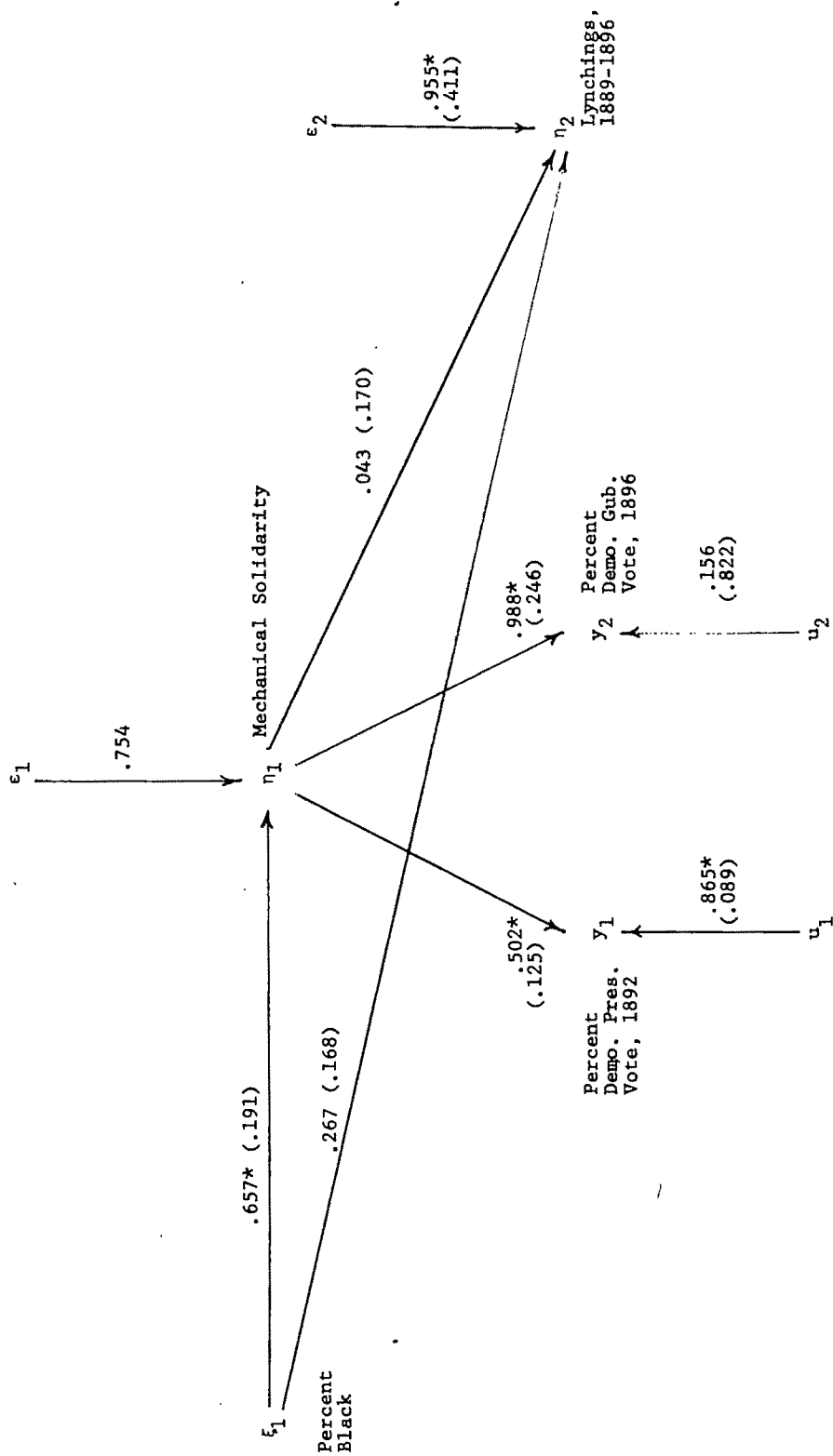


Figure 2. Maximum Likelihood Estimates of Inverarity's Figure 4



* Estimate is at least twice the size of its standard error.
Figure 3. Revised Causal Model Retaining Mechanical Solidarity



* Estimate is at least twice the size of its standard error.
 Figure 4. Revised Causal Model Retaining Mechanical Solidarity and Allowing for Direct Effects of Percent Black on Lynchings

closer to this substantive area argue the merits of this alternative explanation.⁶

This exercise in model building has assumed the correctness of Inverarity's claim that Percent Black is a cause of Mechanical Solidarity and that the two measures of Democratic vote adequately reflect it. To the degree that these assumptions can be questioned, the substantive conclusions drawn above may have to be altered.

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REPLY TO BAGOZZI, BERK, BOHRNSTEDT, POPE AND RAGIN, AND WASSERMAN

The five comments that have appeared in recent issues of this journal correct and elaborate several ideas in the original article, and raise several issues that merit further discussion. Due to space limitations, however, I will restrict my reply to the following three issues, which seem to me in greatest need of clarification: (1) the relationship between the theories of Durkheim and Erikson; (2) the operational definitions employed by the article; (3) the role of significance tests in historical investigation.

Erikson versus Durkheim

First, Pope and Ragin's confusion over the relationship between Durkheim's thesis and Erikson's reformulation¹ requires a brief re-

¹ Erikson's (1966) point of departure is Durkheim's parable of the community of saints, in which repressive justice persists even though the community consists entirely of individuals leading exemplary lives (Durkheim, 1950:67-9). The moral of the parable, Erikson points out, is that punishment is the consequence of exigencies of community organization. Variation in those exigencies are the focus of Erikson's concept of *boundary crises*. There is, thus, much greater continuity between Erikson's study and Durkheim's theory than Pope and Ragin seem to recognize. It is, however, true that Erikson does not address many of the issues posed by Durkheim in his discussion of law and social structure (e.g., the evolutionary development thesis), and Pope and Ragin quite legitimately chastise my article for being too elliptical in discussing the relationship between the two.

⁶ Black Population Size would seem to be an even better choice as an exogenous variable since its zero-order correlation with lynchings is .418 compared to .229 for Percent Black.

statement of the purpose of my article. In *Wayward Puritans*, Erikson (1966: 5) set about "to see if [Durkheim's] insights can be translated into useful research hypotheses." Lost in the translation was the distinction between mechanical and organic solidarity. My article reintroduces this distinction because:

some actions . . . are more strongly repressed than they are reproved by general opinion [The state can become] an autonomous factor in social life . . . [defining] as criminal actions which shock it without, however, shocking the collective sentiments in the same degree. (Durkheim, 1964: 82-3)

Under what conditions will repressive justice be independent of collective sentiments? Durkheim (1964) provides no coherent answer² and Erikson (1966:9) bypasses the problem altogether, asserting that boundary crises and crime waves are universally associated. My article (Inverarity, 1976:263) questions this assumption on empirical grounds. Since this question is central to my analysis, let me belabor it briefly. I suspect that Erikson's argument could not be applied to crime waves in Massachusetts after the seventeenth century when that society moved from being a relatively homogeneous theocracy to a complex society characterized by occupational differentiation, denominational pluralism and ethnic diversity (see, e.g., Thernstrom, 1969:33-56). In the latter type of society, some actions are more likely to be strongly repressed than strongly reproved and, consequently, we could not expect any systematic relationship between crime waves and boundary crises. In general, attempts to apply Erikson's analysis to modern nation-states (e.g., Blumenstein and Cohen, 1973) are, from this standpoint, essentially misguided. My article seeks to formulate the general conditions under which boundary crises and crime waves are related by reconsidering Durkheim's distinction between mechanical and organic solidarity. In particular, the article attempts to formulate the issue empirically, conceiving of communities as varying in degree of mechanical solidarity. In communities with a low degree of mechanical solidarity (e.g., seventeenth-century Virginia), repressive justice will have little to do with collective sentiments, and boundary crises will

tend to be unrelated to crime waves. In communities with a high degree of mechanical solidarity (e.g., seventeenth-century Massachusetts), repressive justice will be closely linked with collective sentiments and, as Erikson (1966) demonstrates, crime waves vary systematically with boundary crises. The ideal research setting in which to test this argument would be one in which a number of diverse, yet comparable communities face a single boundary crisis. Unfortunately, Clio seldom provides for research designs so we are forced to make do with approximations. Such an approximation for our present problem can be found in the relationship between the solidarity among Southern whites and their propensity to lynching during the 1890s. Several of the comments raise serious questions over how good an approximation this period actually provides.

Operational Definitions

Specifically, the comments question the following equations made in the article: (1) lynching = repressive justice; (2) Solid South = mechanical solidarity; (3) Populist revolt = boundary crisis.

Lynching and repressive justice. Pope and Ragin reject the equation of lynching with repressive justice because, in most cases, white mobs lynched black victims and it is written (Durkheim, 1964:73) that a community imposes repressive justice on "those of its members" who offend the collective conscience. Since black victims are not members of the white community, the lynchings of blacks *ipso facto* cannot be legitimately interpreted as instances of repressive justice. Such lynchings are, rather, instances of "intergroup sanctions."

While Durkheim's discussion of repressive justice typically does deal with cases in which a member of the group is singled out and punished for some offense committed, the transformation of an insider into an outsider is not a universal characteristic of repressive justice.³ For example, Durkheim (1964:85-6, 90) writes:

punishment consists of a passionate reaction. This character is especially apparent in less cultivated societies. In effect, primitive people punish for the sake of punishing. . . . It is thus that they punish animals which have committed a wrong act, or

² This issue arises only *en passant* in *The Division of Labor*, but Durkheim (1973) later elaborated on this qualification, introducing centralization of political power as a limitation on the conditions under which repressive justice is related to mechanical solidarity. This theme is not explored here; for some suggestive discussion of secular trends in agencies of capital punishment, see Bowers (1974:40).

³ Pope and Ragin suggest further that the punishment of outsiders falls more properly into Simmel's conceptual balliwick, overlooking the possibility that Simmel and Durkheim, at several points, are addressing the same phenomenon in different terminology (cf. Coser, 1961; Scott, 1976).

even inanimate beings which have been its passive instrument . . . [In more cultivated societies, punishment is] more clarified, it expands less on chance. One no longer sees it turn against the innocent to satisfy itself.

The object of repressive justice may, thus, be neither a member of the group nor objectively guilty. The *sine qua non* of repressive justice is not the object of punishment, but the diffuse, passionate, ritualistic character of the reaction against an alleged offender. This point is of particular importance for Erikson and others who subscribed to some version of the labeling approach to deviance. Central to the labeling orientation is Durkheim's discussion of crime as a consequence of punishment and suggestion that the instigation of repressive justice lies not in the nature of the criminal act, but in the exigencies of the social system (see, e.g., Bordua, 1967).

Wasserman's objection to equating lynching with repressive justice rests more on empirical than exegetical grounds. Following Tilly's (1975) thesis that collective violence is often a consequence of collective action undertaken by political actors to gain or maintain their position in the polity, Wasserman contends that the increase of lynching in the 1890s could be explained as a consequence of the disfranchisement battles during this period. He then attempts a crucial test between my argument and the implications of Tilly's general formulation by comparing the impact of the Populist contest with the impact of the disfranchisement controversy on lynching.

It is not clear to me, however, how his test can be used to decide between a boundary crisis and a political contest interpretation of lynching episodes. First, it is not clear why the Populist revolt could not be considered a political conflict in Tilly's terms. A major issue for the Populists was the political representation of the class interests of lower-class whites. On the other hand, the disfranchisement controversy can be viewed as a manifestation of the boundary crisis of the Solid South. The theoretic frameworks of both Erikson and Tilly would account for an increase in lynching as a consequence of the political crisis in the South during the 1890s. Both formulations, it seems to me, would take the Populist revolt and the disfranchisement movements as indicators of the political crisis. The difference between the two theoretic formulations lies more in the processes by which political conflict is displaced into forms of violence, which may themselves be manifestly apolitical (e.g., food riots). Adjudicating between the two explanations will require a greater elaboration of the theories than Wasserman presents and prob-

ably more refined data than either of us has presented so far.

The Solid South and mechanical solidarity. Pope and Ragin contend that it is inadmissible to operationalize mechanical solidarity as solidarity among post-Reconstruction Southern whites because of the presence of conflict and the absence of complete homogeneity among Southern whites. The connection is problematic, but their objections are not particularly cogent.

Pope and Ragin's rough content analysis of my article reveals six references to "conflict" or its cognates in the discussion of the Solid South.⁴ Unfortunately, this juxtaposition of events ignores the main theme that after Reconstruction there was a period in Louisiana and throughout the South in which race and region became the focus of identification and political activity. The consensus was by no means total and its extent has been overstated by some historians. Nevertheless, an unprecedented level of solidarity among Southern whites after Reconstruction made Louisiana a very different type of differentiated society than, say, Massachusetts of the same period. Furthermore, the Populist revolt was experienced in certain states as a major social earthquake, to reiterate Carl Degler's metaphor, constituting what can be characterized in Erikson's terms as a boundary crisis. In short, white solidarity was not nominal, and the presence of conflicts and contradictions in the society of white Louisiana no more invalidates the applicability of Erikson's argument to this setting than the presence of conflicts and contradictions invalidates the applicability of his argument to Puritan Massachusetts.

The second major objection is that while mechanical solidarity is characteristic of undifferentiated societies, the white population of Louisiana in the 1890s is highly differentiated. For mechanical solidarity to be present among Louisiana's whites, one would have to claim

⁴ Some of the misgivings of Pope and Ragin appear to be motivated by a conflict versus consensus orientation toward social theories. For them, the presence of conflict "brings to mind the Marxian image of class conflict or the Weberian image of competitive advantage between and among classes" (cf. Chambliss, 1976). I find myself more inclined toward Giddens's (1976:717) position that the conflict-consensus dichotomy has outlived its usefulness and tends to hinder analysis. Indeed, one of the virtues of Erikson's study is that it transcends this dichotomy, showing how the structural contradictions in Puritan society produce episodes of repressive justice as attempts to redefine moral boundaries.

that "Southern whites were all alike and exhibited [a] lack of individuality" (Pope and Ragin 1977:364).

To be sure, not all white Louisianans had the same occupation; not all belonged to the Baptist Church, the Masons, or the United Confederate Veterans. But does such heterogeneity preclude the characterization of such a society as mechanically solidary? Durkheim (1950:83), after all, points out that the horde, consisting of undifferentiated individuals, is an ideal type, "the protoplasm of the social realm, the natural basis of classification. It is true, perhaps, that no society corresponds to this description." Using the criterion proposed by Pope and Ragin, *mechanical solidarity* could never be applied to any historical society. The real issue is a much more subtle one: whether the solidarity among Louisiana's whites is of a nature and extent that can be characterized adequately as mechanical. Pope and Ragin feel that it cannot, but the question must be determined on a more explicit assessment of the historical evidence.⁵

Populist revolt and boundary crisis. Pope and Ragin identify a serious equivocation in the original article's (Inverarity, 1976:264) statement that "election statistics provide . . . an index of the extent of variation in boundary crises." This makes the subsequent use of percent Democratic vote as an index of solidarity puzzling. The intended meaning of this statement was that (1) the occurrence of elections would be the focal points of the boundary crisis and (2) the magnitude of the boundary crisis could be approximately gauged by the magnitude of the Populist vote *among states*. Underlying this perspective is the idea that counties in the same state all experience a common

boundary crisis by virtue of the fact that states are the principal foci of political organization—parties are formed at the state level and most of the major political offices (including Presidential electors) are chosen at the state level. This means that a parish which is 100 percent Democratic would be threatened by Populism as much as a 50 percent Democratic parish. The occurrence of elections, then, can be used as indices of variation in boundary crises, while the percent Democratic vote *within* a parish could be used as an index of solidarity.

Role of Significance Tests

Finally, I would like to make some very brief observations on Berk's (1977:675–6) important critique of the use of significance tests with nonexperimental data. Berk is quite correct in arguing that the use of such tests ought not be perfunctory and more care should have been taken in my article in specifying the meaning of such a test for these data. I introduced the goodness-of-fit test as *one* alternative plausible explanation for the observed pattern of relationships. Such a test can best be conceived as a simple counterfactual model (see McClelland, 1975:146–68). Such models have fairly widespread use in econometric history where they generate the same fundamental objections over the utility of hypothetical conclusions drawn on the basis of highly questionable assumptions.

Conclusion

The specification and estimations of my causal models have been corrected by Bagozzi (1977) and Bohrnstedt (1977). The upshot is that percent black is the only significant causal variable and that one could dispense with mechanical solidarity as a cause of lynchings by allowing percent black to affect lynchings directly. These results are not surprising since almost all studies of race relations find racial composition as the overriding independent variable (cf. Spilerman, 1970). The basic problem for those interested in doing research in this area is that percent black is a proxy variable; moreover, it is a proxy variable for a number of different processes. Consequently, a major task in any future analyses of lynching will be to develop models and locate indicators that make it possible to interpret meaningfully the statistical effects of racial composition.

Finally, I would like to thank the authors of these comments for locating errors in the origi-

⁵ Another exegetical objection is that mechanical and organic solidarity have an unilinear evolutionary relationship in Durkheim's work and "Durkheim did not portray societies as rapidly fluctuating in and out of mechanical status" (Pope and Ragin, 1977:363). He had no particular reason to do so, since such fluctuations are atypical. But they do occur. Such a regression occurred among Southern whites after Reconstruction. More generally, disasters are often followed by a "resurgence of mechanical solidarity" in communities (Turner, 1967). Along similar lines, Durkheim (1965:244–5) treats the civic religion of the French Revolution as an atypical resurgence of mechanical solidarity but one useful for analysis of social processes. In short, the typology (mechanical-organic, segmental-differentiated, repressive-restitutive) is not treated by Durkheim quite as rigidly as Pope and Ragin suggest (cf. Durkheim 1964: 105n); moreover, such a rigid construction would render Durkheim's ideas inapplicable to any empirical context.

nal article and helping me to clarify my analysis. I hope to give a more extended and adequate treatment to the issues discussed here in my future work.

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STRUCTURAL CONTROL MODELS OF ORGANIZATIONAL CHANGE: A REPLICATION AND CAVEAT *

(COMMENT ON HUMMON, DOREIAN AND TEUTER, ASR DECEMBER, 1975)

In a recent paper, Hummon, Doreian and Teuter (1975) introduced a methodology which we believe represents a promising advancement for the study of organizational development. With it, the authors purport to have captured the dynamics by which "all components of organizations influence the change of all other components" (Hummon et al., 1975: 816). Hummon et al. test the

* Authors' names are listed alphabetically. Data collection was supported by the Center for the Management of Public and Nonprofit Enterprise, Graduate School of Business, University of Chicago. Data analysis was supported by the School of Graduate Studies, Queen's University.

model with data originally collected by Meyer (1972). These data are from a sample of 194 finance departments and include size of production component (P), size of supervisory component (S), number of divisions (D) and the number of levels (L) for both 1966 and 1971. The major short-run structural effects identified by Hummon et al. are summarized in Figure 1. The substantive findings are somewhat unexpected because organization size, which is closely represented by P, does not appear to have the unidirectional influence on other structural variables found in earlier studies (Meyer, 1972; Blau, 1970). The four structural characteristics are interdependent in a very complex manner, and D and L, which represent horizontal and vertical differentiation, have a substantial direct influence on P.

There is every likelihood that additional longitudinal studies of organizations will be undertaken in the future and that the use of such control models will increase. Thus we undertook to replicate the Hummon et al. procedure in order to test the method and to learn whether the unexpected structural influences are characteristic of a separate sample of organizations. We find that the method appears to have great potential for organization studies. However, during the replication we encountered problems which indicate to us that the particular model used by Hummon et al. may not be sufficiently well specified to yield valid results.

Replication

The structural control model developed by Hummon et al. involves three major assumptions.

Assumption 1. There is some optimal value for each organization component, P, D, S and L, at each point in time. These values

may be represented by a four-dimensional vector, X^* , where

$$X^* = \begin{bmatrix} P^* \\ D^* \\ S^* \\ L^* \end{bmatrix}.$$

Assumption 2. The optimal value for each organization component may be approximated as a linear function of the actual values of the other organization components. In other words, the optimal number of administrators is a function of the actual number of production workers, levels and divisions; the optimal number of divisions is a function of the actual number of administrators, levels, and production workers, and so on. This system may be expressed as

$$X^* = AX + B \quad (1)$$

where

$$A = \begin{bmatrix} 0 & a_{12} & a_{13} & a_{14} \\ a_{21} & 0 & a_{23} & a_{24} \\ a_{31} & a_{32} & 0 & a_{34} \\ a_{41} & a_{42} & a_{43} & 0 \end{bmatrix}$$

$$\text{and } B = \begin{bmatrix} b_1 \\ b_2 \\ b_3 \\ b_4 \end{bmatrix}.$$

As we shall see below, this is the most critical assumption. By defining each optimal organizational characteristic as a function composed solely of other internal organizational variables, the organization is treated as a closed system. The authors specifically exclude external variables from their structural control model (p. 815). The model does not allow for environmental changes and, therefore, contains no explanation for what shocks the structural components out of equilibrium in the first place. Furthermore, if the model is stable, each component will move toward its optimal value and come to rest in a stationary state.

Assumption 3. The difference between the optimal and actual values for organization components causes a change in the structural components. For example, if the actual number of administrators is below the optimal level, the organization presumably will add administrators. These adjustment mechanisms can be represented by the following differential equation system:

$$\dot{X} = C(X^* - X) \quad (2)$$

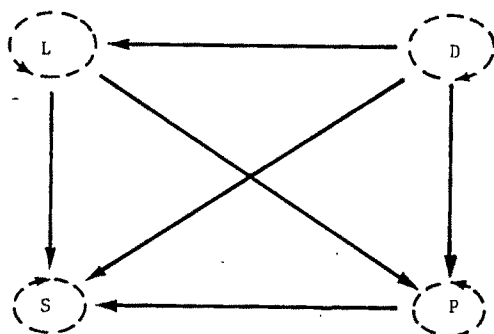


Figure 1. Major Short-Run Dynamic Influences in Finance Departments (Hummon et al.: 821)

where

$$C = \begin{bmatrix} c_1 & 0 & 0 & 0 \\ 0 & c_2 & 0 & 0 \\ 0 & 0 & c_3 & 0 \\ 0 & 0 & 0 & c_4 \end{bmatrix}$$

$$\text{and } \dot{X} = \begin{bmatrix} dP/dt \\ dD/dt \\ dS/dt \\ dL/dt \end{bmatrix}$$

Substitution of (1) into (2) yields the result that short-run structural adjustments may be represented in terms of the actual organizational profile at each point in time. That is,

$$\dot{X} = C(A - I)X + CB \quad (3)$$

or, as Hummon et al. express it,

$$\dot{X} = QX + r \quad (4)$$

where

$$Q = C(A - I), \text{ and } r = CB. \quad (5)$$

We fitted the structural control model in precisely the same fashion as Hummon et al., using data from a sample of 92 Illinois high school districts. Data for 1965 and 1970 were collected from annual reports filed with the Illinois State Superintendent's Office. The specific measures used were:

Size of Production Component (P) = Number of Teachers (1965: $\bar{X} = 103.9$, S.D. = 92.2; 1970: $\bar{X} = 140.6$, S.D. = 132.2).

Size of Supervisory Component (S) = Number of superintendents, principals, assistants, supervisors, coordinators and business managers (1965: $\bar{X} = 5.8$, S.D. = 7.1; 1970: $\bar{X} = 10.8$, S.D. = 12.4).

Number of Levels (L) = Number of hierarchical levels between superintendent and teachers (1965: $\bar{X} = 3.2$, S.D. = 1.0; 1970: $\bar{X} = 3.6$, S.D. = 1.1).

Number of Divisions (D) = Number of occupational specialties extant in the district (1965: $\bar{X} = 6.7$, S.D. = 2.4; 1970: $\bar{X} = 7.3$, S.D. = 2.8).

Our coefficient estimates for the optimal structure, $X^* = AX + b$, are¹

$$A = \begin{bmatrix} 0 & 0 & -434 & -705 \\ 0.053 & 0 & -0.369 & -0.213 \\ 0.261 & 0 & 0 & 0.448 \\ 0.0056 & 0 & -0.013 & 0 \end{bmatrix}$$

$$\text{and } B = \begin{bmatrix} -0.0214 \\ 1880 \\ 3.96 \\ 5.20 \end{bmatrix}$$

¹ The data for intermediate computations are available upon request.

Our estimates for the short-run dynamic adjustment model, $\dot{X} = QX + r$, are

$$Q = \begin{bmatrix} 0.00862 & 0 & 3.75 & 6.08 \\ 0.0379 & -0.719 & -0.265 & -0.153 \\ 0.133 & 0 & -1.11 & -0.497 \\ 0.00215 & 0 & -0.0049 & -0.384 \end{bmatrix}$$

$$\text{and } r = \begin{bmatrix} 0.0947 \\ -16.7 \\ 2.09 \\ 3.27 \end{bmatrix}$$

In Figure 2, we summarize the major short-run features of structural change in our sample, which may be compared with the Hummon et al. findings in Figure 1. The high school districts appear to behave quite differently from the finance departments. Our results are closer to traditional findings because P (size of production component) is the major causal variable. All other variables in the system respond to changes in P. The effect of P is not unidirectional, however, because L and S have reciprocal effects upon P. In the finance departments (Figure 1), D appears to be the major causal variable, while D has no significant influence on other variables in the high school sample. The other major difference between the two sets of organizations is the lack of negative feedback within P of the high schools.²

² One of the eigenvalues of our system is slightly positive. Mathematically, this implies instability. It is interesting to note, however, that Hummon et al. (p. 818) were on the verge of a similar result.

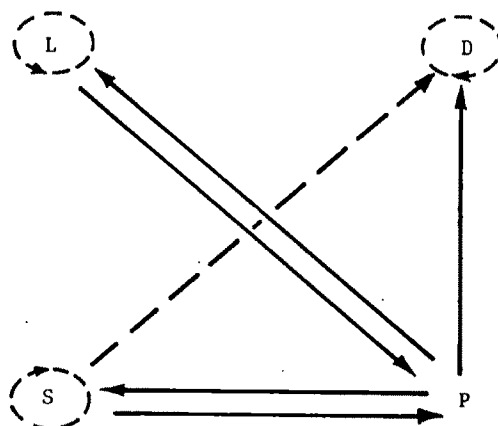


Figure 2. Major Short-Run Dynamic Influences in High School Districts

Specification Problems

The differences in short-run adjustments for the two sets of organizations are of secondary importance at this point. We suspect that major structural effects in both models may have been induced by specification constraints. As Hummon et al. (p. 817) show, stability conditions must be met for a satisfactory dynamic interpretation of the results. But the requirement of stability itself has disturbing implications. If the model is stable, the short-run structural changes for each organization follow a trajectory toward a *fixed* optimal structure given by

$$X^{\infty} = (I - A)^{-1}B. \quad (6)$$

Furthermore, since (1) is assumed to hold for all structures, then it must also be true when the organization has its optimal structure. In which case, we may write $X^* = AX^* + B$, or

$$X^* = (I - A)^{-1}B. \quad (7)$$

From the equality of (6) and (7), we see that Hummon et al.'s assumption of a closed organization system implies that the actual structure gradually (but surely) converges to a fixed optimal configuration, with the *same* number of administrators, production workers, levels and divisions for *each* organization.³ This is surely unreasonable *a priori*, because one does not typically perceive organizations as making short-run adjustments solely to reach identical, fixed configurations. Furthermore, the computed optimal structures for finance departments and high schools (Table 1) contain other anomalies. Some optimal values are impossibly large while others are negative. One might question the validity of the short-run structural influences in Figures 1 and 2, when these influences are presumably moving each organization toward the unlikely optimal values of Table 1.

³ This follows directly from (6) and (7) and the fact that matrices I , A and B involve only constants.

Table 1. Projected Steady State (Optimal) Structures for Finance Departments and High School Districts

Component	Finance Departments	High School Districts
$P^{\infty} = P^*$	355	-54.5
$D^{\infty} = D^*$	-21.8	1879
$S^{\infty} = S^*$	39.5	-8.00
$L^{\infty} = L^*$	0.138	5.00

Hummon et al. are aware of the long-run optimal values, but deny that the organizations are moving toward those values (p. 819). This denial, however, appears to contradict their stability findings (p. 818). We certainly agree that the organizations are not actually evolving toward a fixed optimal structure. The problem is that the observed short-run structural control mechanisms behave as if each organization structure is moving toward fixed optimal values. It does not seem meaningful to consider the short-run outcomes of the model and ignore the long-run outcomes.

As it stands, this model creates a double bind for the user. If the requirement of stability is not met, then the findings assume that organizations can grow without bounds. If the requirement of stability is met, then the findings assume that all organizations are moving toward an optimal stationary state. Neither outcome is satisfactory.

Suggested Solution

The problem with the structural control model, as it now stands, lies with Hummon et al.'s assumption 2, as represented by equation (1). Only variables internal to the organization are included in the model, and optimal values are computed as functions of other internal variables. In order to bring the findings within the bounds of acceptability, it is necessary to specify the variables which truly cause long-run structural change and bring these variables into the model. The most important of these variables lie in the organization's environment.

In the past, there have been many organization studies concerned exclusively with relationships among internal structural variables, such as size, number of levels and administrative ratio. So long as simple correlation and regression techniques were used to describe the relationships, it was possible to exclude the environment without absurd implications. But the methodological advancement toward the specification of structural control models, which we feel is an appropriate step, requires a concomitant advancement in theory. The dynamic model throws the spotlight directly on theoretical shortcomings. It simply is not appropriate to describe an optimal organization structure in terms of itself. The infeasible optimal configuration (Table 1) provides evidence that the true causal variables lie elsewhere. Investigators must extend the theory of organizational change to include environmental

variables that cause structural changes to occur, and operationalize these variables in order to achieve valid results with this method.

For example, continuing the assumption of linearity, the following could be substituted for (1):

$$X^*(t) = AX(t) + DE(t) + B \quad (1')$$

where

$$D = \begin{bmatrix} d_{11} & \dots & d_{1k} \\ \vdots & & \vdots \\ d_{n1} & \dots & d_{nk} \end{bmatrix}$$

$$\text{and } E(t) = \begin{bmatrix} e_1(t) \\ \vdots \\ e_k(t) \end{bmatrix}$$

In this case, we suppose that k significant environmental stimuli have been identified. Since environmental factors will typically change over time, we have indicated this by the functional notation, $E(t)$. Stability in this case implies that the organizations converge continually toward an optimal structure of the form

$$X^*(t) = (I - A)^{-1}DE(t) + (I - A)^{-1}B. \quad (7')$$

As (7') illustrates, an environment-linked model implies that the optimal long-run structure of each organization is dynamic and depends upon the development over time of its own environment. We believe this makes for a more realistic theoretical specification. Here, the proper organizational structure is partially determined by internal adjustment, as in Hummon et al.'s formulation. This aspect is represented by the $(I - A)^{-1}B$ term, as before. But now, in addition, the environment of each particular organization also plays a part in determining the "right" structural configuration. This is represented by $(I - A)^{-1}DE(t)$. Since environmental factors are a function of time and subject to change, then so is the optimal structure of each organization. In addition, since the environment of each organization is different, then so will be the optimal organizational development over time. This specification thereby eliminates the double bind for the researcher. Stability no longer means that each organi-

zation's structure evolves toward the same fixed optimal state but, instead, stability implies that structural components of individual organizations adjust toward congruence with their particular environment as well as toward internal consistency.

Despite our reservations concerning the validity of the relationships in Figures 1 and 2, we shall use those findings as an example of where an investigator might look for relevant environmental variables for finance departments and high school districts. Figure 1 suggests that environmental stimuli may be causing finance departments to add new divisions. These stimuli might be demands for new services or changes in government policy. Based on Figure 2, we might hypothesize that a major external variable for high schools is community size, or number of students, which leads to the required number of teachers (P), which in turn influences other structural variables. There are certain to be additional variables which can be brought into these models. With a properly specified system, the internal structure would be expressed as a function of both itself and environmental variables. A test of the final model's appropriateness will be its theoretical believability and the computed optimal structural trajectory.

Caveat

The technique introduced by Hummon et al. is a promising one indeed, and we urge its further use. We feel Hummon et al. have made a substantial contribution to the methodology for studying organizational development and realize that data for environmental variables were not available to them. However, we warn future users of the technique that the best results can be obtained only when their theory and data are up to the method. When the model is properly specified, each structural variable will be only partly determined by other structural variables. The environment will also play a role in determining organizational configuration. Under a properly specified model, stability will imply that organization structure follows a developmental trajectory determined by the path of environmental rather than evolving toward a fixed stationary state.

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REPLY TO DAFT AND MACMILLAN

We appreciate the comments of Daft and MacMillan on our earlier paper (Hummon et al., 1975) and we shall address ourselves to some of the specification problems they raise.

They remark that we imposed the requirement of stability in order to achieve a satisfactory dynamic interpretation of our results. We disagree. Our original argument (p. 817) presented the idea of examining the eigenvalues of our coefficient matrix with a view to assessing the stability properties of the process of organizational change as we represented it. We presented the "decoupling" of our system to make clear the ideas of stability analysis. We did *not* impose the *requirement* of stability on our model. The model is interpretable under either stable or unstable conditions. Daft and MacMillan are, however, entirely correct in pointing out our lapse when we asserted that the organizations we analyzed were not evolving to a fixed optional structure. Indeed, our empirically estimated model contradicts such a statement. We erred by failing to stress the importance of the time frame required to interpret any empirical dynamic model. Daft and MacMillan argue, "It does not seem meaningful to consider short-run outcomes of the model and ignore the long-run outcomes." Again we disagree. Why not ignore long-run outcomes that have little credibility especially as they require projection over very long periods of time—in this case around five decades—when the observational period was half a decade? We remain convinced that the model can be interpreted usefully in a short-run time frame.

The most important point of their note is the specification of

$$\dot{X}(t) = AX(t) + DE(t) \quad (1)$$

as a more appropriate model¹ of organizational change where, in their notation, $X(t)$ denotes a set of time-varying endogenous variables characterizing organizations and $E(t)$ denotes a set of time-varying exogenous variables characterizing the environments of organizations. The matrix D describes the organization-environment interface and the matrix A describes the processes internal to an organization.

Daft and MacMillan suggest a seemingly straightforward and appropriate modification of the model we proposed earlier. However, mathematical models are most useful when a balance between theory, models and data is maintained (see Doreian and Hummon, 1976: 23-8; Leik and Meeker, 1975: 9-14). A breakdown in any one of these compromises the rest, and constructive developments in one have implications for the other two. Daft and MacMillan credit us with a substantial contribution to the modeling² of organizational change and go on to observe that such an advance requires a concomitant advance in theory. This should come as no surprise, but it also behooves us to consider some of the implications of using a model like that of equation (1). Our response to this comes in two parts and deals with two broad issues: (1) what kinds of theoretical advances are being contemplated and (2) what sort of data analytical implications does such a model have?

First, we would like to discuss the model in the context of the set of theoretical ideas that are currently receiving considerable attention in organizational research literature. Specifically, we describe how the model can be used to represent and evaluate contingency theories of organizational structure. While there appears to be little consensus on a precise statement of contingency theory, the following ideas and concepts run through the literature.

Formal organizations are open systems that interact with their environments. These interactions cause organizations to change their structures so that they are better able to cope with environmental influences and demands. Furthermore, according to this approach, the better an organization's structure is adapted to its environment, the more effective is the organization's performance.

Now consider the model of equation (1). The

¹ The matrix $E(t)$ contains a column of ones and Daft and MacMillan's term, B , has been absorbed into D .

² They talk of methodology but we see our contribution, such as it is, in terms of model formulation.

matrix A describes the process of structural change (adaptation), and the matrix D describes the process of environmental influences on specific elements of an organization's structure. Thus, it would appear that the model can describe the open system—structural adaptation part of contingency theory. The patterns of the elements of the matrices A and D provide an interpretable empirical map of these processes. Moreover, the time constraints of the system provide information on how rapidly adaptation occurs, and these values can be compared to the rates of environmental change to assess whether organizational change is phased to environmental change.

Suppose a researcher wanted to use the model to evaluate contingency theory. Suppose further that he or she had a data set consisting of the following types of variables: measures of organizational structure such as differentiation, hierarchy, numbers of productive employees and number of supervisors; measures of environmental variables such as complexity and the volume of workload demanded of an organization; and a measure of organizational performance. Such a data set also would have to incorporate time, either as a two or more wave panel design or as a complete time series on two or more organizations.

The model could be estimated for two subsets of organizations, one subset consisting of organizations with "high" performance measurements and the other organizations with "low" performance measures. According to contingency theory, high performance organizations ought to have structures which exhibit a more rational adaptation to environmental influences than low performance organizations. We would hypothesize that organizations operating in environments where change in complexity is relatively more important than change in workload would exhibit an adaptation pattern similar to the state finance department of the Hummon et al. analysis, for which change in structural variables is driven by adaptation in differentiation and hierarchy. Conversely, we would hypothesize that organizations operating in environments where change in workload is relatively more important than change in complexity would exhibit an adaptation pattern similar to the schools studied by Daft and MacMillan for which structural change is driven by changes in the number of production employees and, to a lesser extent, the number of supervisors. (Thus, contingency theory can accommodate multiple patterns of structural change if these patterns can be linked to differences in organizational environments.) It must be stressed that these are hypotheses that could be eval-

uated through the use of the model; the empirical patterns for finance departments and schools are used only to illustrate what we mean by patterns of rational adaptation.

Finally, if the high performance organizations exhibit these kinds of rational adaptation patterns and low performance organizations exhibit these patterns to a lesser degree, contingency theory would be supported.

We believe this extended example illustrates the theoretical power and usefulness of the model in the form proposed by Daft and MacMillan.

Having argued that there is great theoretical potential in the model suggested by Daft and MacMillan, we now consider data and estimation issues. Three data formats need to be considered: (1) when a time series of observations is available for a single organization, (2) when time series are available for a set of organizations (without any necessary correspondence of the time points when data were collected) and (3) the panel study format. The solution form to equation (1) can be written as

$$X(t) = e^{At} X(0) + \int_0^t e^{A(t-\tau)} DE(\tau) d\tau \quad (2)$$

and our subsequent discussion hinges about this equation. The case where a single time series is available has already been dealt with (Doreian and Hummon, 1976: chs. 4-5). The solution form was recast, after a change of variable for time to acknowledge the regular discrete observational form of the data, as

$$X(n+1, T) = e^{AT} X(n, T) + A^{-1}[e^{AT} - I] DE(n, T) \quad (3)$$

where T is the time interval between observations. From empirical estimates of e^A and $A^{-1}[e^A - I]D$, it is possible to recover estimates of A and D. Each equation in (3) takes the form of an endogenous variable being predicted from lagged endogenous and exogenous variables. As the data are time-series data, an autoregressive disturbance term is incorporated. An iterative maximum likelihood (MLE) estimation procedure was devised to estimate the matrices in (3). Any desirable properties of these estimators would be preserved under the complex transformations needed to recover estimates of A and D. We would now, in contrast to our article, emphasize an MLE approach in this context.

The second case of a set of time series can be handled in several ways. We suggest that procedures for interpolation and extrapolation can be used to create a close approximation to the panel format as discussed below. Alternatively, each organization could be treated separately as in the first case. The matrices A and D then could be treated as new variables to be analyzed. Such a strategy is suggested by the

work of Baloff (1966) on organizational start-ups, although the models he considered were of an altogether different, and less complex sort.

The case of panel studies appears to be much more difficult. Our earlier article (Hummon et al., 1975) had only two waves without measurement of exogenous inputs. To handle measurements on exogenous variables, we would have taken the solution form of equation (2), specified a specific functional form, e.g., exponential in t for $E(t)$. We then would have used the data on the exogenous variables for each organization to parameterize that form and have integrated the integral in equation (2). The specific solution form would specify an equation to be estimated which would involve endogenous and exogenous variables together with some constraints generated by our treatment of the exogenous variables. An iterative MLE approach could then be used to estimate the system of equations. Finally, we would work back to the specified model, undoing the transformations used to generate the estimation form.

Suppose we had more than two waves of data in a panel study format. The data then can be regarded as a time series of cross-sections of observations on organizations. This can be treated in terms of the pooling of cross-section and time-series data. Here, the pioneering work of Nerlove (1971) becomes relevant, but not, it seems, immediately so. The evidence garnered by Nerlove and others deals with two broad situations: (1) where the number of units (organizations) is reasonably large and the time series of cross-sections is reasonably long and (2) where the number of organizations is small and the time series is short. For the former, analytical results can be appealed to and, for the latter, we have only the results of Monte Carlo simulations. These are suggestive, but far from conclusive. Most likely, sociological

applications will be located in the second, and more messy category. This leaves unresolved many statistical issues, but a start has been made by Hannan and Young (1977) to deal with them.

This exchange of comments between Daft and MacMillan and ourselves illustrates one of the important features of model building; progress in formal modeling requires, and normally generates the simultaneous development of theoretical sophistication, the broadening of empirical scope and the extension of estimation procedures.

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ITEMS (Continued)

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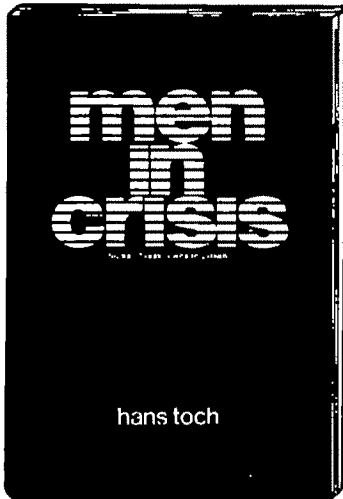
ITEMS (Continued)

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ERRATUM

An incorrect reliability coefficient for Armer's modernity scale was used in the structural equation analysis in Michael Armer and Allan Schnaiberg's reply to the comment by Cohen and Till (ASR April, 1972, pp. 378-382). Eliminating one item that had a negative item-to-scale correlation in the Armer scale results in an alpha of .596, standard deviation of 3.462, and modified correlations with other scales: Smith-Inkeles' (.569), Kahl (.612), SES (.413), and anomia (.530). Reanalysis using the corrected reliability coefficient modifies path coefficients to Armer's scale from modernity (.49), SES (.28), anomia (-.64), and residual disturbance (.02). These estimates lead to a substantial revision of the percentage contribution to scale variance attributable to invalidity factors (74.5%), "modernity" (23.5%), and disturbance factors (2.0%). The corrected results parallel those for Smith-Inkeles' and Kahl's scales, which remain virtually unchanged, and strengthen rather than weaken the conclusion regarding the invalidity of modernity scales.



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ITEMS

October, 1977

■ DEBORAH SCHIFFRIN (Opening Encounters) is a Ph.D. candidate in the Department of Linguistics at the University of Pennsylvania. She is studying deviations from sequential structures in conversation and interaction and also the formal and functional aspects of language use and change.

■ JEAN M. GUIOT (Attribution and Identity) is Associate Professor of Sociology at Boston University, Brussels. His current research interests include social comparison processes, identity construction and interdepartmental coordination processes in complex organizations. He is co-editor (with P.

(Continued on page 830)

AMERICAN SOCIOLOGICAL REVIEW

October 1977

Volume 42 Number 5

OPENING ENCOUNTERS *

DEBORAH SCHIFFRIN

University of Pennsylvania

American Sociological Review 1977, Vol. 42 (October):679-691

This paper examines the social organization of opening encounters. A base form of opening sequences is suggested, and adaptations of this base form are used to illustrate how the ritual and formal structure of openings interact with variable social and personal circumstances. Some general relationships between recognition, ritual, interaction and social order are suggested; and the implications of these relationships for understanding the structure of ongoing conversation and their relation to social organization are considered.

Everyday social activities, such as conversations, meetings and parties, impose specific demands on their participants. For example, individuals are required not only to be physically present for an activity, but to express a psychical or emotional presence as well, such that they may be said to be "alive" to a situation and "properly involved" with their partners in the activity (Goffman, 1963a). At the same time, each participant in an activity is required to show identification, or *cognitive recognition*, of the other as a viable partner in that activity. Such identification involves not only identifying an individual as being a certain person, with a known identity, but more generally, it involves the sorting out and categorizing of known from unknown individuals, and those acceptable for interaction from those unacceptable.

Although the sorting-out procedure is cognitive in nature, the production and provision of the cues which promote such categorization are socially organized both on a societal and individual level; and societies, as well as individuals, differentially produce, interpret and use identificatory cues (see, for example, Lofland, 1973). Despite such variation, the function of those cues and the identifications which de-

pend on them is constant in at least one respect: they separate acquaintances from strangers, one personal acquaintance from another, one sex from another, one ethnic group from another, and so on. In short, they symbolize that such differences are socially meaningful. Given this function, it is not surprising that individuals display both their own identities and their cognitive recognition of others' identities and, further, that the production and interpretation of such *identification displays* is also socially organized.

An identification display alone, however, is neither socially nor ritually sufficient as evidence of personal identifications, at least if such identifications are to be followed by face-to-face interaction. Displays preceding such interactions between acquainted individuals need to signify not only cognitive recognition, but also acknowledgment of the heightened expectations and obligations inherent in the increase in mutual access, which an upcoming interaction contains. *Access displays*, then, signal not cognitive recognition but *social recognition* or "the process of openly welcoming or at least accepting the initiation of an engagement" (Goffman, 1963:113), thus showing that such expectations and obligations are being met.

Although much ongoing interaction is concerned with recognition displays, such displays are intensified and most ritualized in the opening and closing portions of encounters: openings use recognition displays

* I would like to thank Erving Goffman, William Labov, Diane Levy Miller, Dean MacCannell, Louis Scavo and two anonymous ASR readers for helpful criticisms of earlier drafts of this paper.

to demonstrate that time apart has not threatened a relationship in the past, and closings, to demonstrate hope that time apart will not threaten it in the future. Because human ritual is formal and technically efficient (Huxley, 1966) as well as symbolically meaningful (Goffman, 1967), it provides a means by which individuals can readily and efficiently express whatever recognition is demanded, even in one of the briefest of rituals, passing greetings; i.e., those which terminate the minimal increase in accessibility only seconds after its initiation, yet still leave open the possibility for future contact (Goffman, 1971:75-6), and opening greetings, which not only leave open a possibility for future contact but immediately actualize it.

In this paper, I will argue that cognitive recognition and social recognition, and their display in identification and access displays, are important in both the maintenance of social order and in the organization of social interaction. In order to do this, I will suggest a framework which treats cognitive recognition, identification displays and access displays as phases within an *opening sequence*. The main part of this paper will use this framework as a base from which to show transformations of the recognition structure of opening sequences in response to variable social and individual circumstance. Despite such circumstance, such transformations or *adaptations* maintain social order and the coherence of interaction. In conclusion, I will suggest some relationships between opening encounters, recognition, interaction, and social order.

Opening Sequences

Cognitive recognition. Before social interaction occurs, individuals cognitively recognize each other; that is, they "place" or identify another by "linking the sight of him with a framework of information concerning him" (Goffman, 1963: 112). For example, an individual is *categorically recognized* as a member of an ethnic group, a race, a social class, and so on, and such a sorting-out process helps to differentiate potential from non-potential interactions and to organize and qualify those which occur. Interactions based on categorical

identification are different from interactions with acquaintances, which are based on personal identifications. Before these can occur, individuals must *biographically recognize* one another on the basis of personal information. This process differentiates potential personal interactions from non-potential ones and qualifies those which occur; but beyond doing this by just sorting out one category of individuals from another (known from unknown), it also specifies who those personally-known individuals are. Only the latter type of interaction and identification will be considered here.

Cognitive recognition display. Cognitive recognition is not always displayed, but as Goffman (1963:113) notes, "it is difficult to engage in without expressing that one is doing so." Such *identification displays*, then, are the first ritually-required display in opening encounters, especially if it is known by the individual that he has been so identified. For example, upon identification of a personal acquaintance, one's eyes may light up, one may smile, walk briskly toward the other, call the other's name, "yoo-hoo," mutter to oneself, and so on.

Despite being ritually required under certain conditions, however, the function of identification displays is more technical than ritual: they externalize a correspondence between the appearance of a specific individual and an identificatory framework based on such an appearance which allows for personal identification. In other words, because identification displays are an open assertion that an individual has "been seen" in this cognitively referential way, they express the fulfillment of a technical requirement for personal interaction—that individuals personally know one another. Note, however, that just as human interaction requires more than the fulfillment of purely technical or formal conditions such as identification, opening sequences do so too. Therefore, even when identification displays are overlaid with imputations of ritual meaning, as when delight in identification of a friend is displayed by a high-pitched squeal, they are *not* greetings and they are not complete as opening sequences.

Social recognition display. Greetings occur after identification displays. Because they show that further access is ritually

and socially permissible, greetings are *access displays* and, as noted above, they signify not cognitive recognition, but social recognition.¹ Access displays do this by symbolically conveying a social identity and membership in a relationship: they make participants "present" to one another in a special way" (Callan, 1970: 115) and place them within a social context as specific social entities (Firth, 1972). Thus, the main function of greetings—to increase ritual accessibility—is ritual, rather than technical.

The sequence. The obligatory sequence of cognitive recognition, identification display and access display exhaust the ritual interchange between two acquainted individuals which accompanies an initial increase in their potential for mutual accessibility. It is this interchange which I call an opening sequence as represented in *Sequence 1*:

<i>Sequence 1:</i>	<i>A:</i>	A1	A2	A3
<i>Base form</i>	<i>B:</i>	B1	B2	B3

Such a representation can be read as a formal depiction of either the structure of opening sequences or production rules for participants representing possibilities for action within normative limits. Individuals are *A* or *B* and the three stages are represented with corresponding numbers. Reading across denotes sequential ordering of moves and, reading down, simultaneous occurrence.²

¹ When the term "social recognition" is used in a broader sense, e.g., as knowledge of specific social and cultural expectations and obligations, there is a cognitive side to social recognition displays to match the cognitive side of identification displays. Although such knowledge is certainly required for social interaction, in opening encounters the fulfillment of such norms (not just the knowledge of them) is displayed when one individual welcomes the initiation of an engagement with another and socially recognizes him in this narrower sense.

² *Sequence 1* suggests (and, I would argue, correctly) that in a base form of openings, *A* and *B* begin simultaneously and, therefore, it is the potential increase in accessibility which generates openings between acquainted individuals, not the first individual's greeting which generates the second's. This is not to deny that a second greeting is conditionally relevant on a first, but this dependency should not be allowed to obscure the ritual motivation behind both greetings. Thus, if

Adaptations

Adaptations of the base form of openings in *Sequence 1* illustrate how the ritual and formal structure of opening sequences interact with variable social and personal circumstance. The data used to illustrate these adaptations are typical of those in a set of 75 openings: of which I participated in 35 and observed 30, with the rest reported to me.³ Openings aren't limited to any one occasion or locale, so I recorded them whenever and wherever they occurred, and as soon as opportunity would allow. Because I wanted to observe natural behavior, the sequences were not generated intentionally for data or manipulated for certain effects. In recording, I noted as much as I could remember without limiting myself to any particular feature of the interchange. Obviously, this method of observation is less rigorous than some others, e.g., video tape; but the behavior that I was interested in was most amenable to this method. Most of my data are limited to white members of the American middle class; social class and racial differences would certainly produce stylistic differences.

Although all of the opening sequences discussed here were represented in my data, they do not exhaust the total range of opening possibilities. I would argue, though, that the existence of sequences, which are merely further modifications of

it were the case that a first greeting generated a second, we would still have to figure out why that first greeting occurred; and if we were to suggest that first greetings had different causes than second greetings, we would have to explain why two functionally equivalent and structurally interchangeable moves had different causes.

³ Despite the relatively small data base in which this framework was grounded, its validity can be examined on several levels: (1) empirical—how much of the behavior in openings can be described, how accurately, how parsimoniously, what is the range of openings which can be described both ritually and formally; (2) structural—is there a logical relationship between the stages within the scheme, is it internally consistent; (3) theoretical—does this scheme provide any new insights about interaction and social order which build on what we already know, does it suggest any new generalizations or any revisions of previous models.

the base form of openings, is more a strength than a weakness, especially with a heuristic scheme such as this. Note, also, that the observations which I used to formulate both the base and adaptive forms were assembled *before* the recognition framework was developed. I continued to observe openings without adding them to this data base (although I would have re-evaluated the framework if I had found a sequence that didn't fit) primarily because I wanted the base form and the adaptations to be grounded in the observations that I had collected before analyzing openings in the categories of the recognition framework, to be sure that I didn't consequently observe and record *only* what could be seen in those terms. However, in both the original set of openings and in those that I've continued to observe without recording, I have found none that couldn't be analyzed in this framework.

Recyclings. Misidentifications occur when an individual is linked with a framework of information which does not accurately relate to him. When a misidentification is discovered, the individual substitutes the relevance of one identification for another with a *double-take*; but if the misidentification has already continued into a display, the double-take has to be accompanied by a withdrawal of the display in a *take-back*. Both of these recyclings are illustrated in *Encounter 1*:

- (1) DS is standing at a bus stop.
Four men in a car stop at the light and look at DS.
DS looks at the car, then looks away.
- DT—DS looks at the car again, especially at a man in the back.
DS raises her eyebrows, peers into the car.
DS Hi-i-i (softly, questioningly); waves.
Man waves (exaggerated) and leans forward, laughing.
Other men in the car also lean forward and laugh.
- TB—DS Oh (to self) whoops.
— H-h-eh, sorry (softly); grabs hand back.
— Shit (to self).

Double-takes, then, rearrange opening sequences because they recycle back to a second round of identification, with only one leading to the opening displays (or civil inattention) appropriate to the identification. Take-backs recycle back to a second

round of identification and/or access displays (or lack thereof).

Encounter 1 illustrates a misidentification in which DS misidentifies a stranger as an acquaintance: she thought that she knew the man in the back of the car. The double-take and take-back resulting from that misidentification are represented in *Sequence 2*.

*Sequence 2:*⁴

Double-take/take-back

I	II
A: A1 ₁ (A2 (A)) ₁ Φ [A1 ₁ (A2 (A3)) ₁]	
B: — — —	— — —

With the opposite sort of misidentifications, that of acquaintances as strangers, openings also recycle; and, in either case, the recycling involves the resorting of an individual into a more appropriate category. *Encounter 2* illustrates this type of misidentification.

- (2) EM is sitting in a downtown office building;
EM is visible to the street; he is staring at the street.
DS walks by the building; she recognizes EM.
DS smiles.
EM continues to stare into the street, without focusing on DS.
TB—DS (to self) He doesn't remember who I am.
—DS's smile continues until she is past the building, but it is no longer directed at EM.

The recycling in *Encounter 2* is also represented by *Sequence 2*.

Note, next, that there is a dependency between double-takes and take-backs. In

⁴ Explanation of symbols:

- I, II, III... recycling of sequences
() optional
()₁ if option was taken in first cycle, has to be taken in second; with 1', if option *not* taken in first, has to be taken in second
[] obligatory application of operation preceding brackets, to phases within brackets
 Φ delete
— no behavior relevant to opening sequences
{ } obligatory selection of one within curly brackets; subscripts indicate that whichever was chosen, the one on the same line in the next set of curly brackets should be chosen

Encounter 2, for example, if *EM* had recognized *DS* in time, her take-back would have become unnecessary and the encounter would have continued into hellos. This possibility, that a double-take by the already-identified individual can alleviate the need for a take-back by the identifier, is represented in *Sequence 3*:

Sequence 3:
Double-take

I			II		
A:	A1	A2 (A3)_1	—	A2 (A3)_1	
B:	—	—	B1	B2	B3

This dependency also is illustrated when an individual is forced to begin recycling with a take-back because the other individual has seemed not to recognize him, but then has to undo that take-back with the other's eventual recognition. Thus, what occurs is a negation of a negated display, or a redone display, as in *Encounter 3*:

- (3) *SW* is walking on a downtown street.
DS is walking towards *SW*.
DS recognizes *SW* and smiles.
SW stares past *DS*.
 DT—*SW* turns around quickly.
 TB—*DS* has already compressed her lips.
SW Hi, Debby.
 TB/TB—*DS* Hi! I didn't think you recognized me . . . warm out, isn't it?

That an individual's late identification and double-take can alleviate the need for the other's already begun take-back and create the need for a redoing of the display as in *Encounter 3*, is represented in *Sequence 4*:

Sequence 4:

Late double-take/take-back

I			II		
A:	A1	A2 (A3)_1	$\Phi[\Phi\text{—}\text{A2 (A3)}_1]$		
B:	—	—	B1	B2	B3

Note that the dependency between double-takes and take-backs is due to their timing: a prompt double-take can prevent the need for a take-back. Because of the nature of this dependency, certain ritual complications develop when the misidentifier has *not* done a double-take at all and, therefore, has not been able to take back his displays in time. This was the situation of *Encounter 1*, for example. *Encounter 4* also illustrates these complications:

- (4) *MS* is walking on a beach; walking towards her is a woman and an elderly man.
MS walks over to the woman.
MS Oh, Judy! Hello!
J (looks confused; looks at the man with her)
 DT/TB—*MS* Oh! You're not Judy! But you look just like her! And you're pregnant too!
J Everyone confuses us. She's my cousin!
 (to *MS* and man with her, while laughing and shaking her head).
MS Oh! Isn't that funny! Well, have a nice walk!

Two ritual binds requiring remedy are created in situations like these. First is an appropriateness bind. A ritual should not be initiated with a ritually unavailable person, and any attempts to complete one that has been initiated, only prolong what is inappropriate. The behavior of *MS* remedies this bind by closing the encounter in a manner similar to the way she might have closed it with the real *J*: her talk not only fills the empty interactional slots, it signifies that although *J* is the sort of person with whom a similar encounter would not have been offensive, *MS* is not the sort of person to exploit the inappropriate availability. Thus, identities are preserved, civil inattention is resumed, and a potential ritual offense is avoided.

Second, a completion bind is produced: an inappropriately initiated ritual has to be completed and closed, but cannot be completed efficiently when neither individual was ritually accessible in the first place. One remedy for this bind is to fill the empty slots with talk about the experience itself: since both individuals have participated in the inappropriate initiation of a ritual, talk about it creates a momentary bond which substitutes for the bond that was supposed to be signified by ritual. Such meta-comments may be spoken of as reflexive frame breaks (Goffman, 1974:502). Thus *MS* shares with *J* the only experience which is theirs to share, the misidentification.

Similar complications and binds develop with misidentifications of one personal acquaintance as another. Here, even more confusion results because although remedial work is required, so too are recognition displays and, although some have been offered, they have been misdirected. *Se-*

quence 5 represents this reordering, in which entire new sets of both identification and access displays have to be worked up by the misidentifier.

Sequence 5:

Misdirected double-take/take-back

A:	A1	I			Φ	[A1	II		
		\downarrow (A2	(A3)) \downarrow				\downarrow (A2	(A3)) \downarrow	
B:	B1				—	—			
		{B2	B3}						
		\downarrow —	— \downarrow						
		III							
		A1'	A2'	A3'					
		—	{—	—}					
			\downarrow B2	B3 \downarrow					

Thus, what occurs for *A* is a rerun of the entire sequence and, for *B*, a replay of his displays. It is not surprising to find that if a misdirected display of this type is never actually received by its misidentified object, then neither individual actually is forced into complicating ritual binds. Furthermore, when the real object of the display is present along with the misidentified object, the experience is even less anomic for the displayer, whose display can be easily redirected and his experience re-anchored. For example, *Encounter 5*:

- (5) *MM* enters kitchen, where several people are seated at a round table. *RS*'s back is facing her.
MM goes to *RS* from behind.
 DT/TB—*MM* halts abruptly and laughs.
MM goes over to *MS*, who is seated on the other side of the table.
MM Oh! y'know I thought that was you, Michael—
RS (laughing) from the back—
MM —an' I was gonna go, how y'doin'! (slaps *MS* on the back)
 Everyone laughs, and a general chorus of Ohhh!
MS Thanks, hon! Oh, she's a pleasure, isn't she!

In (5), *MM* can maintain the relevance of her initial identification and can anchor her display to its appropriate object. In short, *MM* is not confronted with an appropriateness bind. *MM* is momentarily faced with a completion bind, however, and she therefore couches her display to *MS* as a reflexive frame break: she reports on her misidentification and includes her greeting as the mistake that would have occurred had the misidentification not been remedied in

time. In terms of ritual organization, *MM*'s break invites *RS* to share in her experience and it signifies that even though a display had been withdrawn from him, it was not withdrawn because of his undesirability.

In sum, misidentifications require re-categorization of personal and social identities and the realignment of identifications and displays—biographical identification with identification and access displays, and categorical identification with civil inattention. Thus, they also require that the potential for an extended encounter after the opening, e.g., a conversation, be readjusted according to the appropriate identification. Recyclings, such as the double-takes and take-backs discussed here, adapt the structure of opening sequences to accomplish these tasks and, in so doing, they organize the web of association between acquainted and unacquainted individuals and maintain the social differences between personally and categorically identified others.

Alterations. Encounters can compete with each other for an individual's attention; and when competing encounters offer alternative sources of involvement, the encounter which wins out can nevertheless be affected, as can the one which loses. For example, when ongoing involvements prevent communication channels from being opened, and make a full ritual orientation unfeasible, opening sequences can be altered to fit the circumstance.

One alteration is for an identification and access display to be squeezed together in a *compression*, to signal that although individuals are neither technically nor ritually available for talk, it is not because they are ritually undesirable. *Sequence 6* shows compressions:

Sequence 6:

Compression

A:	A1	A2
		A3
B:	B1	B2
		B3

Passing greetings are sometimes compressions, and the brief display which occurs combines the features of identification and access displays, such that hats are doffed (MacCannell, 1973), hands waved or shaken (Schiffrin, 1974), heads nodded

or hellos grunted—all typical access displays—at the same time that one's personal identification of the other is displayed. Although found together empirically, the function of the displays remains distinct, so much so that if an individual passes an acquaintance and appears to have identified him without the accompanying social recognition, the acquaintance may feel insulted. Thus, great precautions are taken so as to ensure the other's appreciation of the difficult conditions under which deference was paid, and the encounter may be literally exhausted by compressions, as in *Encounter 6*:

- (6) *A* is riding on a bus.
B is on a different bus, which has pulled next to the first.
A waves, opens mouth in smile.
B waves, throws head back, smiles, opens mouth.
A nods, smiles, waves.
B moves mouth without vocalizing.
A nods, smiles.
A and *B* both twist around in their seats as the buses separate.
B waves.
A waves.
 The buses separate.

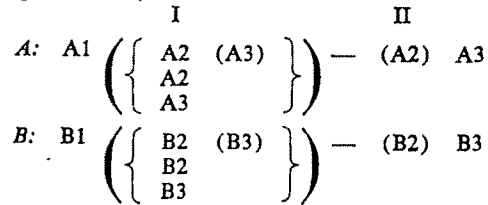
When the contour of an ongoing involvement changes, individuals suddenly may find themselves in contact after they had already signalled that it was unavailable. Such a shift in the potential for access may accompany a transition into a different activity system, as when (for example, Post, 1940: 22–3) one smiles at a friend in church, and then greets and talks with that friend out on the church steps. Alternatively, a shift in accessibility may reside within one such system. That the unavoidable lapse in contact was not a serious representation of the individual's intent is expressed by a redoing of the identification and/or the access display at the point of renewed access.

This type of alteration, a *negative transfix*,⁵ signifies that although availability was technically impossible and ritually unfeasible, such time apart is not indicative of the

individual's evaluation of either the other's self or of the relationship. *Sequence 7* represents this alteration as a time-lapsed recycling of a portion of openings:

Sequence 7:

Negative transfix



Because negative transfixes occur in situations in which participants are not immediately accessible to one another, they often follow compressions. Thus, they are frequent during parties, meetings and classes, when individuals can only display their identification by raising an eyebrow, smiling or pointing a finger, until further access and a fuller orientation is possible. They also may occur when a full exchange of displays *has* been performed, but is prevented from its conversational follow-up until some time later. Even when an ongoing conversation is interrupted, negative transfixes can reestablish the prior structure when it is resumed, as in *Encounter 7*:

- (7) Two women enter a bus together. The bus is crowded and only single seats are available.
A Here, Dear, you sit here (points to a seat and passes it).
B (mildly grumbling) Oh no. . . .
A I'll go back here.
 Several minutes later, a seat behind *B* is vacated.
 A man *C* has just boarded the bus and is about to sit down.
A (coming up from rear of bus, to *C*) Can I sit here? I want to sit near my friend.
C grumbles and moves to another seat.
A sits in vacant seat.
 NT—*B* (turning around in seat) Hi, Dear!
 NT—*A* (touching other's outstretched hand) Hi!

In sum, opening alterations modify an underlying ritual structure to fit it to overriding social circumstances: compressions and negative transfixes make sure that the right categorization, identification display and access display is noticed despite the lack of accessibility; and they assure that the potential for contact will still remain

⁵ Suggested by Erving Goffman in a personal communication. Note that when relationships are considered in their entirety, all greetings may be seen as negative transfixes.

open, despite its not having been immediately actualized.

Strategies. Yet another adaptation of the base form of opening sequences occurs when individuals utilize the structure of the opening as a foundation for individual expression and affectation. One sort of strategy, for example, is used when contact is infrequent or unexpected and is intended to elicit extra displays of involvement and recognition in order to openly attest to the strength and endurance of the relationship. Thus, displays such as backslaps and bear hugs performed *before* the other person has had time to identify the performer are *forced double-takes*, and they may be used when formerly intimate acquaintances who see one another infrequently come into sudden contact, or when intimate acquaintances who see one another frequently suddenly come into unexpected contact.

The effectiveness of such a strategy is due not only to the base structure of opening sequences, but to the structure of double-takes as well: the recipient of the display is forced into rapidly recycling and substituting a non-personal identification by a personal identification. Formally, then, this strategy is merely a replication of an ordinary double-take, except that it is speeded up as shown in *Sequence 8*:

Sequence 8:

Forced double-take

I				II		
A:	A1	A2	A3	—	—	—
B:	—	—	—	B1		
				B2		
				B3		

The ritual effect of forced double-takes can be seen most clearly when the strategy fails to be immediately effective, as for example:

- (8) *LS* and *DS* are at a party. They are dancing, as is everyone else in the room.
GM cuts in on *DS* and begins dancing in an exaggerated fashion, holding *DS* tightly, even though the music is fast.
DS looks over her shoulder at *LS*, who is watching.
LS All right, that's enough. (pushes *GM* away slightly)
LS and *GM* stand and look at each other.
GM (loud) Whadda' y' mean, that's enough?!
LS steps up a little closer to *GM*.
 Other people stop dancing; look at *LS*

and *GM*; someone says, "Take that shit outside man, c'mon. . . ."

GM (loud, forcefully) Sca-a-v-

DT—LS (loud) Gaton!

GM hugs *LS*.

DS (to other people watching) I think they know each other.

The hug between *GM* and *LS* in (8) is a response to the extent to which *GM*'s simulated display was a disguise of his real self, and it matches it dramatically, whereas a wave, nod, smile or handshake would have been so ritually inappropriate as to suggest that *GM*'s display *had* been aggressively intended and that *GM* was not really a friend.

Thus, without a display to match the drama of the stimulation, the encounter collapses, as in *Encounter 9*:

- (9) *LS* is sitting in a car in a gas station.

FG walks up to the car window.

FG Hey man, y'got some change?

LS (half looking at *FG*) Sorry, I don't have any.

FG (starting to walk away) Oh, okay, Scavo.

DT—LS (looks up) Oh! How y'doin'?

FG (continues to walk away) Oh, okay.

LS's response is insulting because it suggests that neither *FG*'s self, nor his relationship with *LS*, are real enough to be recognized independently of the situation which frames them and, even worse, that both can be shuffled about from encounter to encounter without maintaining their integrity—precisely what the forced double-take is intended to disprove.

Ritual devastations, not unlike those unintentionally produced by the tardiness of forced double-takes, can be intentionally created as a different sort of opening strategy. For example, a misidentification of an acquaintance as a stranger can be intentionally fabricated by simply deleting the opening displays altogether. The most devastating deletion, represented in *Sequence 9*, is a *cut*, the fabrication of a misidentification in the face of the other's opening display:

Sequence 9:

Cut

I				II		
A:	Φ [A1]	—				
B:	B1	B2	1(B3) ₁	Φ [B1	B2	1(B3) ₁]

Cuts destroy the inherent reciprocity of opening encounters and, as in *Encounter 10*, leave little room for remedial work:

- (10) *DS* is seated on a bus.
NK boards and stops by the empty seat next to *DS*.
NK Debby Schiffin?
 Cut—*DS* (looks up blankly, shaking head) (slowly, doubtfully) No. . . .

DS's response to *NK*'s identification display not only refutes his identification of her but, even more strongly, it claims that she cannot identify *NK* as an acquaintance. *NK* then has to take-back his display, but, because his take-back has to be performed without the other's support, the ritual dilemmas of appropriateness and completion are left unresolved. Thus, the cut necessitates *ad hoc* constructions of alternative frames of reference: wasn't it really her? isn't she feeling well?

Because cuts are so insulting, it is not surprising that they are infrequent (e.g., Goffman, 1971:75, fn. 17). However, "carefully not seeing" an acquaintance is quite frequent, and such *snubs* are represented in *Sequence 10*:

Sequence 10:
Snub

	I		II
A:	A1	— —	—
B:	B1	1(B2 (B3))1	1(Φ [B2 (B3)])1

Along with their increased frequency, snubs are not as ritually damaging as cuts. Although both destroy the inherent reciprocity of opening sequences, snubs do not blatantly fabricate a misidentification as do cuts. Thus, rather than countering the other's display with a denial, they merely ignore it, thereby making alternative reframings of the experience more readily available: after all, that "he didn't see me" is borne out by the evidence.

Snubs are also more flexible than cuts, so that they can easily be integrated into sequences of varying form, even those whose initial identification has seemed to go smoothly, as when, for example, the second half of a negative transfix is deleted. Thus, individuals can snub one another at crowded parties by displaying their identification, but making no effort towards ac-

cess. Indeed, snubs are so flexible that they can even span boundaries between encounters, as when two politicians can snub a foreign dignitary by not providing him with a traditional reception when he arrives in their country several years after an anti-Semitic remark (see *New York Times*, 3/26/75). So not only can snubs be explained away by situational evidence, but they can be reinterpreted by the insulted as having been due to the unavailability of access, not the undesirability of selves.

A strategy even less ritually devastating and more protective of selves, but with a similar result—the avoidance of prolonged and undesired contact—is a *fast-start*. Fast-starts control the key and tempo of the opening encounter: by not waiting for the other to begin, individuals can use them to establish the temporal parameters of the opening sequence and assure that an extended encounter will not result. *Sequence 11* represents a fast-start, and *Encounter 11* illustrates one:

Sequence 11:
Fast-start

	I		II	
A:	—	—	A1	A2 A3
B:	B1			
	B2			
	B3			

- (11) *DS* (while walking by *JB*) Hi, how are you?
JB (looks up surprised) Oh, hi.

Note that formally, fast-starts are the reverse of forced double-takes (compare *Sequence 8*) and that ritually, too, they are a play on that strategy: they include the element of surprise, without the follow-through of contact.

In sum, opening strategies show how a base ritual structure can be adapted for the expression of alternative meanings. Although interactionally, opening strategies are a play on the base structure of opening encounters, they function at the same time as a vehicle for individual expression. Thus, in terms of social order, they help to sustain the fiction that the individual is a unique, creative personality, and to show that social organization is flexible enough to allow such individuality to flourish.

Opening Encounters, Interaction and Social Order

In this paper, I have suggested a framework for opening encounters. As an extension of this, I now suggest that openings be considered as encounters in and of themselves, i.e., that cognitive recognition, identification displays and access displays not only open encounters, but, by themselves, form an *opening encounter*. This view requires a slight rethinking of the traditional definition of encounters which Goffman (1961:7) has argued, i.e., that encounters occur when "people effectively agree to sustain for a time a single focus of cognitive and visual attention."

First, consider openings that are followed by encounters. Such openings precede a spate of focused activity, e.g., a conversation, party, encounters in the traditional sense, and they appear to continue without a break into that activity. The focus of attention and involvement for participants shifts, however, from the recognition displays of openings to the business at hand and, in fact, there even seem to be transition devices to smooth the flow from one frame of activity to another (Sacks, 1967). When such a shift is so regular a part of interaction, we might well speak of shifting encounters. Thus, some opening encounters, those with opening greetings, are *followed* by other encounters.

Openings with passing greetings that are not followed by other encounters suggest a different modification of the definition of encounters. Some passing greetings constitute the only focus of their participants' attention, but others occur during a more ongoing and dominant involvement. I argue, however, that such greetings are still a focused activity despite their subordination to other activities. Thus, some opening encounters *co-occur* with other encounters.

These considerations suggest, first, that the individual's capacity to split himself into several selves within one interaction might be matched by a capacity to use those selves to participate in several encounters simultaneously, or in encounters that shift from one moment to the next; and, second, that alternative sources of involvement within one encounter might rather be seen as signs

of competing encounters. With this revised and expanded description, all opening sequences can be considered as encounters.

Separating a technical aspect of interaction from a ritual aspect makes the structural parallel between opening encounters and other encounters even clearer. A technical side shows itself best in the apparatus, or machinery, by which focused interaction is organized.⁶ For example, mutual identification by participants, open communication channels, signals of mutual availability and a turn-taking mechanism are formal technical prerequisites of interaction. The technical aspect is concerned mostly with formal prerequisites such as these; but because focused activity requires *continued* dramatization of particular identities and relationships, a ritual aspect, concerned with symbolic organization and meaning, pervades both the form and content of the interaction for as long as it is sustained. Turn-taking in conversation (an aspect of form), for example, may have ritual meaning (Albert, 1972), as may the choice of locatives in place descriptions, a feature of content (Schegloff, 1972a). Conversational closings have both formal and ritual functions (Goffman, 1971; Schegloff and Sacks, 1973). In short, the technical aspect establishes the formal structure of focused interaction, and the ritual aspect overlays various parts of that structure with symbolic meaning about selves, relationships and situations.

In opening encounters, technical aspects of interaction are also overlaid with symbolism and ritual meaning, through the progressive change from a cognitive identification and its display into a ritual display of social recognition. Opening encounters, then, are a micro-structural representation of focused interaction—a kind of dramatization in miniature—which can bracket an upcoming encounter by framing precisely that structure which is about to be required: identification displays provide a technical bracket, and access displays provide a ritual frame of reference.

⁶ Compare Goffman's (1976) system requirements.

Previous analyses of greetings (Callan, 1970; Eibl-Eibesfeldt, 1972; Firth, 1972; Goffman, 1971; Goody, 1972; Youssouf et al., 1976; Irvine, 1974; Kendon and Ferber, 1973; Krivonos and Knapp, 1975; Sacks, 1967; Schegloff, 1972b) are correct in recognizing these bracketing and framing functions. However, because such analyses incorrectly identified the greeting as the only opening display, and not one display within an opening encounter,⁷ they were unable to see that openings *are* encounters; and that it is not just the greeting, but also the dependency between technical and ritual aspects of interaction which are represented in opening encounters which function to open more extended encounters.

Similarly, because previous analyses of greetings did not analyze openings as encounters, they were unable to appreciate their structural significance on a larger scale. As students of modern, urban society have noted, the transformation from a more primitive, rural folk society has resulted in a shift in emphasis—from personal relationships based on similarities to impersonal relationships based on rationality and division of labor, i.e., differences. Further, as Goffman (1967:95) has noted, the same transformation has resulted in a secular world which, however,

is not so irreligious as we might think. Many gods have been done away with, but the individual himself stubbornly remains as a deity of considerable importance. He walks with some dignity and is the recipient of many little offerings.

One way in which individuals remain deities despite the de-emphasis of personal relationships, then, is for human ritual to be so pared down that all of its work can be signified by no more than a brief passing glance and a smile, that is, a passing greeting, which, without actualizing an extended period of increased access, suggests that it would have been so, if it could, and it will, when it can.

⁷ Exceptions are Kendon and Ferber (1973), whose treatment is almost totally descriptive, and Krivonos and Knapp (1975) whose approach is experimental.

Recognition, Interaction and Social Order

Cognitive recognition, social recognition and their display are crucial not only to opening encounters, but to ongoing encounters as well; so important in fact, that a large part of social organization is concerned with making recognition and identification easier to accomplish.

Consider first, the role which recognition plays in sequences of ongoing talk or conversation. One reason for the coherence of talk is that both speaker and hearer recognize each other as valid partners in the interaction and, beyond that, both recognize, albeit implicitly, a set of culturally, situationally and topically relevant standards of conversational and interactional behavior. Thus, this sort of cognitive recognition is not unlike a "baseline" requirement for conversation, and the structure of ongoing talk includes slots in which hearers can display their recognition of the speaker as one who shares the standards, or experiences related to those standards. Through such displays as mm-hmmm's, head nods and smiles, for example, hearers show their appreciation of the other's presentation (Dittmann and Llewellyn, 1968; Jefferson 1973; 1975; Yngve, 1970).

Other than attesting to shared standards, enclosed identification displays have two more immediate interactional functions. Ritually, they express that the hearer, although not as visibly or audibly involved as the speaker, is not inattentive during the speaker's turn at talk and is sympathetic with the speaker's intentions. In this sense, these displays are similar to compressions and negative transfixes: they signify that the hearer's comparative passiveness does not indicate disdain for the speaker or for the relationship. Technically, enclosed identification displays differ from alterations in opening encounters because they respond to a situation in which technical conditions *are* being met (they maintain the technical requirement of conversation that only one person speaks at a time [Duncan, 1973; Schegloff and Sacks, 1973]), because they occur in the back channel (Duncan, 1973; Yngve, 1970) and are neither a claim for a turn nor a disruption of the speaker's turn.

Access displays also are enclosed within conversation: they occur when the speaker recognizes the auditor's right for a turn at talk and when he relinquishes his own turn (see, for example, Duncan, 1973; Sacks et al., 1974). Exchanging turns at talk, then, is a display of social recognition, i.e., "the process of openly welcoming or at least accepting the initiation of an engagement . . . [and] the according of a special role within an engagement" (Goffman, 1963:113). Thus, the according of the special roles of speaker and hearer in conversation is the same sort of dramatization: it is a display of deference for the identities which each assumes in the interaction and for the division of labor and reciprocity of access which the conversation has established.

Consider next the role of recognition and recognition displays in relation to social organization. I have argued that recognition and its displays are functional in both opening and ongoing encounters and in signifying which differences between individuals are socially meaningful. We might expect that the task of identifying, differentiating and segmenting people into easily recognizable categories (biographical or social) would be made easier somehow, for example, by multiple cues signifying the same meaning, or by cues which could be immediately and unambiguously linked with a meaning. Thus, communicative redundancy (multiple cues) and stereotyping (unambiguous meaning) can be functional on both interactional and structural levels.

Thus, not only are such communicative structures relevant on a micro-level of social organization, such as face-to-face interaction, but they are relevant on a macro-level as well, such that phenomena as seemingly diverse as a centralized articulation of a back vowel (e.g., Labov, 1972), a particular style of handshake, urban block parties in ethnic neighborhoods, or the joining together of citizens in national birthday celebrations, have more in common than might at first be suspected. All are ritual signs of identity which symbolize and help to maintain the structural differentiations of modern social organization. Perhaps an appreciation of such communicative and functional similarity can provide

an often sought-for structural link between a micro-sociological concern for face-to-face interaction and a macro-sociological concern for social structure.

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ATTRIBUTION AND IDENTITY CONSTRUCTION: SOME COMMENTS *

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This paper presents a tentative theoretical framework for investigating the process of identity construction. It is suggested that, at any given time, the perceiver's construction of the other's identity may be characterized by either one of two inferential perspectives. "Viewing the other qua performer" involves the attribution of role-relevant qualities on the basis of observing behavior as role performance. "Viewing the other qua person" entails the linking of observed behavior to psychological causes which have their origin in the other's personality. When the identity-construction process is envisaged in the particular case where the other is a stranger to the perceiver, an initial branching is postulated. This branching is constituted of two inferential paths (corresponding to the two inferential perspectives) potentially open to the perceiver in his construction of the other's identity. Several factors that may be invoked to account for, or to predict the nature of the perceiver's inferential activity are discussed; these include: the perceiver's needs and objectives, his expectancies concerning the likelihood of events to be encountered, his preference for certain cues in making inferences, the consequences implied by alternative categorizations and their relative value to the perceiver, the need for adjusting to situational requirements, and the kind of cues exhibited.

A number of sociologists and psychologists, in spite of important differences in their research interests, tend to concur with one another in viewing identity construction as an act which involves the placement of objects (human, physical, etc.) and events into categories (see, for example, Bruner, 1957; Berger and Luckmann, 1966).

Identity, however, is commonly conceptualized in a dichotomous manner, i.e., a distinction is made between social and personal identity in terms of type of categories used (occupational, kinship, traits, etc.) Such a distinction does, in fact, reflect more than just a differentiation among category types. As Sarbin and Allen (1968:550) point out, while "some writers imply that identity is an unfolding of personal dispositions, those with a role-theoretic orientation would declare that identity is built up from interactions with occupants of complementary statuses."

In the perspective adopted in this paper, identity refers to an integrated system of cognitions, and no qualitative precedence

is granted *a priori* to cognitions relative to psychological characteristics over cognitions about social attributes, or vice versa. Hence, the present approach treats as problematic and researchable the ways in which categorizations in terms of social attributes and psychological characteristics contribute, in a given situation, to the identifying act. The tentative theoretical framework proposed below constitutes a means of studying the identity-construction process that does not involve any assumption as to which category types *necessarily* form the organizing core around which identity is elaborated. Instead, the framework focuses the attention on the critical problem of specifying under which conditions certain categorizations are most likely to take place.

With Bruner (1957:148), identity is viewed here as representing the range of inferences about properties, uses and consequences that can be predicted from certain cues acting as signals of category membership. In this view, which is highly consistent with Mead's (1934) treatment of object as plan of action, the other's identity is not intrinsic to him. Identity is constructed in a tentative way by the perceiver "in-situation," and it may undergo

* I am deeply indebted to Professors D. P. Cartwright and T. M. Newcomb for generous advice and criticism.

subsequent revisions through an ongoing categorizing process (Turner, 1968).

Furthermore, in the context of this paper, identity construction is considered as a matching process between observed behavior and categories made available to the perceiver through social processes. Consequently, cases in which identity construction does not involve observation of behavioral events are not dealt with.

Behavior observed in a particular social context supplies information to the perceiver concerning the attributes that might be imputed to the other. Identity construction invokes attributions.

Interpersonal attribution processes play a central role in both Heiderian (Heider, 1958; Jones and Davis, 1965; Kelley, 1967; 1972) and interactionist (Mead, 1934; Turner, 1956; 1962; 1968; Strauss, 1959; Goffman, 1959; 1961; McCall and Simmons, 1966) positions. This similarity of scientific interest generally has been overlooked by most researchers who belong to either social psychological tradition (a notable exception being Alexander and Epstein, 1969). As a result, both approaches have remained largely alien to each other as evidenced by a scarcity of cross-references. In a way, the present contribution may be seen as an attempt to reduce the gap between these theoretical positions.

In the first part of this paper, attention is focused on some specific aspects of the Jones and Davis (1965) analysis of the attribution process in person perception which constitutes an influential attempt at systematic theorizing in the Heiderian approach (see, for example, Hastorf et al., 1970; Shaw and Costanzo, 1970; Steiner, 1970). More specifically, attention is centered on the problem of "personal" and "role" contributions to person perception as well as on the manner in which Jones and Davis portray the perceiver.

The second part is devoted to a brief consideration of Turner's conceptualization of role-taking and to contrasting Turner's representation of the perceiver with that of Jones and Davis. The critical comments formulated in the first two parts serve to introduce the tentative framework in the third one.

It should be borne in mind that these

critical comments are not criticisms of the theories put forward by these authors. Neither Turner nor Jones and Davis address themselves to the question of identity construction per se. However, they do deal with concepts and processes that are of central importance to this subject matter; the role concept and the representation of the perceiver's situation are cases in point.

The Jones and Davis Conceptualization of In- and Out-of-Role Behaviors

Attribution theory (see Jones et al., 1972 offers a broad survey of the field) has developed within social psychology primarily around Heider's (1958) influential book, *The Psychology of Interpersonal Relations*. For Heider, man understands his world by referring transient behavior and events to relatively stable underlying conditions or "dispositional properties" which force - people and events to manifest themselves in certain ways under certain circumstances. Central to Heider's position is the assertion that man perceives events as being caused. Of critical importance for the interpretation of the social world is "the separation of the factors located in persons, and those that have their source in the environment of these persons" (Heider, 1958:297), i.e., the distinction between perceptions of internal and external causality. Attribution, "the linking of events with their underlying conditions" (Heider, 1958:89) involves, thus, the use of information in making causal explanations.

Making some simplifying assumptions in Heider's model, Jones and Davis (1965) analyze the process of attribution in person perception by focusing on the specific attributions to the person (internal causality) used to explain behavior. The authors define the task of the perceiver as one of inferring another person's intention from his action; the inferred intention may, in turn, be regarded as indicative of the existence of a dispositional property. By implication, the personal property ultimately can account for the other's action.

The dichotomy person-environment that is found in Heider's model becomes,

in the Jones and Davis analysis, an opposition of polar factors such as personal-social, deviant-typical.

Actually, the opposition of such factors results from the manner in which the perceiver's orientation toward the other and his social environment is conceptualized. Jones and Davis seemingly attribute to their perceiver a proneness to divide the conduct of the other into what Goffman (1961:152) calls a profane and sacred part. The profane part is attributed to the obligatory world of social norms; it is formal and exacted by society. The sacred part has to do with "personal" matters, with what the other is "really like" as a person. In this view, the perceiver is portrayed as a naive psychologist for whom personality is a closed system which society "impinges upon" but does not structure. His task is defined as that of inferring the other's personal dispositions.

For the perceiver who is directed toward the other and his psychological processes, and for whom personal is opposed to social, the other's *deviant* behavior constitutes an indispensable datum for inferring personal characteristics. However, such inferences can be made only to the extent that the perceiver views deviant behavior as stemming from the operation of psychological processes immune from any "social" contamination. Otherwise, the possibility exists that the other's behavior, instead of being seen as a "personal" manifestation of irreverence toward some norm, might be viewed, say, as expressing the other's commitments to some counternorms of a particular group. Hence, it becomes understandable why Jones and Davis place the perceiver, or the other, or both, in situations where the perceiver views his social environment, or the environment in which the other's action takes place as a coherent and well-integrated social system. It is in those *atypical* situations (Buckley, 1967), in which clear knowledge of consensually-defined and shared norms prevails, that the perceiver is virtually compelled to explain deviancy in psychological terms. The role context devised by Jones et al. (1961) illustrates the point.

The concept of role espoused by these researchers is Lintonian (Linton, 1936) in

that they consider as given the existence of distinct roles characterized by clearly established role expectations. Their treatment of role which "stresses the shaping of individual responses by social expectations or externally imposed norms" (Jones, Davis and Gergen, 1961:302) entails a strong cultural and mechanical determinism. It is under such conditions, which presuppose rigid role boundaries, that in-role and out-of-role behaviors can unequivocally be distinguished; and according to Jones and Davis, (1965:234) "behavior which conforms to clearly defined role requirements is seen as uninformative about the individual's personal characteristics, whereas a considerable amount of information may be extracted from out-of-role behavior."

Consistent with their treatment of role, these authors tend to represent in-role behavior as actions mechanically enacted in identical ways by all accepting performers. In this manner, they exclude virtually all possibility for in-role behavior to be informative about the other's personal attributes. As a result of their oversimplified approach to role functioning, questions such as that raised by Hasterof et al. (1970:90), as to how people communicate true positive feelings when positive behavior is role-required, cannot be answered. Furthermore, as in that theoretical approach where attributions of personal dispositions are reduced to attributions of deviant characteristics, it would appear that only deviant behavior could contribute to identity formation.

At this point, it should be borne in mind that different people may, indeed, enact a given role in distinctive and expressive ways which may be informative about personal characteristics. Furthermore, roles involve expectations for personal qualities as well as for behavior and, therefore, contribute to definitions of identity. In a similar vein, it is worth noting that positions are socially evaluated, for example, in terms of their utility, prestige and legitimacy and they differentially evoke interest, respect, hostility, etc. Consequently, for any individual, the mere fact of being the incumbent of a particular position entails that personal characteristics are *ipso facto* attributed to him.

In postulating the personal-social dichotomy as having the thickness of reality for the perceiver, Jones and Davis bypass the important problem of identifying the antecedent conditions under which the perceiver does oppose personal to social. There are, indeed, countless situations in which people do not oppose these terms in any radical way. For instance, in the Steiner and Field (1960) study, expressing a segregationist position could be regarded by subjects as indicative of "personal" dispositions, although such a viewpoint is "social" in nature, even for college students.¹

These brief considerations deal with common situations in regard to which the Jones et al. (1961) conception of role has limited validity. In numerous cases, the other's conduct does not seem to lend itself to immediate categorization in terms of personal and social aspects, and those characteristics which may be inferred from in-role behavior are not necessarily less "personal" than the ones that may be imputed from any other behavior. From the *perceiver's vantage point*, "in-role" behavior may be informative about the other's personal characteristics, and its informational value may even be crucially important to the perceiver *in situation*.

The conceptualization of "out-of-role" behavior can be examined along the same lines. According to Jones, Davis and Gergen (1961:303) out-of-role" behavior, or departure from role expectations "suggests a pattern of motivation and skill that is at variance with specific role requirements." Out-of-role behavior emerges because personality overrides role expectations and, consequently, conveys information about the other's personal characteristics.

It should be noted, though, that for

out-of-role behavior to be expressed, the other must be participating in role relationships. From the perceiver's viewpoint, out-of-role behavior may, indeed, achieve meaning by virtue of its being embedded in a role context. In such cases, instead of being viewed as an irrepressible manifestation of the other's "true self," such a behavior may be apprehended in contemporary relational terms. For example, in clear contrast with the Jones et al. (1961) contention, according to which out-of-role behavior should generate confident inferences about personal characteristics, subjects in Ring's (1964) study did not know what to make of behaviors departing from status expectations.² Moreover, a number of subjects tended to interpret such out-of-role behaviors so that they could become appropriate or meaningful in terms of contemporary role relationships (e.g., as strategy).

By virtue of the fact that out-of-role behavior has to be performed while the other occupies a position, the perceiver may possibly come to make judgments from such a behavior concerning the other as a particular *performer* of the role. Accordingly, the "pattern of motivation and skill" at variance with specific role requirements would then generate inferences about the other *qua* performer instead of being viewed as the sacred part of his conduct (Sarbin and Allen, 1968:490). In a related perspective, it is conceivable that the perceiver might regard the other's behavior as suggesting that the other has some measure of disaffection from, or resistance against his role. Such a separateness between the individual and his putative role is called "role distance" by Goffman (1961:108). In this case, the other may be viewed as actively withdrawing from the self-image which is available for anyone entering the position in question.

¹ In that study, groups of three college students were asked to discuss the question of social desegregation. In half of the groups, a confederate of the experimenter was publicly assigned the role of a "typical Southern segregationist"; in the other groups, no roles were assigned. Results showed that both Ss in no-role groups and Ss in role-assigned groups judged the accomplice to be a strong segregationist. Ss were not found to reject him more completely in the former groups than in the latter ones.

² Ring investigated situations in which a high-status person (or a low-status person) presents suggestions, opinions and a request for a favor to a low-status person (or a high-status person) who responds either favorably or unfavorably. Results suggested that Ss viewed the compliant high-status person and the non-compliant lower-status person as behaving in a way inappropriate to their status, which generated uncertainty and suspicion.

For instance, the other's behavior ("out-of-role" in the Jones et al. [1961] perspective) might be perceived as reflecting his attachment or commitment to another role, and out-of-role behavior (role A) would then appear as in-role behavior (role B).

Actually the issue is not whether role distance *is* out-of-role behavior but, rather, whether the perceiver in-situation views out-of-role behavior as, say, role distance. How the perceiver comes to define such behavioral manifestations the way he does is an empirical question which cannot be studied experimentally unless some provision is made to record perceivers' own definitions, be they psychological or nonpsychological in nature. To illustrate, it might be tempting to speculate on the number of subjects assigned to the Submariner-Inner condition of the Jones et al. (1961) study who did view the interviewee's behavior as an expression of his inferred antimilitarist position.³ Since, in that study, subjects were presented only with impression rating scales on which they had to indicate what they thought the interviewee was "really like" (i.e., forcing subjects to view out-of-role behavior in psychological terms, that number remains unknown (see also Alexander and Epstein, 1969).

These remarks, formulated with regard to the Jones and Davis approach to in-role and out-of-role behaviors, point to the need for an inclusive role concept. Such a concept should be compatible with the fact that in-role behavior may provide the perceiver with information about the other's personal characteristics, while out-of-role behavior may be regarded as

role performance. Turner's conceptualization of role appears to meet that need; some very specific aspects of his approach to role-taking are discussed in the following section.

Turner's Approach to Role-Taking

Turner (1956) adopts an action framework in which he examines the dynamic interrelations among the elements operating at a given time to determine action. He argues that role-taking designates a kind of relationship which may be assumed contemporarily toward a relevant other in the context of an act in progress. In this view, role-taking is a process of looking at another's behavior by viewing it in the context of a role imputed to that other.

This process of placing specific actions in the context of the entire role of which they are assumed to be a part requires that the role of the other be either identified or constructed. Turner (1956:317) cites several ways in which the individual may conceive the other's role among which the following one is particularly relevant to the present discussion: role-taking "may proceed from observing a segment of behavior to identifying the feelings or motives behind the behavior. . . ." Thus, Turner views as a central characteristic of role-taking the process of discovering and creating consistent wholes out of behavior, i.e., the grouping of behavior into units or roles. Roles become points of reference for placing interpretations on, and making evaluations of individual actions.

In Turner's treatment, the concept of role is conceived more inclusively than in the Jones et al. (1961) approach. By role, Turner (1956:316) means a collection of patterns of behavior which are thought to constitute a meaningful unit and deemed appropriate to a person occupying a particular status in society, occupying an informally defined position in interpersonal relations (e.g., compromiser) or identified with a particular value in society (e.g., patriot). The term "appropriate" purposely is left without referent by Turner who stresses the fact that what is regarded as appropriate will vary depending upon

³ In that study, Ss listened to recorded "job interviews" knowing that the interviewee had been instructed to make a good impression on the interviewer in order to qualify for the job. Some Ss heard the interviewee scrupulously play his assigned role (i.e., he presented himself as "other-directed" when applying for the job of submariner or as "inner-directed" for the job of astronaut) whereas others heard the interviewee respond to questions with statements indicating he lacked the personal qualifications required for the job (the submariner-inner and astronaut-other conditions which thus involved out-of-role behavior). Results indicated that out-of-role interviewees were perceived to be revealing their true character more than in-role interviewees.

the vantage point of the person formulating the role conception. In this broadened version of the role concept, Turner expands the limits of the domain to which it can be applied. "Emergent" roles (Newcomb et al., 1965), interactive roles (McCall and Simmons, 1966), situated roles (Goffman, 1961), conditioning roles (e.g., sex, age and ethnic group roles) can now be considered too, in addition to roles, as traditionally handled by role analysis (organizational, occupational roles).

Information derived from observing behavior thus is regarded both in the Turner approach and in the Jones and Davis analysis as being processed to infer intentions, purposes or motives. Whereas in the former treatment the assignment of intention is considered as a phase in the identifying process of the other's role, in the latter it is asserted that the attribution of intention may be used to infer "those underlying stable characteristics toward which the perceiver presses in attaching significance to action" (Jones and Davis, 1965:222).

Basically, behavior becomes meaningful action in ways that differ depending upon how the perceiver's perspective is conceptualized. In Turner's (1962:24) approach, attention is focused on the immediate functionality of imputations of role attributes for a social act in progress. The perceiver is represented as a kind of naive role analyst to whom behavior makes sense when "a series of actions is interpreted as indicating that the actor has in mind some role which guides his behavior." Whereas Turner's (1962:22) emphasis is placed on a postulated "tendency to shape the phenomenal world into roles," Jones and Davis (1965:220) primarily stress the importance that people attach to understanding persons as causal agents. As noted above, they portray the perceiver as a naive psychologist whose cognitive task "involves processing available information about, or making assumptions about, the links between stable individual dispositions and observed actions."

Such a difference in the conceptualization of the perceiver's perspective does not stem logically from the adoption of

different levels of analysis by these authors. Actually, what is at stake is the phenomenal experience of the perceiver, and how behavior becomes meaningful action is an empirical question. The perceiver is in-situation and the manner in which he defines his position there, his problems at hand, affects his inferential activity. Hence, the question as to whether some behavior is informative cannot be tackled properly unless the vantage point of the perceiver in-situation is taken into consideration. In accordance with this general assertion, a theoretical scheme is outlined in the following section. It provides a basis for studying the identity construction process which is broad enough to encompass potentially-diversified inferential activities.

Identity Construction: A Tentative Theoretical Framework

Anticipating what follows, it is necessary to indicate that the present theoretical framework does not constitute some kind of "resolution" between the Turner and the Jones and Davis positions; nor is it a conversion of the Jones and Davis conceptualization of in-role and out-of-role. The problem that the framework addresses is not what the perceiver is or is not able to infer about the other's personality but, rather, under which conditions the perceiver in-situation comes to view the other as a performer of role(s) or as a person endowed with distinctive personality characteristics which explain action.

According to the framework, the perceiver's perspective is treated as variable in time and across situations. *At any given time*, the perceiver's construction of the other's identity is characterized by either one of two perspectives: "performer" and "person" perspectives which will be explicated below.

It is because of the definite influence which the early stages of the categorization process have upon the subsequent ones that, here, identity construction is examined in the particular case where the other (O) is a stranger to the perceiver (P). However, the framework does also apply to situations in which O is not, or is no longer a stranger to P.

The entire framework rests upon the following hypothesis: It is assumed that P may *initiate* his construction of O's identity by following either one of two inferential paths which form an "initial branching." The initial branching constitutes a dichotomization of the *early stages* of the identity-construction process made in terms of a differential emphasis placed upon certain cues selected and used by P to make inferences. (Categorizations may proceed on the basis of information derived from observing behavior in a given situation as well as prior information such as knowledge about the social context in which O is viewed.)

Along the first inferential path of the initial branching, P may be said to be viewing the other *qua* performer when behavior is looked at in the context of a role imputed to him. O's identity, as P constructs it, derives essentially from the whole scene of O's actions as they are seen meshed in specific role relationships in a particular interactive situation. Viewing the other *qua* performer presupposes that O's role has been recognized or constructed. Basically, P may arrive at identifying O's role from locating O's position to inferring its role, or P may proceed from observing some behavior and then inferring O's role of which that behavior is assumed to be a part. In the latter case, in trying to discern O's role, P attributes certain motives in the light of which O's sequences of behavior appear as tied together by a certain consistency of direction (Turner, 1956; McCall and Simmons, 1966).

Role imputation is variable as to the number of different roles in a person's repertoire and, consequently, P cannot place O's actions in a role context unless his role repertoire enables him to interpret O's actions as the manifestations of a role configuration. It must be stressed that, in the present perspective, identifying O's role is a necessary but not sufficient condition for viewing the other *qua* performer. O's role must not only be recognized or constructed, but it must also invoke role attributes used for placing O into identity categories.

In judging O's specific performance, P views O's actions against the background

of more or less clearly-defined role expectations held by him. Not only is O expected to perform certain acts, but also he is expected to perform them in specified ways. Following Sarbin and Allen (1968), P may be represented as making inferences as to the appropriateness, propriety, and convincingness of O's specific performance. Moreover, P may come to regard O as being attached to his role, or even embraced by it (Goffman, 1961). Such judgments contribute importantly to identity construction since role-appropriate personal qualities are effectively attributed, i.e., the virtual self that is implied in the stranger's role is imputed to him.

In contrast, the stranger may be seen as portraying a character whose qualities his performance is designed to evoke. Such an inferential outcome reflects a dramaturgical conceptualization of O's behavior. Seen as something enacted (role playing), behavior may appear as being performed with guile ("playing at") or good faith, with or without awareness. In this case, O's performance enables P to assess the stranger's attachment to his role and to what extent he withdraws from the image generated for him by his mere participation in role relationships. It must be emphasized that, regardless of how substantial O's role distance may appear to P, O is seen as enacting a role. Otherwise, P cannot possibly view the other *qua* performer, and his identity construction of the stranger proceeds along the other inferential path of the postulated initial branching.

Along the second inferential path, P may be said to be viewing the other *qua* person, i.e., as an organized entity characterized by a unique configuration of personal attributes. What characterizes P's inferential activity, when he views the other *qua* person, is the assignment of observed behavior to psychological causes internal to O (Heider, 1958; Jones et al., 1972). However, when O's behavior appears attributable to the operation of external forces, P's construction of the stranger's identity is blocked if, indeed, he views him *qua* person. If, however, P regards O as the causal agent, he then may attribute to him certain personal char-

acteristics which may subsequently lead P to infer additional dispositions (Schneider, 1973).

So far, the discussion has focused exclusively on the two types of categorizing possibilities open to P without specifying the conditions under which identification takes place. Indeed, construction of O's identity is not an isolated form of behavior. Therefore, the process cannot be studied unless factors which determine P's perspective, at a given point in time, are defined.

Determinants of Branching

Numerous factors may acquire primary importance in channeling P's inferences along one branching path rather than the other; their potency varies as a function of the particular circumstances under which O's identity is being constructed. The topic under examination is so complex that the present discussion has to be restricted to considering a selected sample of determinants. The principle of selection is predicated on the proposition that constructing the other's identity involves an act of categorization. This proposition is consistent with Bruner's (1956; 1957) work on inferential activity on which the discussion draws substantially.

The initial branching basically reflects a differential use of cues and categories in identity construction. Therefore, factors affecting cue utilization (which cues will be more impressive) and category accessibility (which categories will be more readily available for use) are potential determinants of branching. To illustrate, several of the factors are: P's needs and objectives; his expectancies concerning the likelihood of events to be encountered; the consequences implied by alternative categorizations and their relative value to P; P's preference for certain cues in making inferences; the need for adjusting to situational requirements; and the kinds of cues exhibited.

The channeling influence that P's needs and objectives may have on his inferences derives from the functional relationship which links perception and action (Mead, 1934; Hastorf et al., 1970). Need and interest states force P to be selectively

tuned to seek out relevant information and to perceive those events and objects that relate to achieving his goals. The emerging process of identity construction provides P with direction for instrumental activity in that, for instance, it enables him to anticipate the other's action and to adjust his own behavior appropriately.

In this perspective, it can be hypothesized that, when P's performance of his own role in a given interactive situation is contingent upon the stranger's role, P will tend to view the stranger *qua* performer and will process information accordingly. The demands of his role presumably will determine the selection of particular aspects of the stranger's role behavior for emphasis. It is worth noting that, under such circumstances, the identifying act loses most of its instrumental value outside the bounds of the particular interactive situation. In contrast, the following hypothesis refers to categorizations having greater trans-situational generality. This hypothesis posits that P initially will tend to view the stranger *qua* person whenever he anticipates interacting with the stranger in a situation that does not involve the same type of activity system as the one in which the stranger is being observed. It can be expected that P's construction of O's identity will involve categorizations in terms of personality traits which subjectively constitute generalized response dispositions.

In an experiment reported elsewhere (Guiot, 1975), a series of propositions derived from these two hypotheses was tested. Each subject was instructed to listen to a taped conversation, then to send a message to his teammate concerning the stranger on the tape. The message was composed by selecting four sentences from a list of sixteen pretested sentences describing the stranger from either the "person" perspective or the "performer" perspective. Two independent variables were manipulated. The first was "type of anticipated interaction" (the teammate was going to work with the stranger, either in the "same" type of situation as the one on the tape or in one totally "different" from it). The second was "role distance" (in the taped conversation, the stranger either manifested disaf-

fection from his leader role or did not). It was hypothesized that, irrespective of role distance variations, type of anticipated interaction would induce subjects in the "same" conditions to view the stranger *qua* performer, while those in the "different" conditions would view him *qua* person. The data indicate that type of anticipated interaction was the only significant factor. No significant interaction between this variable and role distance was found, while predictions concerning inferential perspective proved to be correct in all conditions except one.

In the situations just considered, the channeling of P's inferences results from the necessity to adopt a state of preparedness for coping with O and to make interaction more predictable. Similar channeling effects due to P's objectives may result in a large variety of contexts of purposive activity. To illustrate, two cases involving very different types of objectives are briefly considered. The first is one in which the channeling of inferences is a consequence of a technique of interpersonal control used by P. In this case, identity construction can be viewed as a process through which P attempts to exercise control over O by casting him into an identity type that is congruent with P's goals or values.⁴ Most "altercasting" (Weinstein and Deutschberger, 1963) seems to require the adoption of the "performer" perspective (casting O into a role type); however, it may invoke the "person" perspective when attribution of certain personality traits is viewed by P as necessary to promote and sustain interaction in a desired direction (e.g., to generate O's concern with his feelings or needs).

The second case of channeling deals with situations in which P's objectives are anchored in the goals of a group which also involves O. If the group operates under well-defined goals and arrangements of roles for carrying out tasks, then P's objectives will elicit construction

of O's identity from the "performer" perspective by virtue of the interdependence of their activities. However, if objectives and tasks are ill-defined, then the tendency to use O's role as a background against which his behavior is assessed is reduced; hence, P's proneness to adopt the "performer" perspective is reduced as well.

The learned probabilities of occurrence of interdependent events in P's world play a role comparable to need and interest states in inducing certain categorizations rather than certain others (Bruner, 1957). For instance, P's expectancies concerning the likelihood of encountering a particular type of individual in the situation where O appears may trigger and then channel P's inferences along one path or the other, depending upon P's assumptions about the situation.

Identity construction may involve a resolution of expectations about the values of alternative categorizations. Channeling of P's inferences along one path is likely to occur when categorizations along the other path foreshadow less valued outcomes (e.g., damaging consequences for P's self-conception or a costly payoff in case of inadequate identification). In a related perspective, the channeling of inferential activity may result from a search for attributes in terms of which a given situation appears most "balanced" to P (Eiser and Stroebe, 1972).

If the channeling process is looked at from a point of view offered by cognitive dissonance theory (any other consistency theory could be referred to), it can be asserted that those inferences which entail a *reduction* of experienced cognitive dissonance will tend to emerge. Hence, depending upon the concrete situation in which P finds himself, one inferential path may be taken rather than the other. In the same vein, one path may be avoided if it implies categorizations that could *generate* cognitive dissonance. For example, in Ring's (1964) study, the author attempts to explain his data by invoking a stereotype of "status-linkage," i.e., a tendency to view behavior as *consistent* with a person's status.

These general propositions may apply

⁴ Among such values, the perceiver's self-conception (Turner, 1968) may occupy a central position. Constructing the other's identity involves making interlocked categorizations about events and persons including the *perceiver himself*.

to situations in which O is not a stranger to P. To cite one example, it may be that the identity implications of O's actions could engender cognitions dissonant with prior knowledge about O if P were viewing the other *qua* person; hence, P's adoption of the "performer" perspective.

Preference in cue utilization also can enter into determining which path of the branching will be followed. Certain cues may be used predominantly by virtue of their discriminatory value demonstrated in past categorizing situations, their face validity, their linguistic codability, etc. Systematic preference which may stem from P's professional training or his having developed some interpretative scheme of human action in general (e.g., from an ideology emphasizing an individualistic conception of social causation) similarly can orient the inferential process.

Besides preference in cue utilization, it is worth noting that certain cues may be registered preferentially because of situational demands exerted on P. Time pressure, for instance, tends to increase the salience of immediately available cues in making categorizations. Under such conditions, discerning the other's role in an interactive context might be precluded if it requires a search for subtle attributes.

Finally, P's perceptual selectivity may emphasize certain cues that permit an accentuation of the differences between the people encountered in a given situation (Bruner and Perlmutter, 1957; McGuire and Padawer-Singer, 1976). To illustrate, when O is the odd man out in a social context (e.g., the only teenager), attention tends to be focused on him not as a person but as an occupant of a position. As a result, the odd man is likely to be viewed *qua* performer and his behavior seen as role performance.

Some Additional Comments

To conclude the presentation of the framework, it is necessary to stress the following points that have been treated as implicit.

First, adoption of one inferential perspective may or may not result from a conscious decision on the part of P. Often an unconscious process, categorizing may

become conscious, for instance, because of disconfirmation of expectations or emergence of a problematic situation.

Second, identity construction is an ongoing process which, at any given time, can be characterized by either the "performer" or the "person" perspective. Activation of one perspective is assumed to suppress the *simultaneous* adoption of the other perspective because each rests upon a distinctive organization of information. Several experiments in the field of perception have demonstrated similar phenomena (see, for example, Leeper's classical experiment cited in Hastorf et al., 1970).

Third, the postulation of an initial branching *does not* preclude the possibility that, once an inferential path is taken, inferences along the other path can be made *subsequently*. Inferences made from one perspective may instigate, or fuse with inferences drawn from the other perspective.⁵

Fourth, because the present approach has focused heavily on the *initial stage* of identity construction, it has devoted little attention to later stages in which identity is being tested, negotiated and revised mainly through interpersonal communication. This approach, however, is entirely compatible with the notion that identity is socially constructed.

Fifth, the postulation of an initial branching entails a definite differentiation in the way identity construction is initiated and evolves from there. For example, in an unpublished study, a number of stories, each describing a brief social interaction, were presented to respondents who were asked to indicate what they thought of the focal person. Depending upon the nature of their inferential perspective (assessed by independent judges), respondents described the focal person in very different ways. To illustrate, a story involved a sergeant, a private and a captain (the focal person) who

⁵ Turner's position (1961; 1968) as to the framework of categorization for personality being a derivation from the categorization of roles does not actually contradict the present approach. His approach suggests a relationship between the two branches that, however, is not viewed here as invariable so far as *identity construction* is concerned.

did not intervene when the private was punished erroneously by the sergeant. While some respondents perceived the captain as being unfeeling, a "heartless creep" (the other *qua* person), others saw him as thoughtful, doing the right thing (the other *qua* performer). To those respondents who did not have an "appropriate" military role repertoire, viewing the captain *qua* performer virtually was forbidden since they could not realize that the captain, in that situation, was following military rules.

Another story portrayed a teenaged son engaged in heated conversation with his mother. For some respondents, the placement of his behavior in the context of a role typically performed by teenagers in relation to their mothers did not occur (the other *qua* performer); they viewed him as being insensitive, unable to compromise (the other *qua* person).

Thus, these stories (among others) triggered divergent categorizing activities, the outcome of which could then be interpreted parsimoniously in terms of the suggested framework.

Summary and Conclusion

According to the framework outlined in this paper, at any given time P's construction of O's identity is characterized by either one of two inferential perspectives. While viewing the other *qua* performer entails the attribution of role-relevant qualities from observing behavior as specific role performance, viewing the other *qua* person involves the assignment of observed behavior to psychological causes having their origin in O's personality.

Of considerable importance for the kinetic study of the identity construction process is the case where O is a stranger to P. In that case, an initial branching constituted of two inferential paths potentially open to P is postulated. These paths correspond to the two inferential perspectives which ultimately reflect a differential emphasis placed upon certain cues selected and used by P as a basis for inference.

Although this framework has been introduced by referring to the Jones and

Davis analysis of attribution processes and the Turner treatment of role-taking, it obviously should not be regarded as a "resolution" of these theoretical positions. Actually, some aspects of both approaches have been examined and contrasted to underline the necessity of using a role concept for person perception studies inclusive enough to account for diversified categorizing activities.

In emphasizing the importance for identity construction of an inferential perspective by which P views the other *qua* performer, the present approach can deal with those common cases where P does not divide O's conduct into "personal" and "social" parts. It fully recognizes and accommodates the fact that traits or dispositions represent only one type of person concept (e.g., Little, 1967; Peevers and Secord, 1973). As shown earlier, viewing the other *qua* performer might generate inferences about O which could not possibly be derived if P were viewing him *qua* person.

One implicit assumption underlying most research in person perception is that P's inferential set is immutable; that is, he invariably views O as a self-contained entity whose actions reflect internal states and processes. What generally has been overlooked is the question of identifying the circumstances under which P behaves in the manner hypothesized. Postulating the existence of two inferential perspectives, the present approach addresses the problem of specifying conditions which, at a given time, determine P's perspective. Several factors have been mentioned as variables that might be invoked to account for, or to predict the nature of P's categorizing activity. Most of these factors indicate the imperativeness of considering the vantage point of P. Unless this basic requirement is met, the question as to whether a given behavior is in-role, out-of-role, or whatever, cannot properly be tackled except in rather atypical situations. Neither can its informational value be assessed. In the proposed framework, perceivers are not represented as undifferentiated entities juxtaposed to an environment defined in universal terms. Placing the emphasis upon the constitutive relationships between P and his

environment, this approach treats identity-construction processes in an action context where time is a factor.

The foregoing discussion intentionally has been confined to a conceptual outline. Nevertheless, some broad lines of research have been suggested from which empirical statements can be derived. In order to make predictions about inferential outcomes, a number of specific considerations must be taken into account depending upon the particular research interest. A major problem inherent in the suggested approach lies in the difficulty of defining P's vantage point. For example, what constitutes appropriate role behavior depends on P's role conception, hence the importance of gaining access to it. Although such empirical problems are not insoluble, they clearly must be resolved if the present approach were to be adopted.

Experimental work in the different lines of research that this tentative framework suggests will undoubtedly necessitate its reformulation. As it stands, it addresses important issues in identity construction that have not yet received the attention they deserve.

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MARITAL STATUS, LIFE-STRAINS AND DEPRESSION

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The relationship between marital status and psychological distress, consistently documented by a variety of studies, traditionally has been interpreted as reflecting the unmet inner needs and emotional frustrations of never married and formerly married people. In contrast, the present study examines the depressive consequences of economic hardship, social isolation and parental responsibilities as three durable, structured conditions of life to which unmarried people are both more exposed and more vulnerable.

Occasionally important relationships between social structural factors and mental disorders are discovered far in advance of our learning how these relationships are forged. This has been the case with regard to the psychological impact of such fundamental features of social organization as social class (Kohn, 1972; Mechanic, 1972) and sex roles (Gove, 1972; Gove and Tudor, 1973; Pearlin, 1975) as well as for marital status, the concern of this report. For many years, at least since Durkheim's (1966) work, it has been recognized that the unmarried are more disposed than the married to emotional problems. There is now an accumulation of studies showing with rare, though not perfect (Dupuy et al., 1970), consistency that people with spouses are more likely to enjoy psychological well-being than those without

(Blumenthal, 1967; Bradburn, 1969; Briscoe and Smith, 1974; Gurin et al., 1960; Knupfer et al., 1966; Radloff, 1975; Srole et al., 1962).

Although there is relatively little research aimed directly at understanding how these differences arise, three major explanatory themes can be extrapolated from existing writings. The first, discussed by Bachrach (1975:6-7) in her extensive review of the literature, pertains exclusively to the formerly married. It views the very transition from marriage to singleness as a stressor event entailing a variety of psychic traumas. Self-conceptions may be assaulted in the course of becoming single, social networks often become unravelled, and established habit patterns frequently must be abandoned. It also has been suggested

(Hunt, 1966) that the formerly married have the difficult task of accommodating to the new role definitions that are more ambiguous than those that gave meaning and direction to their behavior as married men and women. Sexual problems, too, may be especially severe for those newly single who had been accustomed to sexual gratification (Bell, 1966). But regardless of the particular problem identified by different observers, each emphasizes essentially the same process: becoming unmarried is itself a condition imposing difficult and often unclear demands for change, and it is these demands that lead to psychological disturbance.

A second interpretation, perhaps less convincing now than in earlier years, attributes the psychological distress of the unmarried to the powerful norms defining marriage as the most desirable state adults can attain. According to this view, the single who remain single are moving against the grain of these norms, eventually running the risk of being treated as people either unwilling or unable to conform to accepted practice. They are considered as being outside the normative pattern of living (Lee, 1974; Udry, 1974; Gurin et al., 1960) and come to be seen as unfit and deviant. Their disposition to psychological problems, therefore, is a consequence of being objects of contempt because of their failure to conform to prized norms.

Finally, it has been proposed that the psychological problems of the unmarried are not a consequence of their marital status but, rather, a cause of it. This explanation argues that people who are depressed or who suffer other personality debilities are selectively less able than others to be married and to stay married. Such an interpretation is especially favored by epidemiologists and clinicians who have observed the disproportionate number of unmarried people among those suffering from psychoses (Ødegård, 1946; 1960; Malzbert, 1964; Garfield and Sundland, 1966). Because interpretations of selectivity usually are made from observations of psychiatric populations, we cannot be sure how applicable they are to more representative populations. Nevertheless, while such interpretations may be

limited in their generality, they underscore the difficulties in determining whether the marital status or the onset of the psychological disturbance comes first (Bachrach, 1975:2-3).

The present analysis emphasizes the stress-provoking problems of daily life which, with rare exception (Warheit et al., 1976), are ignored by prevailing perspectives. A comprehensive understanding of the relationship between marital status and mental functioning must recognize more than transitional traumas, normative violations and the possibility of psychological selectivity. The part played by structured, persistent life-strains also must be considered. Three such conditions concern us here: the economic resources of people, isolation from social networks, and parental responsibilities. We shall see that these conditions do, in fact, contribute to differences in the psychological functioning of the married and the unmarried, because the unmarried are more frequently exposed to them and are more vulnerable to their effects.

Background and Methods

The data presented here are part of a larger investigation into the social origins of personal stress and were gathered through scheduled interviews with a sample of 2,300 people representative of the population of the Chicago area, which includes sections of Northwestern Indiana as well as some of the surrounding suburbs. A cluster sampling technique was used, each cluster consisting of four households per block. Since the total sample of 2,300 was to be made up of clusters of four households, the interviewing was done in 575 blocks, one-fourth the size of the sample. The 1970 census (U. S. Bureau of the Census, 1972) reports that there are 2,137,185 households in the Chicago Urbanized Area and when this total is divided by the total number of blocks in which households are to be chosen (575), the result (3,716) is the skip-factor for the selection of households. After each 3,716th household was selected, three additional households from that block were chosen by dividing the total number of households on the block

by four and using the result as the factor for counting from the initially selected address. In anticipation of refusals—30 percent of those contacted—and to make allowance for households where contact could not be established within three call-backs, substitute addresses in each block were also pre-listed. The sex of the person to be interviewed in each household was pre-designated in order to have as equal a number of males and females as possible. Finally, only those between 18 and 65 were included, producing a sample still actively engaged in occupational life.

The interview dealt extensively with the strains experienced by people—the conflicts, frustrations and threats that earlier exploratory interviews revealed to be quite common. It particularly focused on strains occurring in the roles of occupation, marriage, childrearing and economic life. Second, the interview sought to identify the coping repertoires people employ in dealing with the strains they experience in these roles. Third, it inquired into people's emotional stresses and their symptoms of disturbances such as depression and anxiety. The present analysis, while focusing on marital status, includes information from a number of different areas covered by the interview.

Depression

Although fundamentally we are interested in the relationship of marital status to all manifestations of emotional well-being and distress, we shall limit our examination to depression. This is partly for ease and partly because it is a state experienced to some degree by everyone under ordinary conditions of life. Indeed, it has been described as the "common cold of psychiatric practice" (Silverman 1968:131). Its commonness suggests that it may be a fairly sensitive psychological barometer of life-strains, and it is largely for this reason that it is selected for use here. But it is important to recognize that depression is only one of several indicators of distress that are related to marital status. Thus, measures of anxiety, happiness, psychosomatic disturbances and self-esteem each are associated with marital status at a level of statistical

significance. Our emphasis on depression, therefore, should not obscure the fact that marital status also is relevant to other dimensions of well-being and distress.

The measure of depression adopted for use here, developed by Lipman et al. (1969) and Derogatis et al. (1971), asks respondents the frequency with which they experience each of eleven symptoms.¹ It should be emphasized that such items are not suitable for the identification of clinical depression among people residing in the community. Although the scales are constructed from symptoms reported by patients in clinical treatment, they cannot be employed as diagnostic tools in field studies. Scores on these measures serve only to reveal an order among people based on the number and intensity of symptoms they experience.

Marital Status and Depression

For much of our analysis, two groups will be delineated: the married and the unmarried. However, in order to show the pivotal relationship between marital status and depression in greatest detail, Table 1 further subdivides the unmarried into the widowed, the divorced and, although they are still legally married, the separated. The results clearly show that the presently married are most free of depression, the formerly married are most burdened by it, and the never married fall squarely between these extremes. Among the groups making up the formerly married category, the separated are outstandingly most susceptible to depression, with no appreciable difference existing between the divorced and the widowed.

Because several important social characteristics are associated with marital status, the relationship between marital

¹ Respondents were asked to reply "never," "once in a while," "fairly often," or "very often" to the following items. During the past week how often did you: lack enthusiasm for doing anything? have a poor appetite? feel bored or have little interest in things? lose sexual interest or pleasure? have trouble getting to sleep or staying asleep? cry easily or feel like crying? feel downhearted or blue? feel low in energy or slowed down? feel hopeless about the future? have any thoughts of possibly ending your life? feel lonely?

Table 1. Marital Status and Depression (Percent)

Depression		Married	Never Married	Formerly Married	Types of Formerly Married		
					Widowed	Divorced	Separated
High	1	12	20	27	22	27	32
	2	14	18	20	22	14	25
	3	18	23	17	15	16	23
	4	29	24	18	21	22	10
Low	5	27	16	18	20	21	10
	N=	(1589)	(288)	(415)	(172)	(141)	(102)

$X^2=95.2$; 8 d.f.
 $p<.001$.
 $\text{Gamma}=.26$.

status and depression may be spurious, shaped not by marital status but by its attendant social characteristics. Sex, age and race are particularly implicated in this regard, for each of these characteristics is differentially distributed among the marital statuses. For example, women and blacks are markedly overrepresented in all categories of formerly married. With regard to age, the never married have the youngest mean age and, unsurprisingly, the widowed the oldest.

In order to determine if the relationship of marital status and depression persists after taking account of sex, age and race, we have partialled out the effects of these characteristics by the use of gamma. The zero-order gamma between marital status and depression is .26 when the sample is divided into married and unmarried categories. When controlling on the effects that sex has on this relationship, the resulting partial gamma is .22 and when controlling on race and age, the partials are .24 and .27, respectively. The same stability exists, furthermore, in applying these controls separately to the never married, widowed, divorced and separated. In no instance is there an appreciable change in the bivariate relationships existing between the marital statuses and depression when the influences of key social characteristics are taken into account. We can be confident, then, that the results of Table 1 do not spuriously stem from ascribed characteristics that are related to, but different from, marital status itself, for the results persist after the possible influences of such characteristics are removed. Nevertheless, as we observe

some of the conditions that do link marital status and depression, the potential relevance of these characteristics will surface again. Consequently, they will be considered throughout the course of the analysis.

Marital Status and Life-Strains

The life-strains we shall be examining—economic hardships, social isolation and parental role overloads—have in common a resistance to rapid amelioration. They are persistent problems, and persistent problems can have deleterious psychological effects over time. These three life-strains do not account for all of the difference in depression between the married and the unmarried, but they do illuminate how adverse circumstances built into the structure of daily life contribute to the differences. Two mechanisms will be shown to underlie this contribution: differential exposure and differential vulnerability. Each of these will be in evidence as we consider the three life-strains one by one.

Economic hardship. Our evaluation of economic strains relies on a three-part question asking people about their difficulties in acquiring the necessities of life: How often does it happen that you do not have enough money to afford (1) the kind of food you (your family) should have? (2) the kind of medical care you (your family) should have? (3) the kind of clothing you (your family) should have? Each item was answered by "never," "once in a while," "fairly often" or "very often" and a score ranging from 3 to 12 was formed by sum-

ming the values of the three responses. This indicator of economic strain has two advantages over objective income: over 300 respondents could not or would not provide income information, and more important, a given amount of income is not an equivalent resource for families of different size and age composition. Nevertheless, our direct measure of strain is closely related to income, indicating that subjectively experienced strain is rooted in objective economic conditions.

Table 2 divides people according to economic strain, with those scoring 3 being classified as being "free" of strain, from 4 to 6 as under "moderate" strain and from 7 to 12 as experiencing "severe" strain. The column marginals reveal that the unmarried are more likely than the married to experience economic strains: only 73 (5%) of all married people are under severe strain compared to 13% of the unmarried. Correspondingly, 74% of the married are free of strain compared to 55% of the unmarried. Within the unmarried group, the separated experience the most hardship, followed by the divorced, widowed and never married. The table also points to a connection between the intensity of economic strain and the level of depression for, regardless of marital status, the proportions high on depression vary directly with the severity of economic strain. Thus, economic hardship links marital status and depression by (1) being more prevalent among the unmarried and by (2) being a condition disposing people to depression.

However, their greater *exposure* is not the only reason that unmarried people are

more depressed by economic strain; they also appear to have a greater *vulnerability* to the effects of limited resources. This is suggested by comparing the married and the unmarried who are exposed to economic strains of the same severity. The pattern revealed by this comparison appears repeatedly throughout the analysis: the differences in depression between the married and the unmarried are greatest under conditions of greatest strain; as strains diminish, differences in depression among people of different marital status also diminish, although they remain appreciable. These variations in vulnerability to hardship are mirrored in the magnitude of the gamma coefficients under different levels of strain. Whereas we saw earlier that unmarried people are more likely to experience economic deprivation, we see now that they also may be more readily depressed by such deprivation. Both differences are reflected in the relationship of marital status and depression.

Is the effect of economic hardship on the relationship of marital status to depression attributable to extraneous social characteristics? This question is especially pertinent with regard to race; for a disproportionate number of blacks experience severe economic hardships, thus raising the possibility that race more than economic strain is the influential condition in Table 2. However, for blacks and whites alike, the association between marital status and depression is closest under conditions of greatest strain, with the association being reduced appreciably with each reduced level of economic

Table 2. Economic Strains, Marital Status and Depression (Percent)

Depression	Severe Strain		Moderate Strain		No Strain	
	Married	Unmarried	Married	Unmarried	Married	Unmarried
High 1	26	50	19	29	9	15
2	17	15	22	21	12	18
3	8	12	16	27	19	17
4	26	16	27	13	30	26
Low 5	23	7	16	10	30	24
N=	(73)	(92)	(335)	(220)	(1175)	(383)
	$X^2=16.6.$		$X^2=31.2.$		$X^2=24.4.$	
	4 d.f.		4 d.f.		4 d.f.	
	$p<.01.$		$p<.01.$		$p<.01.$	
	Gamma=.43.		Gamma=.24.		Gamma=.17.	

strain. The gamma for this association among blacks under severe strain is .44 and under no strain it is .31; for whites, the corresponding coefficients are .31 and .14. Furthermore, these relationships are found in the same order among men and women, among those less than forty years of age and those forty and over. While there are variations from one group to another in the magnitude of the association, invariably the unmarried, regardless of their race, sex or age, differ most from the married when under greatest strain and the difference is considerably diminished when strain is minimal. Thus, it is economic strain that is the pertinent condition, not race, sex or age.

The same patterns of association emerge when comparisons are made of the married with the never married, widowed, divorced and separated. Except in the case of the separated, where gamma is greater in the "no strain" than in the "moderate strain" category, differences in depression between the married and each sub-type of unmarried are most pronounced where there is most deprivation and are markedly less with improved economic circumstances. Regardless of the circumstances leading to the absence of a spouse, therefore, it is a condition especially likely to be associated with depression under adverse economic conditions.

The unmarried, then, are doubly disadvantaged with regard to economic circumstances. First, they are unequally exposed to a hardship conducive to depression; second, such hardship, even when equally severe among people of different marital statuses, is still more likely to result in depression among the unmarried. Severe economic strains thus are more heavily concentrated in the unmarried population and, in addition, they take a greater psychological toll from the unmarried.

Social isolation. The apparent inability of the unmarried to withstand hardship as well as the married is not necessarily because of a greater psychic fragility on their part. It is equally reasonable to suppose that marriage itself is a barrier standing between outside strain and inner depression. When faced with adversity, married persons may have the advantage of being able to draw emotional support and con-

crete help from their partners. Of course, supportive and helping relations between people are not limited to marriage; unmarried people often establish interpersonal ties that serve as partial functional alternatives to marriage. This suggests that when one has neither a spouse nor surrogate relations one will be especially open to depression.

Several items are indicative of the extent and stability of people's social affiliations. One question asks respondents how long they have lived in their present neighborhoods; another inquires into membership in voluntary associations; and a third asks about the number of "really good friends" who live within an hour's drive. These indicators were pooled to form a measure of isolation from extrafamilial relations. The highest possible score indicates neighborhood residence for less than two years, having either only one or no friends close by, and not belonging to a voluntary association. As judged by this measure, the unmarried are more likely than the married to experience isolation, a result consistent with observations that single people, especially the formerly married, have greater difficulty than couples in establishing a durable and extensive social life (Gove, 1972). Moreover, as the extent of isolation from extrafamilial relations increases, so do the tendencies to depression. Thus, marital status is related to isolation and isolation to depression.

Since isolation is related both to marital status and to depression, it also may be one of the circumstances linking marital status and depression. As in the case of economic deprivations, however, differential exposure alone does not bear the burden of explanation. In Table 3, the married and the unmarried are divided into three groups according to the degree of social isolation they experience. Once again, the findings show the unmarried to be doubly susceptible to depression: first, a larger proportion lives in considerable isolation and, next, they are more likely to be depressed by equivalent conditions of isolation. Indeed, married people are virtually unaffected by isolation from extrafamilial contacts. For this reason, the differences in depression between the

married and unmarried who are considerably or fairly isolated stem entirely from the impact of isolation on the latter. Evidently, the absence of social ties can represent a more complete isolation for the unmarried than for those with spouses, making the depressive consequences of isolation singularly severe for unmarried people.

Again, it is necessary to ask if the results of Table 3 are being shaped by extraneous social characteristics, for blacks substantially more than whites and the older slightly more than the younger are isolated. Both among blacks and whites, isolation is most likely to have depressive consequences for the unmarried. Interestingly, however, the decline in the magnitude of the association with decreased isolation is greater among whites: for this group, the gamma drops from .35 under considerable isolation to .16 where isolation is absent, compared to .35 and .29, respectively, for blacks. Thus, the results of Table 3 arise not because blacks are more often isolated from extrafamilial ties but in spite of their being somewhat less adversely affected by isolation. Moreover, we find that, regardless of the age or sex of respondents, vulnerability to depression is greatest among the unmarried who are separated from social contacts. We can be assured, then, that the relationships in Table 3 are being conditioned by isolation, not by extraneous characteristics associated with isolation.

With regard to the different subgroups of unmarried, it can be noted that there are differences in the degree of isolation

they experience: the widowed are least isolated, followed in order by the never married, divorced and separated. However, when these groups are treated separately, we find in each case that the association between marital status and depression is closest among people experiencing either a considerable or fair degree of isolation, dropping off sharply among those not isolated. Isolation, it can be noted, makes least difference to depression among the separated, many of whom are immersed in the first steps of marital dissolution where the peak emotional crises of this period may overshadow the more low-keyed problems of life.

The absence of a spouse, therefore, is most apt to result in depression when it is in combination with isolation from contacts outside the family. Under any condition, isolated or not, the unmarried are consistently more disposed than the married to depression; but the difference between the two groups is considerably reduced in the absence of social isolation. Being without a mate apparently leaves one open to the depressive consequences of life-strains, especially so when one is also lacking alternative supports.

Parental responsibilities. Economic hardships and social isolation, while seemingly very different conditions of life, have a very important property in common. It is the rather relentless constraints each typically imposes on behavior and experience, the kind of life-strains that remain constant across a range of life-situations. The burdens and responsibilities of parenthood, to which we turn now, also may

Table 3. Social Isolation, Marital Status and Depression (Percent)

Depression	Considerably Isolated		Fairly Isolated		Not Isolated	
	Married	Unmarried	Married	Unmarried	Married	Unmarried
High 1	13	28	11	24	11	21
2	15	19	13	15	14	16
3	18	19	18	20	19	22
4	28	21	30	21	29	20
Low 5	26	13	28	20	27	21
N=	(270)	(200)	(315)	(135)	(957)	(343)
	$X^2=24.5$		$X^2=27.1$		$X^2=29.9$	
	4 d.f.		4 d.f.		4 d.f.	
	$p<.001$		$p<.001$		$p<.01$	
	Gamma=.32.		Gamma=.35.		Gamma=.20.	

impose persistent strains having depressive consequences.

The most direct indicator of parental burdens is the number of children for whom the parent has responsibility. There is one respect, however, in which this feature of parental responsibility is quite different from the other strains we have examined. Although 38 percent of the unmarried people in our sample have the daily care of at least one child, it is the married who are much more likely than the unmarried to have the larger number of children at home. Unlike their greater exposure to economic hardship and social isolation, single people are less exposed than the married to this potential source of strain. If parental responsibilities have a part in the relationship of marital status and depression, therefore, it is not because these responsibilities are more concentrated among the unmarried, but because such responsibilities, where they do exist, are more onerous for the unmarried.

The unmarried do, in fact, have a greater sensitivity to these burdens. Table 4 reveals that as the number of children in the household increases to three or more, there is a corresponding increase in the proportions of unmarried parents who are highly depressed. By contrast, there is a slight tendency in precisely the opposite direction among the married. The net result, reflected in the magnitude of the gamma coefficients, is that the difference in depression between the married and the unmarried having three or more children at home is substantial, while the difference between those having no children

in the household is considerably smaller. Thus, marital status and depression are most closely connected when parental responsibilities are most demanding. The never married who are parents, it should be pointed out, are somewhat more affected by these responsibilities than are the formerly married groups.

Because the social characteristics of people are closely interwoven with the magnitude of their parental responsibilities, it is again necessary to examine their potential part in producing the results in Table 4. Sex is especially salient in this regard, for almost all unmarried people with responsibility for the care of children are women. Only 18 single men have any parental responsibility, four of whom have the care of three or more children. It is difficult, therefore, to separate fully the possible effects of sex from those of parental responsibility on the relationship of marital status and depression. It is instructive, however, to focus on the 18 single men who have responsibility for children. Although few in number, they are entirely similar to their female counterparts, for they are much more disposed to depression than are married men having the same parental responsibilities and more depressed, too, than those men who are also single but free of these responsibilities. Although this is by no means conclusive, it suggests that sex is not responsible for the results in Table 4.

Race, too, is intertwined with the magnitude of parental responsibilities, with unmarried blacks more likely than unmarried whites to have children and to have

Table 4. Number of Children at Home, Marital Status and Depression (Percent)

Depression	Number of Children					
	Three or More		One or Two		None	
	Married	Unmarried	Married	Unmarried	Married	Unmarried
High 1	12	34	12	26	11	20
2	12	18	14	20	17	19
3	16	17	19	24	20	19
4	31	16	28	18	27	23
Low 5	29	15	27	12	25	19
N=	(458)	(89)	(715)	(179)	(416)	(435)
	$X^2=36.9$; 4 d.f. $p<.001$. Gamma=.42.		$X^2=41.1$; 4 d.f. $p<.001$. Gamma=.36.		$X^2=19.3$; 4 d.f. $p<.001$. Gamma=.19.	

them in larger numbers. However, when the relationships in Table 4 are examined separately for blacks and whites, the pattern of associations is parallel. Thus, the gamma coefficient for the association between marital status and depression among whites having three or more children is .41 and for blacks with the same number of children, it is .51; for whites with no children, it is .18 and for the corresponding group of blacks, it is .15. Finally, it is the younger parents who are likely to have the care of children at home, making it necessary to distinguish the effects of family size from the possible effects of parental age. When the relationships in Table 4 are examined separately for people younger than forty years and those older, equivalent results appear. The results in the table, therefore, cannot be attributed to race or age or, judging from the 18 unmarried male parents, to sex.

The age of children, like family size, is also a dimension of family composition indicative of parental responsibilities. Briefly, we find that the younger the children in the household, the greater is the association between marital status and depression. The gamma coefficient for this association among parents whose youngest child is five years or less is .48; for those whose youngest child is from 6 to 17, the coefficient is .39; and it declines further to .13 among parents having a youngest child over 18. These age groupings, of course, represent general stages of enlarged self-sufficiency on the part of children and, correspondingly, less constraint on, and more freedom for, the parent who attends his last-born child as he ascends through the age ranks. Thus, depression is most likely to exist among unmarried parents of very young children, the arc of the life cycle in which the burden of parental responsibility is greatest. By the time children reach 18 years and beyond, ages of greater independence, differences between the married and unmarried parents shrink to insignificance. It is apparent that the burdens of caring for young children are felt most keenly by the parent who faces the task alone.

Being a parent, then, entails very different obligations, has very different mean-

ings, and yields very different experiences for the married and the unmarried, regardless of their other social characteristics. The greater the number and the younger the ages of children for whom unmarried parents have responsibility, the greater these differences become. For single parents, the joys of parenthood are most likely to be aroused when the number of their children is small and their ages large.

The Life-Strains: Their Independent and Joint Effects

The foregoing analysis has succeeded in identifying the ways in which some key life-strains contribute to the association between marital status and depression. It is apparent that persistent problems, unequally concentrated among and having an unequal effect on the unmarried, forge the links between marital status and depression. Being unmarried increases one's chances of experiencing hardship and of being psychologically hurt by such experience.

But there are certain issues that remain unaddressed. For example, we have not yet taken into account the overlap that exists in being poor, being isolated and being burdened by parental responsibilities. In order to know the order of importance among these conditions, it is necessary to weigh the effect each has when independent of the others. Second, a direct assessment of the greater vulnerability of the unmarried to life-strains requires that we sort out the interactions of strains and marital status and observe the effect they have on depression in conjunction with each other. Finally, we have not yet dealt with the magnitude of the combined influence of the life-strains on the relationship between marital status and depression. Each of these issues can be dealt with by regression analysis.

Two sets of terms are brought together in the regression equation, the results of which are presented in Table 5; one is made up of the additive terms—marital status and the three life-strains—and the other includes the interaction terms—variables created by joining each of the strains with marital status. The coefficients show the relationship of each vari-

able to depression after the effects of the others are removed. Looking first at the main effects of the additive terms, it can be seen that economic strain is preeminently more important to depression than any other variable. Because, as we saw, the unmarried are much more likely to be exposed to economic deprivation, it is by far the major contributor to the association of marital status and depression. But what about the differential vulnerability of the unmarried to life-strains? Some of the depressive effects of strains were found to be conditional on the marital status of people experiencing them, suggesting that life-strains and marital status jointly produce effects uniquely different from those created by each separately. These effects are captured by the interaction terms which, as shown in the table, are appreciably related to depression, with parental responsibilities in conjunction with marital status being more closely associated with depression than the other combinations. This is consistent with our earlier observation that the number of children is positively related to depression among the unmarried, but is slightly negatively related among the married.

Given that the purpose of this inquiry is to understand why being married or unmarried should make a difference to depression, perhaps the most central issue concerns the extent to which the life-strains have provided this understanding. Our success in this regard can be gauged from the magnitude of the reduction in the relationship between marital status and depression that occurs when the life-strains are taken into account. Thus, the bivariate regression coefficient of depres-

sion on marital status, shown in Table 5, is .206. As can be seen in the table, the coefficient is lowered to .064 when both the main and interactive effects of the strains are separated from marital status. This means that about 69 percent of the original association of marital status with depression is attributable to the differential exposure and vulnerability of the unmarried to the life-strains considered here, economic hardship being foremost among them. The life-strains, therefore, carry most of the burden of explanation, although other conditions remain in marital status that continue to be related to depression.

Overall, being without a spouse is more likely to result in depression when one is also enmeshed in a context of unrelenting strains, especially strains of an economic nature. It is apparent that single people withstand these conditions of hardship less well than do married people. Indeed, marriage is the more beneficial arrangement under all conditions, since being single is somewhat disposing to depression even when strains are absent. Nevertheless, the advantages of marriage are especially apparent when life circumstances are most difficult, not when they are most benign.

Summary and Implications

Few items in the sociological arsenal of information have had a longer history or a more consistent empirical documentation than the differential access to psychological well-being by married and unmarried people. Yet, surprisingly little empirical effort has been made to account for this

Table 5. Regression of Depression on Marital Status, Life-Strains, and Their Interactions (Standardized)

	Regression Coefficients	Standard Error	R ²
Marital Status (MS)	.064	.499	.042
Parental Responsibilities (PR)	.089	.499	.043
Social Isolation (SI)	.056**	.099	.053
Economic Strain (ES)	.231**	.058	.111
PR x MS	.164*	.279	.113
SI x MS	.057**	.089	.117
ES x MS	.043**	.087	.119

* Significant at the .05 level.
 ** Significant at the .001 level.

difference, possibly because the explanations seem so beguilingly self-evident that there is little need to spell them out: the single are open to psychological problems because they are deviant in a society that prizes marriage, because of the traumas they suffer and because of their unmet emotional needs.

While there indeed may be some validity to these views, we sought to learn if the greater disposition of unmarried people to psychological disturbance, depression in particular, also results from their greater exposure to hardship and strain. It was our expectation that when people had to contend with the same life-strains, they then would be similarly inclined to depression, regardless of their marital status. The unequal distribution of hardships provides only a partial explanation, however, for even when hardships are equally severe, their effects are more penetrating among the unmarried. Clearly, it is where one is confronted both by social and economic strains *and* is single that one is most prone to depression. The combination most productive of psychological distress is to be simultaneously single, isolated, exposed to burdensome parental obligations and—most serious of all—poor.

What we have learned suggests that marriage can function as a protective barrier against the distressful consequences of external threats. Marriage does not prevent economic and social problems from invading life, but it apparently can help people fend off the psychological assaults that such problems otherwise create. Even in an era when marriage is often a fragile arrangement between couples, its capacity to protect people from the full impact of external strains makes it a surprisingly stable social institution, at least in the absence of alternative relations providing similar functions.

It should be underscored that it was not our purpose to identify all factors relevant to depression, most of which are beyond the scope of this paper. Thus, as important as they may be, we have ignored personality factors, eruptive events and ephemeral experiences. Instead, we focused on conditions that are durable and persistent, anchored in economic organ-

ization, social relations and family structure. These sorts of conditions deserve to be stressed not because they explain everything we want to know, but because they bespeak the fact that the links between marital status and depression are, in part, shaped by conditions rooted in broad social and economic structural arrangements and are not simply reflections of individual adjustments made on the basis of personality.

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SIZE, CENTRALIZATION AND ORGANIZATIONAL ADOPTION OF INNOVATIONS *

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This study concerns attributes of organizations likely to facilitate or inhibit the adoption of innovations by organizations. First, it offers an interpretation of the often-found relationship between size and adoption frequency. Second, it investigates relationships between structural attributes—specialization, functional differentiation and centralization—and adoption and it develops a model which specifies interactions between these attributes and the type of innovation adopted. An interaction between size and centralization is also posited, and the model is tested using data gathered from a sample of U.S. hospitals. The findings on the whole are consistent with the hypothesis that adoption of innovations compatible with the interests or perspectives of lower-level decision-makers occurs more frequently in large, specialized, functionally-differentiated and decentralized hospitals. Centralization and the interaction between size and centralization do not appear to affect adoption of innovations which are not compatible with the interests of lower-level decision-makers. Contrary to expectations, the data indicate that functional differentiation facilitates adoption of this type of innovation. A revised model of adoption behavior is suggested, and its implications for a general theory of organizational adoption behavior are explored.

Studies of innovation adoption in organizations have suffered from inadequate conceptualization and from a failure to distinguish among types of innovations. Specifically, despite numerous empirical findings which document a positive relationship between size and adoption behavior (Aiken and Hage, 1971; Becker and Stafford, 1967; Corwin, 1972; Hage and Aiken, 1967; Mytinger, 1968; Mohr, 1969; Rosner, 1968), no clear interpretation has been offered. It is often assumed that size implies availability of the uncommitted resources required for adoption. However, there is little logical explanation for this relationship. In addition, there have been few studies designed to identify differential adoption patterns for different types of innovations. The conclusion frequently drawn is that organizations are either "pioneers" or "laggards" in general, rather than pioneers in some areas and

laggards in others. An approach which distinguishes among innovations types is likely to help explain otherwise inconsistent findings. One such inconsistency involves the relationship between centralization and adoption (Corwin, 1972; Evan and Black, 1967; Hage and Aiken, 1967; Palumbo, 1969; Sapolsky, 1967; Wilson, 1966). While inconsistent findings may be attributed to differences in conceptualizing and measuring centralization, they also may be due to a failure to take the type of innovation into account. It is possible that centralization facilitates the adoption of some types of innovations while inhibiting that of others. This paper distinguishes between two distinct types and empirically assesses their relationships with centralization. It also articulates a theoretical rationale for explaining the impact of size and empirically assesses the direct effects of size on adoption behavior. The findings reported here are limited to the population of non-federal U.S. hospitals; however, these organizations are of widely varying types and have a diversity of structural arrangements. The findings, therefore, provide insight into the processes of organizational adoption in general.

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The Implications of Organizational Size

Mohr (1969) has argued that larger organizations, simply because they are large, are unlikely to adopt innovations. Recognizing that size and adoption are often associated, he states that "size itself is not related to innovativeness by logical necessity; it becomes significant only when it implies or indicates the conceptual variables that are important in themselves" (Mohr, 1969:121). Size may indeed have spurious or indirect effects. However, it is also likely to lead directly to economies of scale which enhance the feasibility of innovation adoption. Larger organizations process input in sufficient volume to justify adoption of new technology to accommodate variations in input even when variations occur infrequently. Smaller organizations, however, experience many types of input variations so infrequently that they could not reasonably expect to benefit from making similar accommodations. While Mohr is likely to be correct in believing that size has indirect effects on adoption, it is likely that, conceived as input volume, size has a direct effect as well.

Indirect effects of size may be traceable to its impact on organization structure. As it allows for differentiation in technology, size also allows organizations to more finely differentiate tasks (functional differentiation) and personnel (specialization).

Specialization. Evidence presented by Blau (1970b), Blau and Schoenherr (1971), Child (1973), Hinings and Lee (1971), Meyer (1972) and Pugh et al. (1968) is consistent with the expectation that large organizations are in a better position to employ specialists and formally to differentiate responsibilities assigned to personnel in order to accommodate variation in input material. By employing specialists, the organization gains access to knowledge of new ideas, practices and technical skill (Aiken and Hage, 1971; Corwin, 1972; Hage and Aiken, 1967; Mytinger, 1968). To the extent that both knowledge and skill required to utilize innovations are necessary prerequisites for adoption, organizations with more spe-

cialists may be expected to adopt more innovations when the innovations are compatible with the specialists' needs and interests.

Differentiation. Task or functional differentiation can have a similar impact on adoption behavior. Formally differentiating task responsibilities to organizational personnel focuses their interests within specialized areas. This in turn acts as a constraint on individuals to increase their expertise independently of specialized training while at the same time giving rise to the development of vested interests. Both factors increase intra-organizational pressures to adopt innovations which are compatible with the interests and perceptions of department heads.

Centralization. Besides increasing an organization's access to knowledge and expertise by facilitating specialization and differentiation, organizational size also affects the degree of control top management can feasibly exercise over the decisions and activities of specialized lower-level personnel (Blau, 1970a; Downs, 1967). With increases in size, both financial costs of control and distortions in communication required for control are likely to increase. Wilensky (1967) and Pondy (1970) suggest that with incremental increases in size, control costs increase exponentially to the point where top management is obliged to allocate a portion of its discretionary power. As a consequence, lower-level personnel are in a better position to facilitate the adoption of innovations which suit their needs and interests while resisting those which do not (Coughlan et al., 1972; Wilson, 1966; Zaltman et al., 1973). The degree to which the organization's authority structure is centralized therefore can be expected to be negatively related to the frequency with which organizations adopt innovations compatible with the interests of lower-level personnel.

Innovation Types and Expected Relationships

These considerations lead us to expect positive relationships between adoption

behavior and size, specialization, functional differentiation and decentralization. Innovations helpful to some, however, may be viewed as an inconvenience or even as a threat by others. Organizational attributes which facilitate the adoption of some innovations will not facilitate and may even inhibit the adoption of others. Specifically, functional differentiation, specialization, and decentralization will affect adoption of those innovations which are compatible with the interests or perspectives of specialists or department heads, while having no effect on—and perhaps even inhibiting—the adoption of noncompatible innovations.

Size, unlike other attributes, is likely to facilitate adoption of innovations regardless of their compatibility with the interests of specialized personnel. As noted earlier, greater input volume allows for adoption of innovations which can be employed to process types of material which account for only a small percentage of the total. Since small organizations cannot reasonably be expected to acquire these innovations, size may be expected to lead to adoption regardless of the innovations' compatibility with the interests of specific individuals. Though the indirect effects of size via specialization, differentiation, and centralization may be attenuated by incompatibility, a direct effect is likely to remain.

The considerations outlined thus far lead to the following expectations concerning relationships between structural attributes of organizations and the adoption of innovations which are compatible or noncompatible with the interests and perspectives of lower-level personnel such as specialists or department heads.

1. Size, conceived as input volume, will be positively associated with the frequency of adoption of both compatible and noncompatible innovations.
2. Specialization and functional differentiation will be positively associated with the frequency with which compatible innovations are adopted. They will not be associated, save perhaps negatively, with the frequency of adoption of noncompatible innovations.

3. Centralization will be negatively associated with the adoption frequency of compatible innovations. Its association with the frequency of adoption of noncompatible innovations will be zero or positive.

In addition to these hypotheses, the theoretical perspective suggests the presence of an interaction effect between size and centralization on the one hand and adoption behavior on the other. If, as hypothesized, decentralization facilitates adoption of innovations compatible with the interests of specialists and department heads, its impact should be greatest when top management would incur substantial costs were it to exercise its monitoring and control prerogatives to limit expenditures. As noted earlier, this would be the case in large organizations. Specifically, specialists and department heads may exercise their decision-making discretion in their own interest primarily when top management must incur costs to constrain them. In addition to the three sets of relationships hypothesized above, then, we may look for a fourth:

4. Size and centralization will interact to affect adoption decisions in such a way that compatible innovations will be adopted more frequently by organizations which are simultaneously large and decentralized. Noncompatible innovations will not be adopted more frequently—and, perhaps, even less frequently—by these organizations.

The four sets of hypotheses outlined above represent a rather complex model of organizational adoption behavior. This model posits one main effect (size and adoption), three first-order interaction effects (specialization x compatibility x adoption, functional differentiation x compatibility x adoption, and centralization x compatibility x adoption), and one second-order interaction effect (size x centralization x compatibility x adoption). The goodness of fit between the model and actual adoption behavior was assessed by comparing structural determinants of the adoption of medical and administrative innovations in a representative sample of U.S. hospitals.

The Study

This study is part of a project designed to discern organizational correlates of adoption behavior in a random sample of approximately 1,000 U.S. hospitals. Hospitals were selected for study because they offer variability in several theoretically important areas while at the same time not being substantially different in ways which would preclude measurement comparability and standardization (Perrow, 1965). For example, some hospitals are private and run for profit; others are essentially voluntary associations; still others are public agencies operating under government auspices. Likewise, hospitals are located in areas with varied populations and resources and exhibit a variety of linkages with other organizations (e.g., universities, welfare agencies). Hospitals also show wide variation in the degree of integration, technological complexity, structural differentiation and centralization of decision-making, all of which are considered germane to the study of adoption behavior in organizations (Zaltman et al., 1973). At the same time, hospitals are similar in ways which make them ideal for large-scale comparative study. Measures can be constructed to quantify the relevant variables, and the measures can be standardized to apply equally well to almost all types of hospitals.

Each hospital in the sample was sent two questionnaires, one to the chief medical officer and one to the chief administrative officer. Sixty-seven percent of the medical officers and 68 percent of the administrative officers responded to the questionnaires; and 489 hospitals responded to both instruments.¹ Information on several dimensions was secured from the American Hospital Association to supplement the data base. Since these data were gathered for all hospitals in the sample, it was possible to test for response

bias. Twenty-seven percent of the proprietary (for profit) hospitals returned both instruments, while 52 percent of the voluntary (private) and 49 percent of the local public hospitals did so ($p < .001$). No response bias was found when response rates were compared on the basis of SMSA classification, long- versus short-term facilities, or number of beds.

The independent variables: size and structure. The literature dealing with organizational size has employed many different measures of this concept. Recent works increasingly have employed logarithmic transformations of size to adjust for curvilinearity in relationships between size and other variables (Kimberly, 1976). Logarithmic transformations may be justified on either theoretical or empirical grounds. In the current research, size was taken to be the log of the number of annual patient admissions to reflect (1) input volume and (2) the expectation that the effect of an incremental increase in size on structural variables would decrease as the organizations became larger.²

Using the Directory of Medical Specialties, the measure of the degree of specialization was taken to be the number of each of the 25 medical specialty areas represented on a hospital's staff. The appropriateness of this counting procedure was confirmed by a Guttman Scale analysis which yielded a coefficient of reproducibility of .92.³ A similar measure was constructed to assess functional differentiation. The presence or absence of 14 distinct services available in each hospital (e.g., an outpatient care unit, an emergency unit, an intensive care unit, etc.) constituted the measure of functional differentiation. Services require either dis-

² The correlations between the annual number of patients (logged) and other size measures (also logged) were .81 for number of support personnel, .84 for number of beds and .76 for total noncapital assets.

³ A technique suggested by Upshaw (1968) was employed to test the hypothesis that this coefficient was greater than .90 due to the frequency distribution of responses to the component items. Using this method, this hypothesis was rejected for the measures of specialization, differentiation and adoption.

¹ Thirty-nine of the 489 were operated by the federal government. In terms of resource allocation decisions, these hospitals were actually subunits within larger organizations. To avoid confounding the measure of centralization, therefore, hospitals operated by the federal government were excluded from the sample.

tinct structural divisions or specific personnel assignments (e.g., dental services, premature nurseries, organ banks, etc.). Each service, therefore, represents a distinct functionally-differentiated unit in the hospital. The coefficient of reproducibility for this measure was .91.

The measure of centralization was constructed to reflect (1) the effective, rather than simply the formal, location of discretion and (2) the location of resource-allocation discretion, rather than the discretion to determine procedures. The measure itself was constructed using an indexing procedure which yielded a score reflecting the extent to which each of five specific resource-allocation decisions were made by (1) the hospital board of directors, (2) the chief medical or administrative officers or (3) the heads of medical departments.⁴ Those hospitals in which most decisions were made by the board were considered centralized; those in which most decisions were made by department heads were considered decentralized. Hospitals in which most of the decisions were made by administrative or medical officers were considered moderately centralized. The scores ranged from a high of ten (centralized) to a low of zero (decentralized).

The dependent variables. The innovations studied fall into one of two categories: (1) those which are likely to be sought by organizational specialists and department heads and (2) those which do not directly facilitate their activities and may even be perceived as threatening. It is assumed that, other things being equal, the adoption of new technologies for diagnosis and treatment will be supported by the medical staff. While those responsible for the expenditure of hospital resources—administrators and board members—are unlikely to oppose such adoption, their concerns tend to focus

upon limiting expenses (Rushing, 1974). At the same time, innovations which allow administrators or board members greater control over resource expenditures and personnel are not likely to be sought by specialists and department heads who may even perceive such innovations as a threat to their autonomy (Sapolsky, 1967). Two types of innovations are studied here: (1) the adoption of new medical technology for diagnosis and treatment and (2) the adoption of electronic data processing (EDP) for monitoring and controlling fiscal expenditures and the activities of hospital personnel.

The measure of the frequency of adoption of medical innovations consists of the number of each of 12 new technologies adopted by the hospital. The technologies were selected in consultation with experts in an area central to the delivery of hospital service in general (i.e., respiratory disease). An initial list of 83 respiratory disease innovation items was generated, and these were rated by experts in terms of their initial and current importance, their communicability and the extent to which they represented departures from past medical practice. A literature search also produced ratings on their cost and potential risk to patients. To allow for testing the importance of these characteristics in the adoption process, 11 items were selected which represented diversity in communicability, cost, risk, etc. Despite this diversity, however, a general tendency to adopt innovations regardless of their attributes was confirmed by a Guttman Scale analysis yielding a coefficient of reproducibility of .92. To assess the reliability of this adoption measure, information concerning the presence or absence of each innovation was gathered from both the chief medical officer and the chief administrative officer. The product-moment correlation between the adoption measures based on their independent reports was $r = .78$. In addition, the researchers visited sixteen hospitals to determine for themselves whether the items were present or absent. The correlations between these determinations and the chief medical and administrative officer reports were .86 and .75, respectively. The administrative officers provided additional

⁴ The indexing procedure combined responses from both the chief medical and the chief administrative officers. The decisions involved hiring, supply acquisition and equipment purchasing. High scores were given to organizations in which all of the decisions were made by the hospital board. Low scores were given to organizations in which all of these decisions were made by physicians, almost always department heads.

information from their records concerning the time of adoption as well as the items' presence or absence at the time of the survey. Since it would be difficult to report these data for nonexistent innovations, the administrative officers' responses were taken as the indicator of adoption.

An innovation likely to enhance the monitoring and control capacities of top management was taken to be the adoption of Electronic Data Processing (EDP) for several different functions (Meyer, 1968; Sapolsky, 1967). Though most hospitals had adopted EDP, they varied widely in the diversity of functions for which it was employed. The measure of adoption, therefore, was taken to be the number of functions performed using EDP.⁵

Results

The presence or absence of the expected main effects of structural attributes on adoption was assessed by regressing both dependent variables on size, specialization, functional differentiation, and centralization. In addition, a dummy variable was introduced to test for the presence or absence of the expected interaction between size and centralization.⁶

The data set allowed for controls on several variables which could confound relationships between structure and adoption behavior. Hage and Aiken (1967) and Aiken and Hage (1971) have argued that extra-organizational contacts provide channels through which information concerning innovations may be transmitted. The frequency of such contacts is likely to be associated with both size and adoption

behavior. To allow for this possibility, three controls were introduced: whether or not the hospital reimbursed physicians for travel, the percent of staff physicians traveling at hospital expense, and the percent of staff physicians attending at least one out-of-state meeting during the year. Rosner (1968) has argued that research hospitals are likely to be more innovative than others. They are also likely to be larger and more specialized. To account for this possibility, controls were introduced for the presence of physicians with funds for research in respiratory disease, the presence of laboratory research in the hospital, and the presence of physicians with publications in the area of respiratory disease. In addition, three other controls were introduced. First, hospitals not treating the full range of diseases may be likely to be less specialized or differentiated and more likely to adopt respiratory disease innovations (e.g., hospitals treating chronic respiratory ailments such as tuberculosis). A dummy variable reflecting the general service versus specialty nature of the hospital was therefore included. Second, some hospitals have departments specifically for treating respiratory diseases. Since we were concerned with assessing medical adoption in general, rather than respiratory disease in particular, a dummy variable indicating the presence or absence of such a department was created and introduced. Finally, the presence of at least one other hospital in the area may stimulate adoption either through competition or increased information flow. Controlling for this factor also provided for a general control for rural-urban differences. In all, nine controls were introduced. A stepwise regression procedure was employed to reduce these to a more manageable number. This procedure identified three significant variables which were included as controls in subsequent analysis: (1) the presence of another hospital in the area, (2) the presence of an inhalation therapy department and (3) the presence of physicians with research funds in respiratory disease. Results of the regressions of adoption frequencies on the independent and control variables are presented in Table 1. Taken together, these variables account for 63

⁵ The specific functions performed by EDP involved accounting, admissions, discharges, personnel records, payroll, medical records, research and patient care. All of these functions have direct relevance for the visibility and control of the activities of specialists and department heads.

⁶ Hospitals were divided into three size categories and three centralization categories (nine groups) on the basis of their scores on both size and centralization. Since the expected interaction between size and centralization was expected to occur for large and decentralized hospitals, those scoring in the high size category (N=53) were assigned ones, and remaining hospitals assigned zeros.

Table 1. Adoption Frequency of Compatible (Medical) and Noncompatible (EDP) Innovations and Structural Attributes of Hospitals^a

Cases with Complete Data Independent Variable	Compatible (246)		Noncompatible (245)	
	Beta Weight	p	Beta Weight	p
Size	.18	.005	.18	.043
Functional Differentiation	.15	.018	.21	.017
Specialization	.16	.021	.10	.334
Centralization	-.07	.127	.03	.647
Size × Centralization	.11	.051	-.01	.915
	F=50.20		F=9.82	
	p<.0001		p<.0001	
	R ² =.63		R ² =.25	

* Controls: the presence (1) or absence (0) of another hospital in the area; the presence (1) or absence (0) of an inhalation therapy department; the presence (1) or absence (0) of physicians with research funds in the area of respiratory disease.

percent of the variance in the adoption of compatible innovations and for 25 percent of the variance in the adoption of non-compatible ones. Both regressions are highly significant.

As predicted, size has a significant, albeit small, impact upon the adoption of both types of innovations. This is consistent with the notion that organizations processing greater volume will be in a position to differentiate their techniques or services to articulate with variety in input material. Specialization and functional differentiation have significant impacts on the adoption of compatible innovations. This effect was expected to be attenuated in the case of noncompatible innovations; however, attenuation occurred only for specialization and this difference was not large. Functional differentiation affects the adoption frequency of both types of innovations.

Centralization and its interaction with size appear, as expected, to affect the adoption of compatible innovations; the same relationships do not appear in the case of noncompatible innovations. The coefficient describing the impact of centralization on the adoption of innovations compatible with the interests of specialists and department heads—those who make decisions in decentralized hospitals—is small but approaches statistical significance.⁷ It appears that decentraliza-

tion, as expected, affects the adoption of medical innovations; however, the data indicate that it does not lead to adoption of innovations which do not directly facilitate the work of lower-level decision-makers.

The interaction effect between centralization and size lends additional support to the self-interest perspective on adoption behavior. It was hypothesized that lower-level decision-makers would be most likely to allocate resources in their own interest when they had resource allocation discretion and when those responsible for the organization as a whole would incur significant costs were they to exercise their control prerogatives. These conditions obtain primarily in organizations which are simultaneously large and decentralized. The coefficient in Table 1 describing this interaction effect on the adoption of compatible innovations, .11, is consistent with this expectation. While it also is not large, it is statistically significant. Moreover, this interaction effect does not recur in the case of innovations which are not compatible with specialists' or department heads' interests.

Overall, the data provide considerable support for the theoretically derived model for the adoption of innovations compatible with the interests of lower-level personnel. They support most of the expectations regarding adoption of non-compatible innovations; however, the hypothesized relationship between functional differentiation and adoption of these types of innovations—expected to be zero

⁷ An analysis of covariance was conducted as an additional test for the main and interaction effects of centralization. This analysis confirmed both effects at or beyond the .05 level of significance (see Appendix).

or negative—clearly is not supported by the data. The deviations from expected relationships, particularly that between functional differentiation and the adoption of noncompatible innovations, suggest alterations in the theoretical perspective.

Structure, Innovation Type and Adoption Behavior: Discussion and Conclusions

Burns and Stalker (1961), Thompson (1969) and others have argued that there is an identifiable cluster of characteristics which determine an organization's proclivity for adopting new techniques. Others such as Wilson, (1966), Sapolsky (1967) and Hage and Dewar (1973) have argued that the values, interests and perspectives of those who allocate organizational resources play a significant part in the adoption process. The findings reported here suggest that attributes thought to characterize the generally innovative organization—specialization, differentiation and decentralization—do successfully predict to the frequency of adoption of innovations compatible with the interests or perspectives of lower-level decision-makers. Two of these attributes, however, failed to predict to the adoption of an innovation which is noncompatible. Specialization and centralization were significantly associated with only the adoption of compatible innovations; however, the difference between coefficients for specialization was small and must be taken as only suggestive. In addition, as expected, centralization appeared to interact with size to affect only the adoption of compatible innovations. This effect did not appear in the case of innovations which were not compatible. In short, different models appear to predict to the adoption of different types of innovations. The general model described by Burns and Stalker appears to be more applicable to some types of innovations than to others.

The model predicting to the adoption of noncompatible innovations was not completely supported by the data. It had been anticipated that those factors associated with adoption of compatible innovations, with the exception of size, would not be

associated, save perhaps negatively, with the adoption of noncompatible innovations. Hospitals which had a high degree of functional differentiation, however, were more, rather than less likely to have adopted noncompatible innovations. One possible explanation is that EDP, while less compatible than innovations used for diagnosis or treatment, is not incompatible with the interests of department heads, because these interests are as much administrative as medical. Department heads in hospitals, however, almost invariably are physicians, and their concerns tend to revolve around patient care rather than administration. The inconsistency, therefore, ought not to be dismissed as being due to a faulty appraisal of the relative compatibility of the two types of innovations studied. Rather, a theoretical adjustment appears to be called for.

A factor which had not been anticipated in the initial theoretical formulation involves the control requirements of the organization. Specifically, organizations which are highly functionally differentiated require greater coordination and integration among their constituent departments (Lawrence and Lorsch, 1967). Departments in hospitals, while relatively independent functionally, share—and often compete for—the same pool of resources. Optimal allocation of these resources, therefore, requires superordinate fiscal monitoring and control, and the importance of this requirement is likely to increase in proportion to the number of sharing or competing departments.

Competing department heads may be caught between a desire to realize their own interests and a need to constrain other department heads from obtaining more than their just due. As a collectivity, therefore, it may be in their own interest to encourage rather than discourage fiscal monitoring and control. This support would be forthcoming not because department heads tend to be concerned with administration per se, but because monitoring would decrease the opportunities available for other departments to acquire more than their share. This argument does not apply to specialization, since resource allocations tend to be made to departments rather than to individual

specialists. The relative compatibility of innovations under consideration, therefore, may be less relevant for highly differentiated organizations than for highly specialized ones. The impact of the interests of department heads on adoption behavior may be neutralized to the extent that these interests are in competition. More than this, the competitive situation may encourage department heads to support the adoption of innovations which would not directly facilitate their work were they not interdependent with others.

The empirical findings reported here can, within the limits mentioned earlier, be generalized to the population of U.S. hospitals. The theoretical perspective, however, promises to have more general applicability. The costs of control in many types of organizations are likely to increase with size and differentiation. The potential for divergence of interests or perspectives between specialists or department heads and top management—and among department heads themselves—is also present in industrial firms, government agencies, service organizations, etc. The impact of size conceived as input volume may also apply to organizations other than hospitals. These economies of scale, in fact, have

been documented in a wide variety of settings (e.g., Corwin, 1972; Mohr, 1969). The findings suggest that the adoption of innovations compatible with the interests of specialists and department heads will occur to the extent that the organizations are large, specialized, functionally differentiated and decentralized. Less compatible innovations, specifically those which facilitate coordination and control, are likely to be adopted by large and functionally differentiated organizations. Limits to the generalizability of these findings, of course, must be assessed through additional empirical research. Such efforts also ought to investigate adoption patterns for innovations which are clearly incompatible with the interests of top management or decision-makers located at lower levels in organizations.

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APPENDIX

Analysis of Covariance to Assess the Main and Interaction Effects of Size and Decentralization on the Frequency of Adoption of Compatible (Medical) Innovations* (N=246 Cases with Complete Data)

Size	Decentralization		
	Low	Medium	High
Low	2.88 N=40	2.96 N=24	2.55 N=11
Medium	3.90 N=30	4.16 N=31	5.17 N=24
High	5.17 N=12	7.12 N=25	7.61 N=49

Analysis of Variance				
	DF	Sum of Squares	F-Ratio	Significance Level
Between Rows (Size)	2	50.24	10.23	p<.001
Between Columns (Decentralization)	2	50.64	10.31	p<.001
Interaction	4	24.96	2.54	p<.05
Error (Adjusted)	232	569.98		

* Covariates: specialization, functional differentiation and the three control variables.

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THE ROLE OF INSTITUTIONALIZATION IN CULTURAL PERSISTENCE *

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Traditional approaches to institutionalization do not provide an adequate explanation of cultural persistence. A much more adequate explanation can be found in the ethnomethodological approach to institutionalization, defining acts which are both objective (potentially repeatable by other actors without changing the meaning) and exterior (intersubjectively defined so that they can be viewed as part of external reality) as highly institutionalized. Three levels of institutionalization were created in the autokinetic situation to permit examination of the effects of institutionalization on three aspects of cultural persistence: generational uniformity of cultural understandings, maintenance of these understandings, and resistance of these understandings to change. Three separate experiments were conducted to examine these aspects of cultural persistence. Strong support was found for the predictions that the greater the degree of institutionalization, the greater the generational uniformity, maintenance, and resistance to change of cultural understandings. Implications of these findings for earlier approaches to institutionalization are discussed.

"The only idea common to all usages of the term 'institution' is that of some sort of establishment of relative permanence of a distinctly social sort" (Hughes, 1936:180). Specific explanations of cultural persistence have been varied and, frequently, institutionalization and persistence have not been clearly separated conceptually. This paper will show that a more fully developed conception of institutionalization derived in part from the ethnomethodological approach can be used to make clearer predictions about cultural persistence. Much of the confusion of earlier discussions of institutionalization centers on the use of intervening mechanisms to explain persistence. It is argued here that internalization, self-reward, or other intervening processes need not be present to ensure cultural persistence because social knowledge once institutionalized exists as a fact, as part of objective reality, and can be

transmitted directly on that basis. For highly institutionalized acts, it is sufficient for one person simply to tell another that this is how things are done. Each individual is motivated to comply because otherwise his actions and those of others in the system cannot be understood (Schutz, 1962; Berger and Luckmann, 1967); the fundamental process is one in which the moral becomes factual.¹ Yet institutionalization is not simply present or absent; unlike many of the earlier approaches, institutionalization is defined here as a variable, with different degrees of institutionalization altering the cultural persistence which can be expected.

The research reported here investigates

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¹ Ethnomethodologists deal both with emergent culture, facing the problem of creating new culture, and with existing culture, facing the problem of cultural persistence. It is this second thread of ethnomethodology which is selected for further examination in this paper. When emergent culture is the focus, then the problem of establishing facticity becomes the central problem. It is here that the moral character of social facts becomes the central concern (Garfinkel, 1967). When the social facts are not well established, their transmission is problematic and may well depend on an obligatory, moral response to a specific situation. However, when social facts are well established, the moral character becomes less significant than the cognitive. It is this situation that is examined further here.

the effect of different degrees of institutionalization in constructed realities on cultural persistence in three distinct experiments, each one focusing on a different aspect of persistence. First, for cultural persistence, transmission from one generation to the next must occur, with the degree of generational uniformity directly related to the degree of institutionalization (Transmission Experiment). Second, once transmission has taken place, maintenance of the culture must occur, with the degree of maintenance directly related to the degree of institutionalization (Maintenance Experiment). Third, once maintenance has occurred, cultural persistence depends on the resistance to attempts to change, with the degree of resistance directly related to the degree of institutionalization (Resistance to Change Experiment).

Traditional Explanations of Cultural Persistence

Two traditional explanations of cultural persistence have received the most attention in the literature: the subsystem approach and the normative framework approach. The subsystem approach focuses on specific clusters or sectors, such as Family, Economy or Polity. Institutional subsystems, then, are separate spheres of activity, each with distinctive clusters of norms and each forming a distinct part of a typology of institutions. While the specific typology used varies widely (see Storer, 1973; Bierstedt et al., 1964; Merton et al., 1959), explanations for persistence rest on functional necessity (Angell, 1936) or on self-interested desire for rewards (Blake and Davis, 1964; Parsons, 1939; 1940; Sumner, 1906). However, both explanations have received strong criticism. First, it is difficult to determine functional necessity independently of persistence; in fact, persistence often is used as an indicator of functional necessity. Second, it has been found that some actions in institutions appear to require sanctions while others do not, so that direct social control cannot fully explain persistence.

The other major traditional approach focuses on the normative framework of institutions which persists because the

norms are shared. No external motivation for conformity is necessary because norms which are central to institutions become internalized. The actor is internally motivated to do what he has to do (Parsons, 1951; Berger and Luckmann, 1967). However, the normative framework approach provides no criteria for separating processes which are institutionalized from those which are not. There is no independent measure of which norms are most important in a social system—it is only after a norm is internalized that it can be identified as institutionalized. Further, there are no explicit criteria for determining whether or not an act is internalized. Certainly, actions performed without direct social control will not necessarily be considered internalized. Yet it is not clear how to distinguish internalization of acts on any other basis.

When explaining persistence, both traditional approaches to institutions focus on the actor's compliance with the action prescribed by the institution. Recognition of functional necessity, self-interest or internalization are thought to motivate the actor to comply. The actor plays no independent role in maintaining these institutions, rather they serve to constrain his behavior. The social structure (macro-level) determines the behavior of individuals and small groups (micro-level) and exists independently of them. While the limitations of traditional approaches to institutionalization have been stressed, they are appropriate to deal with some aspects of institutionalization and the transmission of some kinds of meaning. In fact, they may provide good explanations when institutionalization is low.

The Ethnomethodological Approach to Institutionalization

A relatively recent approach, the ethnomethodological approach, provides a strikingly different view of the role played by institutions in cultural persistence, dealing explicitly with highly institutionalized action. Reality, while socially constructed, is "*experienced* as an intersubjective world known-or-knowable-in-common-with others," which exists historically prior to the actors and fur-

nishes "the resistant 'objective structures' " which constrain action (Zimmerman and Pollner, 1970:37). To arrive at shared definitions of reality, individual actors transmit an exterior and objective reality; while at the same time this reality, through its qualities of exteriority and objectivity, defines what is real for these same actors. Macro-level and micro-level are inextricably intertwined. Each actor fundamentally perceives and describes social reality by enacting it and, in this way, transmits it to the other actors in the social system (Berger, 1968). Generational transmission provides the clearest example of this process. The young are enculturated by the previous generation, while they in turn enculturate the next generation. The grandparents don't have to be present to ensure adequate transmission of this general cultural meaning. Each generation simply believes it is describing objective reality.

Hence, institutionalization is both a process and a property variable. It is the process by which individual actors transmit what is socially defined as real and, at the same time, at any point in the process the meaning of an act can be defined as more or less a taken-for-granted part of this social reality. Institutionalized acts, then, must be perceived as both *objective* and *exterior*. Acts are *objective* when they are potentially repeatable by other actors without changing the common understanding of the act, while acts are *exterior* when subjective understanding of acts is reconstructed as intersubjective understanding so that the acts are seen as part of the external world (see Berger and Luckmann, 1967, on "reification" and "objectivation").

Objectification and exteriority often covary, with an increase in one causally producing an increase in the other. Depending on the specific relationship between these two variables, the degree of institutionalization can vary from high to low. Hence, acts may vary in the degree to which they are institutionalized. When acts have ready-made accounts (Garfinkel, 1967), they are institutionalized; that is both objective and exterior. Ready-made accounts won't exist for acts unique to a single actor nor for acts where inter-

subjective knowledge is low. While these accounts are socially created, they function as objective rules because their social origin is ignored (Schutz, 1962). At the same time, ready-made accounts define the *possible*—institutionalization makes clear what is rational in an objective sense. Other acts are meaningless, even unthinkable (Meyer, 1971). Thus, direct social control—whether through incentives or negative sanctions—is not necessary. In fact, applying sanctions to institutionalized acts may have the effect of de-institutionalizing them. They may seem less objective and impersonal, less factual—and the very act of sanctioning may indicate that there are other possible, attractive alternatives.

But acts are not simply either institutionalized or not institutionalized. The meaning of an act may be perceived as more or less exterior and objective, depending on the situation in which the act is performed and/or depending on the position and role occupied by the actor. For example, acts which are dependent on a particular unique actor are low on institutionalization as in personal influence.² In contrast, acts which are performed by an actor occupying a specified position or role are high on institutionalization.

Settings can vary in the degree to which acts in them are institutionalized. By being embedded in broader contexts where acts are viewed as institutionalized, acts in specific situations come to be viewed as institutionalized. Indicating that a situation is structured like situations in an organization makes the actors assume that the actions required of them by other actors in that situation will be those typical of a more formal and less personal interaction. This assumption leads the actors to believe that acts will be more regularized and that the interaction will be more definitely patterned than if the situa-

² Personal influence is used throughout this paper to mean direct influence between actors who perceive each other as equals. Influence between actors who are seen as occupying differentiated roles or who are seen as interacting in a specialized setting governed by consensual rules is *not* considered to be personal influence.

tion were not embedded in an organizational context.

Any act performed by the occupant of an office is seen as highly objectified and exterior. When an actor occupies an office, acts are seen as nonpersonal and as continuing over time, across different actors (Hughes, 1937). In addition, an office increases intersubjective knowledge of appropriate action (Weber, 1947; Berger and Luckmann, 1967). Both the position and role of the occupant establish conditions which maximize treating any act as an accurate reflection of a "fact of life." Thus, acts performed by occupants of an office are by definition institutionalized, though the degree of institutionalization may vary.

In contrast to office, personal influence is dependent on the particular unique actor. There is no rationale under which such an actor can be replaced without changing many of the expectations for behavior. When an actor exercising personal influence leaves the situation, the next actor cannot be classified as having the same quality or qualities. The effect of unique personal influence depends solely on the characteristics of the particular people interacting. In no way is legitimacy derived from other actors or contexts. Each actor is taken as unique and each one influences others independently on that basis. Acts performed by actors exercising personal influence are low in objectification and exteriority, hence low in institutionalization.

Implications of the Ethnomethodological Approach to Institutionalization for Cultural Persistence

As described above, three aspects of cultural persistence are directly affected by institutionalization: transmission, maintenance and resistance to change. Institutionalization is thought to increase all three.

Transmission is defined as the process by which cultural understandings are communicated to a succession of actors. Cultural understandings may be transmitted either in a branching manner, in which each successive actor communicates the meaning to multiple actors, or in a purely

sequential one, producing a chain of actors each of whom communicates the meaning only to the next actor on the chain. In any case, whether transmission occurs within a single "generation" or between "generations," it proceeds from actor to actor independent of any of the earlier transmitting actors.

It is argued here that transmission of acts high on institutionalization is not problematic. The actor doing the transmitting simply will communicate them as objective fact, and the actor receiving them will treat them as an accurate rendition of objective fact. However, depending on the degree of institutionalization of acts, transmission of them will vary. Some transmission will occur with personal influence (as shown in Jacobs and Campbell, 1961); but since acts performed by actors exercising personal influence are low on exteriority and objectification (unique to the particular actor and not transferable to succeeding actors), personal influence will not have as great an effect on transmission as institutionalized context or office. That is, while some transmission occurs with personal influence, increasing objectification and exteriority will increase transmission.

Continuity of the transmission process will also increase institutionalization. The more the history of the transmission process is known, the greater the degree of continuity the actors assume. The history of transmission provides a basis for assuming that the meaning of the act is part of the intersubjective common-sense world. As continuity increases, the acts are increasingly objectified and made exterior to the particular interaction. The act is clearly repeatable, not tied to a unique actor or situation. The basic assumption, then, is that continuity causally produces objectification and exteriority.

Turning now to maintenance, one major assumption is that transmission of acts high on institutionalization is sufficient for maintenance of these same acts. While there are a large number of experimental findings illustrating the central role that direct social control plays in maintaining or modifying behavior, in these experiments the acts to be maintained are generally low on institutionalization and,

further, other methods of modifying or maintaining behavior are not examined. In this theoretical approach, it is argued that the degree of institutionalization radically affects the role and impact of direct social control. For acts low on institutionalization, direct social control (or other intervening mechanisms, such as internalization) is necessary, while for acts high on institutionalization, all that is required is transmission. The institutionalization process simply defines social reality and will be transmitted and maintained as fact.

The third aspect of cultural persistence which should vary with the degree of institutionalization is resistance to change. Acts high on institutionalization will be resistant to attempts to change them through personal influence because they are seen as external facts, imposed on the setting and, at the same time, defining it. Acts performed by actors exercising personal influence, on the other hand, are seen as highly dependent, both on the particular actor and on the particular situation in which the influence attempt is made. Thus, once an act high on institutionalization is transmitted, attempts to change it through personal influence will not be successful and, in fact, may result in a redefinition of the actor rather than the act.

Each one of these aspects of cultural persistence was examined in a separate laboratory experiment in which institutionalization could be varied.

General Experimental Design

The research problem requires a situation in which institutionalization of acts can vary from high to low. While the degree of institutionalization can vary in both ambiguous and unambiguous settings, it can be varied more readily in ambiguous settings (Cicourel, 1964:ch. 7). Hence, this initial experimental investigation is limited to the less problematic ambiguous setting. In addition, it is necessary that all actors in the situation be committed to obtaining an appropriate understanding of the situation (Garfinkel, 1967:ch. 1), though not necessarily committed to the task itself. Further, the task must be one which does not have preexist-

ing relevance to organizational settings, to minimize the possibility of normative, obligatory responses. The more tenuous and unbelievable the connection between the experimental setting and organizational settings, the more likely that differences can be attributed to a cognitive understanding of the "facts of life" rather than obligatory conformity. Finally, the setting must be sufficiently flexible to permit examination of transmission of a preestablished set of norms to actors new to the situation.

A setting which is ambiguous, generates commitment to obtaining a common understanding of the situation, does not have preexisting relevance to organizations, and can be flexibly designed is the autokinetic situation (Sherif, 1935).³ The autokinetic effect is a visual illusion—a stationary pinpoint of light presented in a totally dark room appears to move, smoothly or erratically. In his early studies, Sherif (1935) found that subjects alone developed a judgment standard for the apparent movement and that the particular standard was peculiar to the individual. Over time (three sessions in one week), this standard remained highly stable. He also found that in group situations individuals did not form their own judgment standards but, rather, that the group as a whole established a standard peculiar to that group.⁴

By varying Sherif's basic design, Jacobs and Campbell (1961) developed a transmission situation consisting of a series of stages, referred to as generations, each of

³ There is much support in articles published since Sherif's original study for the claim that the underlying effect is highly variable, including studies of the physiological basis of the effect (Marshall, 1966; Gregory and Zangweil, 1963; Farrow et al., 1965). The effect is so unstructured that it frequently has been proposed as a projective technique (Cornwell, 1966; Rechtschaffen and Mednick, 1955).

⁴ As demonstrated in an earlier study (Alexander et al., 1970), expectations that the light movement will be patterned and stable account for judgmental convergence. When subjects' expectations are altered, either through instructions explaining the autokinetic illusion or through overhearing a confederate give divergent judgments of a light they could not see, it was found that subjects' judgments did not converge. However, Sherif's basic instructions, used in the experiments reported in this paper, lead subjects to exhibit judgmental convergence.

which contains two people, one who has judged the previous set of light exposures and one who is new to the situation. In the first generation, the person identified as having just participated in a preceding generation is a confederate who judges the light as moving considerably farther than subjects do who respond alone to the light (the "control" group). The confederate represents an existing enculturated definer of the situation, transmitting a distance judgment standard to the first subject. In the next generation, the confederate leaves the experiment, a new subject is brought in, and the enculturated subject transmits to the new subject. The same procedure is followed in succeeding generations.

Jacobs and Campbell (1961:342-3) were concerned with "manipulating cultural strength" by varying the number of "culture-bearing" confederates and by varying the number of naive subjects. For each experimental condition, generations were continued until the judgments were approximately equal to those in the control condition (judging light movement alone). While Jacobs and Campbell had anticipated producing strong cultures, their findings did not support this prediction. After the last generation with the confederate, the arbitrary norm was transmitted in some degree only to the fourth or fifth generation. Little difference was found between conditions with different numbers of confederates.

In the research reported here, it was expected that varying institutionalization would produce more striking differences. Instructions were used to create three levels of institutionalization. The higher the level of institutionalization, the higher

the transmission maintenance and resistance to change of cultural understanding expected. Each of the aspects was examined in a separate experiment. Figure 1 presents a summary of the overall design for the three experiments.

TRANSMISSION EXPERIMENT

The Transmission Experiment tests the proposition that the greater the degree of institutionalization, the greater the generational uniformity of cultural understandings. It was predicted that the generational uniformity of cultural understandings would be, at least with personal influence, highest with organizational context and office and intermediate with organizational context alone.

Subjects

A total of 180 female subjects were used, with 45 subjects in each of the three experimental conditions and 45 subjects in the control condition. Three generations were used, with fifteen replications in each condition.

Apparatus and Procedure

The experiment took place in a completely darkened room to facilitate perception of the autokinetic phenomenon. To provide a constant exposure time, the light was operated by a timer, coupled with a motor, as in the Jacobs and Campbell study. Each naive subject was read the instructions for the appropriate condition prior to entering the experimental room. In the *control condition*, the instructions focused on the task, with no

TRANSMISSION

Order of Response	1st Generation	2nd Generation	3rd Generation	MAIN-TENANCE	RESIS-TANCE TO CHANGE ^a
First	Confederate	Subject 1	Subject 2	Subject 3 (Alone)	Confederate
Second	Subject 1	Subject 2	Subject 3		Subject 3

^a Maintenance and Resistance to Change Experiments are conducted when Subject 3 returns one week after the Transmission Experiment.

Figure 1. Experimental Design Summary

information about social characteristics or understandings:

This is a study of visual perception. It requires judgments made with limited information. In a few minutes, you will be brought into the next room. In this room is an apparatus that will project a small light. After the light appears, it begins to move. Let me explain what you will be doing. You must judge the distance that the light moves from the time that it appears until it is turned off. You will repeat this procedure a number of times. Each time the light is shown, you will be asked to judge the distance the light moved in inches along a straight line connecting the starting point and the ending point. After each time the light is turned off, you will be asked to say how far the light moved.

Try to make your judgments as accurate as possible.

Please fill out the card on the desk. As you will notice, it has been numbered. To assure anonymity in this study, each participant is assigned a number and all names are removed from any records.

The *personal influence condition* closely followed the Jacobs and Campbell (1961) instructions and included the full description of the task as in the control condition instructions:

This study involves problem solving in groups. You will be participating with another person. There are already two people at work in the next room. . . .

To simplify the recording procedure, the person who is already in the room will be asked for her judgment first. At this point, you will be asked for your judgment second. After a while, the other person will leave, you will take her place, and a new person will be brought in. Then you will be asked for your judgment first. . . .

The *organizational context condition* instructions incorporated organizational context and continuity (modified from Weick and Gilfillan, 1971), otherwise retaining the same wording as the personal influence condition:

This study involves problem solving in model organizations. You will be par-

ticipating with another organizational member. Your two-member organization is meant to be a small-scale model of much larger organizations, and it has many of the same characteristics they do.

Most large organizations continue even though individual members, or even whole divisions, may be replaced, due to changing jobs, retirement, reorganization, etc. The model organization in which you will participate also will have this feature: members who have been in it for a while will drop out, and new members will join, but the job will go on. Thus, performance of any single member may not be important to the organization as long as the job continues to be done.

There is already a two-member organization operating in the next room. In a few minutes, one of the members will leave, and you will be brought in. After you have worked together with the other member for some time, you will take her place. Then a new member will be brought in as a replacement, becoming part of the organization. . . .

The *office condition* instructions were built directly on the organizational context instructions, adding an office:

Large organizations also place members in different positions, often according to the amount of time spent in the organization. The model organization in which you will participate also has this feature—the member who has spent the most time in the organization will be the Light Operator. Thus, the member already in the next room, having already participated in the organization, will be the Light Operator. When she leaves, you will become the Light Operator.

As you may already have guessed, the group member labeled Light Operator will be responsible for turning the light on after each member gives her judgment on the previous movement. The Light Operator in each case must depress a button to activate the light. From then on, the timing and motion of the light is controlled automatically until the next trial.

To simplify the recording procedure,

the Light Operator will be asked for her judgment first. She will be called Member 1. At this point, you will be asked for your judgment second. You will be called Member 2. After a while, the Light Operator will leave, you will take her place as Light Operator, and a new member will be brought in. Then you will be asked for your judgment first, being called Member 1. . . .

In each case, the subject was asked to fill out a numbered card designed to enhance the continuity manipulation in the instructions. In the personal influence condition, low on continuity, the number assigned to each subject was 3; in the organizational context and office conditions, each subject was assigned number 103; while in the control condition, number 21 was assigned to each subject, representing neither low nor high continuity.

After the subjects were led into the experimental room, they were seated side by side eight feet from the light box. All subjects were blindfolded before entering the room and between generations so that no visual contact with other subjects was made. The experimenter communicated with the subjects using a microphone system and prompted responses with a name or number (depending on condition). In the personal influence and organizational context conditions, the experimenter controlled the timer and prompted responses by using the subject's first name. In the office condition, however, the subject who had been in the room longest controlled the timer placed next to her in the second set of thirty trials. The subject simply depressed a button which activated the light for a fixed period of time. Responses were prompted by "Member 1" and "Member 2" in the first trial, then simply "1" and "2."

The light was presented and the subjects' responses (judgments of the light movement in inches) recorded for a block of thirty trials. After each block of trials, the senior member of the group was taken out, the other member moved to the right seat, and a new member was brought in. In the first generation, the experienced member was a confederate who had been

instructed how to respond. The confederate was instructed to make judgments with a mean of 12 and a range of 9 to 15 (from Sherif, 1967).

After each generation, the subject taken from the room was conducted to an interview room. If she was the last subject in the Transmission Experiment (each group consisted of three generations), she was not interviewed but scheduled to return again one week later.

Results

Control condition. The control condition was designed to provide a baseline for all experimental data collected, with a single subject responding alone to the light for 90 trials. This makes it clear what degree of change occurred when a higher initial standard of judgment was provided by a confederate in the experimental conditions. It was found that the mean response of the 45 subjects in the control condition was highly consistent across the three blocks of 30 trials (4.37, 3.95, 3.95 inches). Thus, unlike results reported by Sherif (1935) and Jacobs and Campbell (1961) which showed a steady and significant decline in judgments, the mean judgments of the 45 control subjects in this experiment did not show a significant decline. Hence, the baseline response can be defined as the average of all judgments (90 trials): 4.16 inches.

Experimental conditions. The predictions that transmission (and, hence, generational uniformity) would be greater in the organizational context condition than in the personal influence condition, and greater in the office condition than in the organizational context condition are examined in two main ways. First, the response levels of successive naive subjects are used to define a transmission coefficient, permitting prediction of response levels of later generations. Second, mean response levels of naive subjects over the three generations are compared to test the predicted ordering of the experimental conditions.

Turning first to the discussion of the transmission coefficient, the use of mean response levels of successive naive (new)

Table 1. Transmission Coefficients Defining Rate of Decline of Response Level for Each Experimental Condition

Transmission Coefficients	Institutionalization		
	Personal Influence	Organizational Context	Office
T ₁ (First to Second Generations)	.49	.87	.92
T ₂ (Second to Third Generations)	.37	.89	.97
T (Average Coefficient)	.43	.88	.94

subjects to predict response levels of later generations was suggested by Jacobs and Campbell's (1961) data. Although not noticed by them, in their data for all four conditions the relative rate of decline in response level toward the baseline remains essentially constant between generations in the same conditions. This is most clearly illustrated in their Figure 2 (Jacobs and Campbell, 1961:345): with a baseline of 3.8 inches, the naive subject made mean judgments (in 30 trials) of 12.4, 9.3, 7.1 and 5.8 inches. Examining the ratio of elevations above the baseline in successive generations, it can be seen that it remains essentially constant:

$$\frac{9.3-3.8}{12.4-3.8} = .64,$$

$$\frac{7.1-3.8}{9.3-3.8} = .60,$$

$$\frac{5.8-3.8}{7.1-3.8} = .61.$$

In designing the experiment reported here, it was assumed that this decline, so characteristic of the Jacobs and Campbell data and also clear in Sherif (1967:264-8), is a general characteristic of generational transmission in the autokinetic situation. The essentially constant geometric decline toward the baseline permits prediction of the response levels of later generations, so that it is not necessary to collect data on all generations.

Before turning to the specific results in this experiment, the measure of relative rate of decline in response levels should be more generally expressed as a transmission coefficient. This measure of transmission is based on a comparison of

the response levels of the naive (new) subjects in each generation, where response level is defined as the mean of a subject's judgments over thirty trials. The transmission coefficient then can be defined as the ratio of elevations above the baseline of successive naive subjects:

$$T_i = \frac{\bar{S}_{i+1} - B}{\bar{S}_i - B} \quad (i = 1, 2)$$

where \bar{S}_i is the average response of the i th subjects in the naive position and B is the baseline response. One desirable property of the transmission coefficient defined in this manner is that it permits prediction of the generation at which the response level will approximate the baseline allowing a more direct reflection of experimental findings. Since actual experimental data cannot be expected to be completely constant, an average transmission coefficient defined as

$$T = \frac{T_1 + T_2}{2},$$

will be used.

Table 1 presents the average transmission coefficients (T) for the three generations in this experiment.⁵ The transmission coefficients for each pair of generations (T_1 and T_2) in each condition were essentially constant. There was a greater difference between the coefficients in the

⁵ While it is technically possible to use two generations—one ratio of elevation above the baseline—three generations were used to test the assumption that the decline was also constant in the experiment described here.

personal influence condition than between those in the other two conditions, possibly because the response level in the third generation closely approximates the baseline response.

Figure 2 presents the extrapolated response levels based on the average transmission coefficient, T , for each experimental condition, as well as the actual experimental data on which the coefficients were based. The predicted differences between the three conditions are clearly found. In the personal influence condition, the extrapolated response level approximates the baseline response in the 7th generation, while in the organizational context condition, the extrapolated response level doesn't approximate the baseline response until the 29th generation. Even in the 38th generation, in the office condition, the extrapolated response level doesn't yet approximate the baseline response.

Now direct comparisons between the mean response levels of naive subjects will be made to permit significance tests of

the predicted ordering of the experimental conditions. Table 2 presents these mean response levels by condition. The predicted ordering for the magnitude of response level was obtained:

Personal influence <
organizational context < office.

In the personal influence condition, the mean response level of naive subjects declined rapidly so that it was only slightly above the baseline by the third generation (4.58 compared to 4.16 for the baseline response).

In order to evaluate the magnitude of the differences in response level by condition, analysis of variance was used. Because comparisons between different levels of institutionalization involve differences between generations as well as differences associated with institutionalization itself, customary two-way analysis of variance is not appropriate. Instead, analysis of variance for the special case of the two-factor experiment with repeated measures on one factor is used in Table 3

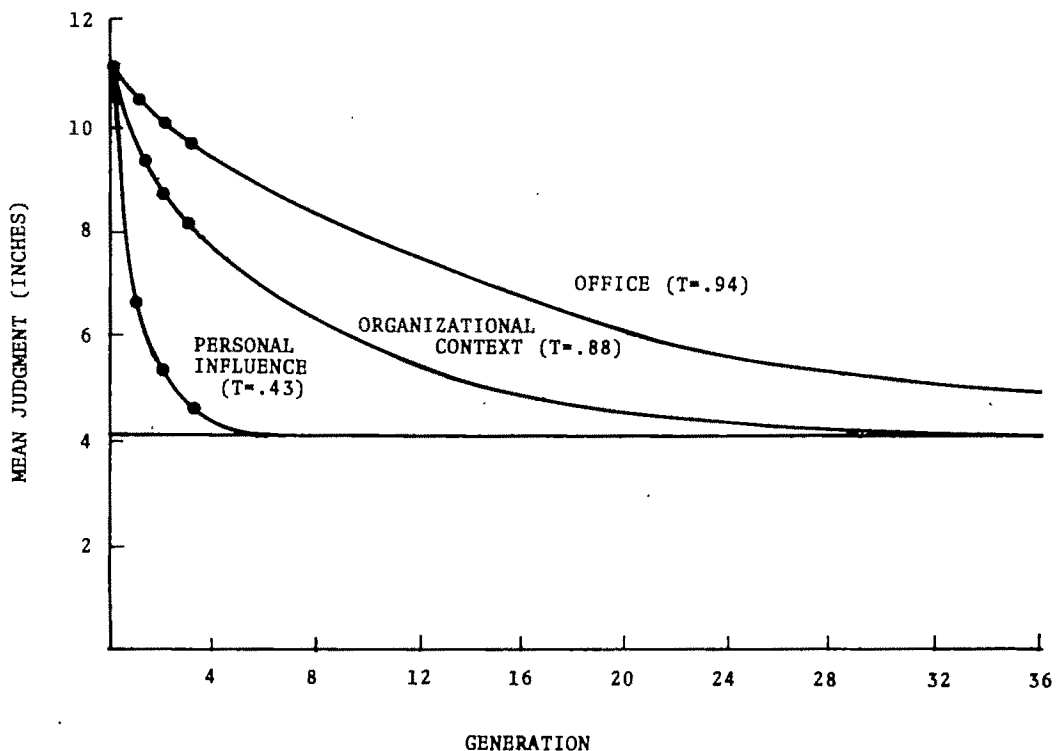


Figure 2. Extrapolated Response Levels Based on Transmission Coefficients Determined by the First Three Generations in Each Condition

Table 2. Mean Response Level of Naive Subjects over Three Generations in the Transmission Experiment

Generation	Institutionalization		
	Personal Influence	Organizational Context	Office
First	6.53	9.44	10.51
Second	5.31	8.77	10.00
Third	4.58	8.25	9.79

(Winer, 1962:298-318). The data provide clear support for the statistical significance of the main effects due to institutionalization.⁶

To recapitulate briefly, the Transmission Experiment tests the proposition that if institutionalization is increased, then generational uniformity of cultural understandings will be greater. From the preceding analysis of the data, it is clear that the effects of institutionalization occurred as predicted. Further interpretation of results will be deferred until all three experiments have been described. Before turning to the Maintenance Experiment, results bearing on the effectiveness of the experimental manipulations will be reported.

Effectiveness of the Experimental Manipulations

As described earlier, the instructions read at the beginning of the experiment were designed to manipulate the degree of institutionalization. Since this is the only

manipulation of the independent variables for all three experiments reported here, it is crucial to assess its effectiveness. Therefore, rather than attempt to assess this directly, a number of other variables which were expected to vary directly with the degree of institutionalization were chosen to validate the manipulation. Specifically, it was predicted that increasing institutionalization should increase the subjects' subjective certainty of the accuracy of their judgments, subjects' reported ease in estimating light movement, and subjects' reported expectations that their answers should be the same as those of the experienced subject (the transmitter of cultural understandings). In addition, it was predicted that the occupant of the office position would be seen as less personal; the highly institutionalized position should increase the role distance and significantly affect perception of attributes of the occupant of that position.

Parts of the postexperimental questionnaire and the adjective list (given before the questionnaire) were designed to determine whether these additional dependent variables designed to measure validity did indeed vary with the degree of institutionalization. Turning first to the questionnaire results, subjects were asked three questions reflecting perception of

⁶ The mechanisms underlying differential transmissions were also explored, comparing response levels of the same subjects when in the naive and experienced positions. Results available from the author by request.

Table 3. Analysis of Variance Test on the Mean Response Level of Naive Subjects

Source of Variance	Sum of Squares	df	Mean Square	F	Significance Level*
Between Groups					
A (Condition)	514.75	2	257.37	74.83	p<.001
Subjects within Groups	144.45	42	3.44		
Within Groups					
B (Generation)	37.96	2	18.98	9.42	p<.001
B x Subjects within Groups	169.30	84	2.02		

* Under the more conservative, negatively biased test.

institutionalization. First, subjects were asked: "How certain were you of the accuracy of your judgments?" They were shown a separate card on which was drawn a seven-point scale, where one represented "Certain" and seven indicated "Uncertain." Certainty was predicted to increase with increasing institutionalization, because it reflected certainty in the accuracy of the group standard, not individual judgment. Subjects in the more institutionalized conditions were predicted both to be more certain of their judgment and to change more toward the confederate's judgments from the baseline response in the Transmission Experiment, a prediction contrary to the assumption made in many psychological studies where certainty is thought to increase resistance to change (Boomer, 1959).

Second, subjects were asked about ease of judgment: "Was it difficult to estimate the distance the light moved?" It was predicted that the more institutionalized and, therefore, certain about accuracy, the easier estimating light movement should be. In the absence of objective criteria for determining difficulty of judgment, assessment of difficulty would rest on a trial-by-trial examination of difficulty in reaching a decision. Where certainty in the accuracy of group standards is high (in the more institutionalized conditions), decisions should be less difficult. Hence, in this case, certainty determines difficulty rather than difficulty determining certainty as most studies assume (Coleman et al., 1958; Kretch and Crutchfield, 1962; Freedman et al., 1974).

The third question asked in the postses-

sion interview which reflected perception of institutionalization was: "Did you feel that your answers *should* be the same as the other person in the first set of judgments?" It was predicted that the greater the degree of institutionalization, the more frequently the subject would report that her responses should be the same as the other person in the "experienced" or transmitter position. The more institutionalized, the more subjects should feel normatively constrained to transmit the confederate's standard.

The responses on these three questions on the postexperiment questionnaire are summarized in Table 4. Each of the three sets of findings is in the predicted direction. Subjects were much less certain of their accuracy in the personal influence condition than in either of the more institutionalized conditions. While subjects in the organizational context condition were less certain of their accuracy than subjects in the office condition, the difference was not striking. Turning now to the responses to the question, "Was it difficult to estimate the distance the light moves," as predicted, fewer subjects in the office condition reported difficulty than in the organizational context condition. Nearly all subjects in the personal influence condition reported difficulty. Finally, over half of the subjects in the personal influence condition did *not* feel normatively constrained to give the same response as the confederate, compared to less than a quarter of the subjects in the organizational context condition. Only seven percent in the office condition did not feel normatively constrained.

Table 4. Questionnaire Responses by Experimental Condition ^a

Question	Institutionalization		
	Personal Influence	Organizational Context	Office
Mean Certainty Score ^b	5.35	3.62	3.22
Number Reporting Judgment Difficulty	41	26	16
Number <i>Not</i> Feeling ^c Answer Should Be Same As Experienced Subjects	28	11	3

^a N in each condition is 45.

^b The smaller the number, the more certain of own judgment accuracy. Scale is from one to seven.

^c Question reversed so that all predicted magnitudes in the table would be in the same direction.

Table 5. Differences in Response Level between the Last 30 Trials in the Transmission Experiment and the 30 Trials in the Maintenance Experiment

Difference between Transmission and Maintenance Experiments	Institutionalization		
	Personal Influence	Organizational Context	Office
Third Subject	.78	-.08	-.47

Turning now to the adjective checklist, it was designed primarily as a check on the office manipulation. Six adjective-pairs were chosen to indicate role distance and the nonpersonal nature of office. Comparing the person occupying the experienced position in the office condition with the person occupying that position in the personal influence condition for each of these adjective pairs, the subjects in the office were characterized more frequently as decisive, cold, unsociable, dominant, methodical and unemotional. Each of these differences, except the unsociable-sociable comparison, is significant by the t-test.⁷

MAINTENANCE EXPERIMENT

The Maintenance Experiment was designed to test the proposition that the greater the degree of institutionalization, the greater the extent of maintenance of cultural understandings without direct control. The subjects in the personal influence condition were expected to maintain the Transmission Experiment response level least well, with subjects in the office condition maintaining it to the highest degree and subjects in the organizational context condition intermediate.

Subjects

The third subject from each group in the Transmission Experiment returned one week later, hence the total subjects available for the Maintenance Experiment were 15 subjects per condition, for a total of 45 subjects.

Apparatus and Procedure

The same apparatus was used as in the Transmission Experiment as well as the same procedures, with the following exceptions: (1) It is not a transmission situation, hence the generational design was not used. (2) The subject responded alone for the thirty trials.

The subject was read instructions (the same for all subjects in the Maintenance Experiment, regardless of experimental condition) identical to those used in the *control condition*, except that they were prefaced with "As you will remember. . . ." The subject was told that the other person scheduled for the same time had not arrived yet and in order to refresh her memory about the task, she would judge the light movement alone until the other person arrived.

Results

Analysis of the results is based on a comparison between the same subject's responses in the last thirty trials in the Transmission Experiment and the thirty trials in the Maintenance Experiment. The mean differences in response levels between these two phases are presented in Table 5. The ordering of magnitude of the differences is as predicted:

Personal influence <
organizational context < office.

However, it was not predicted that the means would actually increase very slightly in the organizational context and office conditions.⁸

⁸ Absolute differences are used because any changes, even an increase in response level, are due to the subject's perception of the common understanding in the situation. The slight increase in

⁷ Tables available from the author by request.

Table 6. Analysis of Variance Test on the Difference in Response Level of the Same Subject in the Transmission and Maintenance Experiments

Source of Variance	Sum of Squares	df	Mean Square	F	Significance Level
Between Groups	12.35	2	6.17	5.27	$p < .01$
Within Groups	49.18	42	1.17		

Analysis of variance was used to examine the difference between response levels of the same subject in the transmission phase and the maintenance phase. These results are presented in Table 6. The data provide clear support for the statistical significance of the main effects on maintenance of cultural understandings due to institutionalization.

Resistance to Change Experiment

The Resistance to Change Experiment was designed to test the proposition that the greater the degree of institutionalization, the greater the resistance to change in cultural understandings through personal influence. A change in the definition of the situation is attempted.

After the trials alone (Maintenance Experiment), a confederate was brought in, identified as the late other subject. The confederate, responding first, attempted to establish a lower-than-baseline response level, permitting measurement of resistance to change. The confederate's judgment had a mean of approximately 1.5 and a range of $\frac{1}{2}$ to $2\frac{1}{2}$ inches. It was predicted that judgments in the personal influence condition would be less resistant to change than judgments in the organizational context condition, and that judgments in the organizational context condition would be less resistant to

change than judgments in the office condition.

Subjects

The Resistance to Change Experiment was a continuation of the Maintenance Experiment and, therefore, had the same subjects.

Apparatus and Procedure

Again, the same apparatus was used. The same procedure was used as in the Maintenance Experiment except that a confederate was brought in. Thirty trials each were given, the confederate responding first, since "she didn't have the refresher session." No other instructions were given.

Results

A comparison between the thirty trials in the Maintenance Experiment and the thirty trials for the same subject in the Resistance to Change Experiment provided the basis for analysis of the results. The mean difference in response levels between the two experiments are presented in Table 7. The ordering is as predicted, with the personal influence condition having the greatest change, the organizational context condition less change and the office condition the least change.

Analysis of variance was used to examine the differences between response levels of the same subject in the Maintenance Experiment and the Resistance to Change Experiment. Table 8 presents the results. The data provide clear support for the predicted effects of institutionalization on resistance to change. Thus, the analysis of results in the Resistance to Change Experiment uniformly support the predictions made.

means in the two more institutionalized conditions is probably due to a "ceiling effect." Responses overall were maintained to a high degree. When developing the design used here, it was assumed that judgment standards artificially established through a confederate plus a one-week time span would be less well maintained than earlier studies found. Such was not the case. In future experiments, the problem of near-perfect maintenance may be avoided by using a more structured, less ambiguous setting in which an artificial definition of the situation would be less well maintained.

Table 7. Difference in Response Level between the Maintenance and Resistance to Change Experiments

Difference between Maintenance and Resistance to Change Experiments	Institutionalization		
	Personal Influence	Organizational Context	Office
Third Subject	1.59	1.03	.05

CONCLUSIONS AND IMPLICATIONS

The predictions derived from an ethnomethodological approach to institutionalization receive strong support in the results of the three experiments reported here. Each specific finding has a number of implications for research on institutionalization and poses significant issues which have not been sufficiently addressed in other, more traditional, theoretical approaches to the problem of cultural persistence.

Transmission of Cultural Understandings

Given that cultural understandings are socially constructed, the problem is to explain why some are so permanent and universal while others are unique to person, place or time. The Transmission Experiment was designed to test specific ideas about the mechanisms underlying generational uniformity, identifying the degree of institutionalization (both objective and exterior) as a determinant of this uniformity.

A number of alternative explanations of the results could be made, but none are as convincing as the ethnomethodological explanation on which the experiment was based. Specifically, creating an organizational context may implicitly lead the naive subjects to believe that they are expected by the experimenter to conform to the "more experienced" subject. On the basis of the experimental procedures alone, this seems unlikely, since in every condition naive subjects know at the out-

set that they will occupy the other position. Further, the task itself is unrelated to organizational settings. The strongest refutation, however, can be made on the basis of the assessment of the effectiveness of the experimental manipulations. Other aspects of institutionalization—unrelated to organizational context—were found to covary with degree of institutionalization: certainty of accuracy, reports of difficulty in judging light movement, and expectations that answers should be the same as the "experienced" subject.

Status value theories (Berger et al., 1966; 1972) would predict the results obtained for the office condition alone, though not fully since the naive subjects know they will occupy the office after one set of trials. However, since positions are differentiated, it could be argued that the occupant of the low position is more likely to be influenced by the occupant of the high position. However, the scope of the theoretical approach presented here is quite different, predicting not only the direction of influence in the office condition, but also the relative magnitude of influence which depends not on status value but on institutionalization.

Maintenance of Cultural Understandings

Maintenance frequently has been described as occurring only when direct social control is present. Direct sanctions produce compliance; as long as the expected behavior is in the actor's self-

Table 8. Analysis of Variance Test on the Difference in Response Level of the Same Subject in the Maintenance and Resistance to Change Experiments

Source of Variance	Sum of Squares	df	Mean Square	F	Significance Level
Between Groups	18.23	2	9.11	16.27	p < .001
Within Groups	23.53	42	.56		

interest, it will be maintained. Even the internalization and self-reward conceptions depend on direct sanctions to establish the behavior initially and/or to maintain the behavior in those actors not fully "socialized."

In the theoretical approach presented here, it is asserted that the more institutionalized, the greater the maintenance without direct social control. The effects of institutionalization can be seen most clearly when no sanctioning process is present. In the experiments reported here, social influence was present, but should operate uniformly in all conditions. The differences found, then, can be attributed to the relatively high dependence of acts low on institutionalization on social control mechanisms compared to the strikingly low dependence of acts high on institutionalization. The greater the degree of institutionalization, the less likely sanctions will exist. For example, laws regulating black-white interaction in the South were enacted only after the institution of slavery was challenged (Woodward, 1957).

Resistance to Change

Resistance to change is fundamentally affected by institutionalization, regardless of sanctions. The confederate's below-the-baseline responses serve to positively sanction lower responses by the subjects, yet it was found that only when the degree of institutionalization was low (the personal influence condition) did this sanctioning cause the subjects to reduce their response to below the baseline.

These results are, on one level, nonintuitive, since the subjects closer in response level to the confederate change the most. Subjects in the personal influence condition essentially adopted the new judgment standard: the situation was redefined. However, those subjects farther from the confederate's response level changed much less. As predicted, they resisted the confederate's attempt to redefine the situation.

The results of this experiment contradict other approaches most directly. In other approaches, resistance to change, regardless of source or content, comes di-

rectly or indirectly from the strength of the previous sanction. The subsystem approach assumes that resistance to change is a function of the distribution of rewards; if more rewards are associated with one action than with another, the more highly rewarded action will be exhibited. Therefore, those actions that are more highly rewarded will be more resistant to change. The normative framework approach argues that resistance to change is a function of internally generated motivation, either internalization or self-reward. Generally, this internal motivation is seen as deriving from previous sanctions applied to some actions and not to others. Neither of these traditional approaches to institutionalization could predict or explain the results obtained in the Resistance to Change Experiment.

General Implications

The findings reported in this paper are, thus, both consistent and inconsistent with previous arguments, serving to modify parts of the traditional approaches to institutionalization. However, the findings reported here do not suggest rejection of the traditional approaches but, rather, serve to conditionalize them by restricting the set of situations to which they apply. That is, it is not that these other approaches should be rejected but, rather, that the class of situations to which they apply should be more precisely specified. These other approaches do not deal adequately with highly institutionalized action: the mechanisms they use are more important when the degree of institutionalization is low.

The theoretical approach developed in this paper, based on the ethnomethodological approach, provides a more complete explanation of highly institutionalized action. In three experiments, persistence of cultural understandings has been shown to vary directly with the degree of institutionalization. The degree of institutionalization, depending on personal influence, organizational context or office directly affected three major aspects of persistence: generational uniformity, maintenance and resistance to change. Thus, a theory which explains highly in-

stitutionalized action also permits more accurate and complete predictions of cultural persistence.

In conclusion, the findings reported in three experiments provide strong and consistent support for the predicted relationship between degree of institutionalization and cultural persistence. As predicted, it was found that the greater the degree of institutionalization, the greater the generational uniformity of cultural understandings, the greater the maintenance without direct social control, and the greater the resistance to change through personal influence.

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SOCIETAL REACTION TO DEVIANTS: THE CASE OF CRIMINAL DEFENDANTS *

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Recent reformulations of the societal reaction theory argue that the thesis is a perspective rather than a theory, and that the perspective is meant to provide a set of sensitizing concepts to those researching deviance. This research examines the degree of congruence between hypotheses deduced from those assertions and a set of real world occurrences. Data for a sample of male defendants charged with felony offenses are examined to estimate the effects of (1) deviants' social attributes, (2) the specific societal reactors, (3) the values placed on certain offenses and (4) the organizational imperatives of the deviance-controlling organization, controlling for the alleged offense, on the probability of being labeled and sanctioned for deviant behavior. Our analyses indicate that characteristics associated with the alleged offense account for more of the explained variance in the labeling decision examined here (full prosecution) than in the sanctioning (sentence severity) decision. Moreover, while we find the deviants' social attributes do have some significant effects, relative to the effects of other variables, these effects are small and not always in the predicted direction. We suggest the interactionist perspective shift its focus toward greater attention to organizational imperatives and the values and expectations of those meting out the societal reaction as key variables explaining the imperfect correlation between deviant acts and the reaction to same.

Beginning with the work of Tannenbaum (1938) and Lemert (1951), a central concern for the study of deviance has been the delimitation of factors that affect the decisions and actions of deviance-controlling organizations and the conse-

quences of these decisions and actions for persons labeled as deviants. This concern is motivated by a theoretical interest in the way in which discretion is manifested in the societal reaction to deviants (Pound and Frankfurter, 1922; Becker, 1963; Turk, 1969) and by a methodological interest in the role of discretion in the production of deviance statistics and deviance categories used in sociological research (Garfinkel, 1956; Kitsuse and Cicourel, 1963).

While interest in the manifestation of discretion continues, it is now generally agreed that the core writings articulating the societal reaction thesis should not be

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treated as a formal theory. Rather, it is argued that these writings provide a set of "sensitizing concepts" relevant to the study of deviance (Schur, 1971; Becker, 1973). However, acceptance of the idea that what was formally termed "labeling or societal reaction theory" is not a *theory*, does not preclude the still unmet need for empirical examinations of the congruence between hypotheses deduced from these "sensitizing conceptions" (or, as Becker, 1973, terms it, interactionist perspective) and real world occurrences. It is to this task that this research is addressed.

A review of the works generally construed as representative of the societal reaction thesis (e.g., Lemert, 1951; Becker, 1963; Erikson, 1964) as well as the recent modifications and reformulations of same (e.g., Lofland, 1969; Schur, 1971; Becker, 1973; Goode, 1975) reveals consensus in emphasis, and relative agreement on a core set of assertions. The emphasis mandates that the study of deviance include attention to the process by which purported rule violators come to have deviant status conferred upon them. The core assertions include: (1) the definition of persons as deviant is a constructed definition resulting from a set of interactive processes (Lofland, 1969; Becker, 1973; Goode, 1975); (2) the societal reaction to deviants is not a direct result of the alleged deviant act (Erikson, 1964; Becker, 1963; 1973; Kitsuse and Cicourel, 1963; Schur, 1971); (3) the societal reaction to deviants varies with the social attributes of the alleged deviant (Becker, 1964; Quinney, 1970); (4) the societal reaction to deviants varies with the organizational imperatives of the deviance-controlling organization (Schur, 1971; Becker, 1973), with the persons doing the reacting (Becker, 1973), with the expectations and values of the reactors (Turk, 1969; Schur, 1971), with the deviants' ability to avoid the imposition of the deviant label (Schur, 1971) and with a variety of other ancillary factors (Goode, 1975).¹

¹ In addition to the above, there are a set of core assertions that relate to the effect the deviant label has upon subsequent deviant behavior. Our analyses do not address these issues. For a review of research that does, see Gove (1975).

Despite the relative agreement as to the emphasis and core concepts, there is little agreement between proponents and critics as to what are the empirically testable hypotheses logically deduced from these concepts. For example, Tittle (1975) argues that one of two theoretically provocative hypotheses is that social attributes of the alleged deviant explain *more variance* in the societal reaction than does the alleged rule-violating behavior. Schur (1975), however, contends that the very hypothesis that Tittle rejects as uninteresting is the one in which interactionists are interested, i.e., social attributes of the alleged deviant affect the societal reaction to deviants (regardless of the size of their effect relative to other variables). Schur concurs with Becker (1973) in arguing that the intent of interactionists is to expand the amount of explained variance in the societal reaction, not to restrict it to a thesis that makes the social attributes of the alleged deviant the major determinative factor. This disagreement spills over to the evaluation of the extant research. For example, in Tittle's (1975) review of research on the reaction to alleged law violators, he concludes there is little support for the hypothesis that social attributes of the deviant explain more variance than the alleged criminal act. Furthermore, he cites Hagan's (1974) review of extra-legal attributes and sentencing to buttress his inclination to conclude that the data don't support his version of the interactionist thesis. However, Tittle's acknowledgement of the methodological limitations of the extant research, coupled with the disparity between his and Schur's definition of the critical hypothesis, precludes him from reaching definitive conclusions about the explanatory power of the interactionist perspective.

Insofar as the interactionist perspective (whether a theory, a set of sensitizing concepts, or a perspective) continues to dominate deviance research, we think it would be most fruitful to examine the degree to which empirical data are consistent with the hypotheses that most closely conform to the assertions of those proposing them. By so doing, we hope to broaden the knowledge base and reduce

the likelihood that empirical findings will be disregarded on the basis of their addressing the wrong questions.

In accordance with our review of interactionist writings, we take a central question to be, controlling for the alleged deviant act, what other factors account for explained variance in societal reactions? Specifically, we deduce the following hypotheses to explore the degree to which interactionists' assertions properly specify and emphasize the relevant determinative factors: (1) indicators of the alleged deviant act don't account for all of the explained variance in societal reactions; (2) social attributes of the alleged deviant account for some of the explained variance (the expectation being that the socially disadvantaged will be responded to more negatively); (3) organizational imperatives account for some of the explained variance, as do the individual persons doing the reacting, the values of the reactors and other ancillary factors.

Before proceeding, we need to acknowledge an obvious leap we make here from the interactionists' assertions to our own specification of research questions. Recall that the emphasis mandated was that the study of deviance attend to the process. Accordingly, a methodological preference for field observations and qualitative analyses is often expressed. While we grant the value of these methods, we contend that quantitative analyses of the same or related questions are not precluded. Gibbs (1972:47), for example, argues that if the ratio of persons formally identified as deviants to those labeled as deviants (e.g., arrested/convicted) is not 1:1, the basis for that disparity needs to be empirically explored. Becker (1973:16-7), Kitsuse (1975) and Schur (1975) articulate a commitment to the value of quantitative analyses that address interactionist questions. The most compelling justification, however, comes from Goode (1975:579) in his call for probability estimates of varying societal reactions, given various conditions: "A completely situational view of deviance can be intellectually paralyzing. The probabilistic view rescues us from the solipsistic logical extreme of absolute situational relativity. . . ." Thus, while we have leaped from the interactionist's as-

sertions to a set of questions couched in terms most congruent with multivariate analyses, the justification for so doing can be well documented.

Research Setting

The criminal justice system is a strategic arena in which to research interactionist questions because there is general consensus that criminal justice decisions, e.g., arrest, severity of sentence, are labeling decisions, i.e., decisions that can be taken as valid indicators of formal societal reactions. Moreover, criminal justice decisions occur in sequence. As such, in examining the bases for one decision, one also can examine the effect of a prior decision. To illustrate: in examining the bases for sentence severity, one can consider a defendant's release status prior to trial, a status that itself represents the culmination of a prior deviance processing decision. Finally, the fact that the criminal justice system operates like a sieve, filtering out defendants at each stage of the process (Blumberg, 1967; Rosett and Cressey, 1976) makes it amenable to analyses organized more like a tree than a table. Schur (1971) and Hagan (1974) underscore this point, noting that the processing of deviants involves a series of decision-making stages, and not all deviants continue through all of the stages. As such, processual analyses that begin with a sample of deviants, and examine sequential decisions where the sample for whom the decision is relevant decreases with each new decision, can bring to light the manifestation of discretion at different stages in deviance processing.

Sample

Our sample consists of all males arraigned in a city in New York State, from December, 1974 to March, 1975, whose most severe arrest charge was a *felony charge*, whose cases were not disposed of at first court presentation² and whose

² Seventeen percent of persons arrested for felonies are finally disposed of at their first court presentation, i.e., within 24 hours after the arrest.

cases were finally disposed of in criminal court within the four-month period of observation, by a judgment other than an acquittal, by judges who disposed of more than one percent of the cases ($N = 1,213$).³ For each of these 1,213 defendants, court record data were recorded daily for the judicial disposition of every court appearance. Data on the defendant's criminal history were recorded from state criminal records and data on defendant's demographic characteristics from personal interviews conducted during the 6–24-hour period immediately following the defendant's arrest (while he was in custody) and preceding his first arraignment hearing. Data on the characteristics of the criminal offense were obtained from court records.

In addition to the above, complementary qualitative data were collected by the senior author through court observations and interviews with judges, prosecutors, defense attorneys and auxiliary court personnel. The qualitative observations were used to determine (1) which exogenous variables to include, (2) the appropriate way to code these variables and (3) major interpretations of results.

Since the disposition process is so truncated, we analyzed data for this group separately. Our findings indicate that the factors that affect the three disposition decisions for these defendants are quite different from those affecting the same decisions for those not so rapidly disposed. These additional data may be obtained from the senior author.

³ The criminal court in the city from which these data come is a misdemeanor court. As such, only cases where the conviction charge is less than a felony as included here, despite the fact that the arrests were all for felonies. However, we can estimate that our sample of felony cases represents about 92% of total felony arrests, since we know that in the observation period, only 8% of the cases were waived to the grand jury for indictment and supreme court processing. Female defendants are excluded because of the developing literature on women in crime (e.g., Brodsky, 1975; Simon, 1975). Since we could not attend to the theoretical propositions of that literature, we exclude them here. Similarly, persons arrested for misdemeanors are excluded here and analyzed in another paper. Acquittals were excluded because there were too few cases acquitted. Finally, we only include cases disposed of by judges who disposed of more than 1% of the cases such that we could reduce the number of relevant judges from 52 to 17, clearly a more manageable number.

Endogenous Variables

Three endogenous variables are examined. While these three variables don't represent all of the decision stages, they do represent the critical formal reactions of the collective audience once the deviants have been brought before the deviance-controlling organization.

For all defendants in our sample ($N = 1,213$), we examine the decision to fully prosecute a case or to terminate the case by dismissal (Y_1). Since nearly forty percent of our defendants (and comparable proportions of other samples of defendants, e.g., Hagan, 1975; Zeisel et al., 1975) are dismissed, research examining the bases for sentencing decisions must examine the bases for the prior decision that determines whether a defendant will be eligible for sentencing. With the exception of Burke and Turk (1975) Hagan (1975) and Zeisel et al. (1975), most prior research on sentencing (e.g., Chiricos and Waldo, 1975; Swigert and Farrell, 1977) ignores this important prior selection process.

For those defendants whose cases are not terminated by a dismissal ($N = 733$), we examine whether the defendant was adjudicated guilty and sentenced, or whether the defendant was adjudicated guilty but not formally convicted and thus not subjected to a sentencing decision (Y_2). Our data come from a criminal justice system that has formalized this second decision in its ACD statute (adjournment in contemplation of dismissal). Defendants whose final disposition is an ACD are adjudicated guilty but not formally convicted unless they are rearrested and charged with a new offense in the six-month period following the original ACD disposition. Since their record carries no conviction, they receive no sentence. This is a particularly interesting labeling decision since *both* those given ACDs and those moved on for sentencing are presumed guilty as charged.

Finally, for those defendants whose cases were not terminated by a dismissal or an ACD ($N = 510$), we examine the severity of the sentence meted out as a measure of the severity of the formal societal reaction (Y_3). Sentence severity is

analyzed as an ordinal scale (see Y_3 , Table 1). The determination of the order is in accordance with that specified by the judges, prosecutors and defense attorneys whom we interviewed.

Exogenous Variables

Since we define our research as exploratory, a large number of exogenous variables are examined in the preliminary stage of our analyses. We included variables related to the defendant's social attributes, e.g., race, age, education, marital status; variables that might determine the reactor's expectations for and perceptions of certain deviants, e.g., the defendant's prior criminal record; variables related to the organizational imperatives of the deviance-controlling organization, e.g., the defendant's cooperation during the arrest, the defendant's acceptance of guilty plea offers; variables related to the individual's doing the reacting, e.g., judges; and variables summarizing the results of prior processes, e.g., the defendant's release status pending his final disposition. In addition, variables related to the alleged offense, e.g., the type of crime, number of charges, and severity of the charges are included. A list of exogenous variables and the way in which each is coded is presented in Table 1.

Analyses

The data are analyzed using dummy variable regression procedures. The general appropriateness of these techniques is reviewed in Cohen (1968) and Kerlinger and Pedhazur (1973). Since we define our research as exploratory, nominal variables are effect-coded (Kerlinger and Pedhazur, 1973:172-85). That is, comparisons are made between each category and the mean of the other categories, rather than between one category and some arbitrarily selected left-out category.

Since there are no published data on a number of variables here considered, we examined first the zero-order correlations between all of the exogenous variables and the three endogenous variables. No problems of multicollinearity were appar-

ent. In the regression equations on which Table 2 is based, only variables whose net effects were statistically significant at .10 were included.⁴ An exception to this is that certain variables were deemed control variables, e.g., the severity of the most severe arrest charge and the type of crime; as such, they were entered into every equation. Finally, the regression coefficients presented represent the coefficients from the equations where the appropriate judges have been stepped in. A comparison of the coefficients before and after controlling for judges revealed little changes in the coefficients. However, the procedure was kept to provide a measure of control for variation by judge and an estimate of the increased variance explained by judges.

Results

Table 2 presents the regression coefficients for those exogenous variables that had net effects on the first endogenous variable (Y_1). If a variable appears in Table 1 and not in Table 2, that variable did not have a statistically significant effect.

According to Table 2, the likelihood of being dismissed is increased if: (1) the defendant's most serious arrest charge was a burglary or assault charge; (2) the defendant's total number of arrest charges was lesser rather than greater; (3) the defendant was detained in jail while awaiting his final disposition; (4) the defendant's felony charge was reduced to a misdemeanor at the latest possible opportunity, i.e., at or after his preliminary hearing.

Since dismissal is purportedly a function of the strength of the evidence (Miller, 1970), we discuss first those findings interpretable as reflective of evidentiary concerns. The fact that we find defendants charged with burglary more likely to have their cases dismissed may be a function of burglary cases being difficult to prosecute

⁴ Our selection of .10 reflects our concern for using too stringent criteria in exploratory work, the possibility of a type II error, and a concern for the continuing debate about the use of significant tests (Morrison and Henkel, 1970) as the determinant of which variables to include in a model.

Table 1. Variables, Notation, and Frequencies

Notation	Variable	Scale	Frequencies (1,213)
Y ₁	Disposition— Dismissal	Not dismissed (0) Dismissed (1)	60.5% 39.5%
Y ₂	Disposition— A.C.D.	Not A.C.D. (0) A.C.D. (1)	81.5% 19.5%
Y ₃	Disposition— Sentence	Discharge (1) Time served (2) Fine only (3) Fine under \$50 with jail default (4) — Fine over \$50 with jail default (5) Probation (6) Jail (7)	24% 4% 2.5% 6.5% 19% 16% 28%
X ₁	Severity of Arrest Charge	Violation (1) Unclassified misdemeanor (2) B misdemeanor (3) A misdemeanor (4) E felony (5) D felony (6) C felony (7) B felony (8) A felony (9) 21% 47% 19% 10% 3%
X ₂	Severity of Arraignment Charge ^a	Violation (1) Unclassified misdemeanor (2) B misdemeanor (3) A misdemeanor (4) E felony (5) D felony (6) C felony (7) B felony (8) A felony (9)	2% 3% 10% 20% 40% 16% 9% 2%
X ₃	Arrest Charge Burglary ^b	Miscellaneous (-1) Not burglary nor misc.(0) Burglary (1)	19.5% 57.5% 23%
X ₄	Arrest Charge Robbery	Misc. (-1) Not robbery nor misc. (0) Robbery (1)	19.5% 66% 15%
X ₅	Arrest Charge Drugs	Misc. (-1) Not drugs nor misc.(0) Drugs (1)	19.5% 73.5% 7.0%
X ₆	Arrest Charge Larceny or Theft	Misc. (-1) Not larceny nor misc. (0) Larceny (1)	19.5% 61.0% 19.5%
X ₇	Arrest Charge Assault	Mis. (-1) Not assault nor misc. (0) Assault (1)	19.5% 64.5% 16.0%
X ₈	Total Number of Arrest Charges	Interval Scale 1-6	$\bar{x}=2.01$
X ₉	Total Number of Arraignment Charges	Interval Scale 1-4	$\bar{x}=1.10$
X ₁₀	Arrest or Arraignment Charge Including Possession of Weapon Charge	No (-1) Yes (1)	91% 9%

Table 1. (continued)

Notation	Variable	Scale	Frequencies (1,213)
X ₁₁	Arrest or Arraignment Charge Including Resisting Arrest Charge	No (-1)	94%
		Yes (1)	6%
X ₁₂	Race/Ethnicity ^c	Black or Spanish (-1)	88.5%
		White (1)	11.5%
X ₁₃	Age	Interval Scale 14-75	$\bar{x}=26.74$
X ₁₄	Time Employed ^d	Unemployed	...
		6+ months (1)	29%
		Unempl. less than 6 mos. (2)	29%
		Empl. less than 6 mos. (3)	13%
		Employed 6+ mos. (4)	29%
X ₁₅	Weighted Index of Prior Convictions ^e	Interval Scale 1-36	$\bar{x}=2.37$
X ₁₆	Elapsed Time since Most Recent Arrest ^f	0-3 days (1)	0.5%
		4-180 days (2)	15.5%
		181-365 days (3)	6.0%
		366-729 days (4)	7.0%
		2-5 years (5)	9.0%
		5+ years (6)	26.0%
X ₁₇	Pretrial Release Status ^g	Detained more than or equal to 30 days (1)	16.5%
		Detained less than 30 days (2)	25.0%
		Released on bail (3)	2.0%
		Released on personal recog. (4)	53.5%
X ₁₈	Felony Charge Reduced to Misdemeanor at First Presentation ^h	Reduced at adj. after arraignment (-1)	59.0%
		Reduced at prel. hearing (0)	34.0%
		Yes (1)	7.0%
X ₁₉	Felony Charge Reduced to Misdemeanor at Preliminary Hearing ⁱ	Reduced at adjournment after arraignment (-1)	59%
		Reduced at 1st pres. (0)	7%
		Yes (1)	34%
X ₂₀	First Arrest	No (-1)	64%
		Yes (1)	36%

^a Severity of the arraignment charge was examined separately from the arrest charge because the arraignment charge is the charge for which the defendant was prosecuted, and it may differ from the arrest charge. Severity is coded here and in X₁ from least to most severe, and the severity code corresponds to the most severe charge if there was more than one charge.

^b The type charge for X₅-X₇ was coded in accordance with the most severe arrest charge.

^c Race/ethnicity was also examined as Black/White/Spanish. Since the difference in effects was White/Black or Spanish, the white-nonwhite code is presented.

^d Time employed is used instead of income because there is very little variation on income in this sample and because employment stability rather than income was observed to be a question often raised in court.

^e Prior felony convictions were given 3 points, prior misdemeanor convictions 2 points and prior violations 1 point. The index is the sum of these scores. The data were analysed with prior convictions differentiated as well.

^f The arbitrary coding here was exactly as the data were collected by the pre-trial services agency.

^g The defendants' status while awaiting final disposition was broken into these four categories because prior research has been criticized for failing to differentiate between those detained for longer versus shorter periods of time.

^h The first presentation is the first arraignment hearing.

ⁱ The preliminary hearing is the last point at which felony charges can be reduced in criminal court. If the felony charge remains, the case is waived to Supreme Court.

Table 2. Zero-Order Correlation and Regression Coefficients for Exogenous Variables with Y_1 , Y^2 , and Y_3 ($p < .10$)

Variables	Y_1 (Dismissal)			Y_2 (ACD)			Y_3 (Sentence Severity)		
	ρ	b	β	ρ	b	β	ρ	b	β
X_6	.05	.035 (.018)	.047						
X_7				.13	.141 (.072)	.155	.07	.801 (.296)	.195
X_8	.01	.036 (.021)	.044						
X_9	-.08	-.022 (.010)	-.042	-.08	-.085 (.046)	.065	-.07	-.425 (.257)	-.107
X_{10}				-.06	-.089 (.030)	.107			
X_{11}							.05	.302 (.162)	.082
X_{12}							.02	.168 (.085)	.087
X_{13}				-.14	-.006 (.004)	-.069	.15	.068 (.021)	.162
X_{14}				.22	.078 (.013)	.214	.02	-.161 (.072)	-.165
X_{15}	-.05	-.034 (.007)	-.092				-.21	-.274 (.085)	-.151
X_{16}	.55	-.083 (.024)	-.106						
X_{17}	.77	.447 (.016)	.853						
R^2 with judges/ without judges				.20	.082 (.018)	.170	-.10	-.374 (.205)	-.142
Number of cases		.625/.610			.232/.188			.154/.114	
R^2 increment for judges		1213			733			510	
		.015 ($p < .05$)			.044 ($p < .05$)			.040 ($p > .05$)	

Standard errors in parentheses.

successfully. Many burglaries are committed at times and in places where eyewitnesses are not present. The absence of witnesses naturally reduces the strength of the evidence. Our finding that defendants who have less rather than more arrest charges are more likely to be dismissed also may be related to evidence, since those with more charges are often those against whom a stronger case can be made. Finally, our most important finding, i.e., that defendants whose felony charges are not reduced until the final opportunity are more likely to be dismissed, also may be construed as reflective of evidentiary problems. These defendants have continued in the court process longer than their counterparts; as such, the standard for continuance of the case has increased. Specifically, Zeisel et al. (1975:134) note: "The standard for sufficient evidence to continue a case becomes more stringent as the criminal process proceeds. To sustain initial prosecution, 'probable cause' is sufficient evidence; eventually [however] proof of guilt 'beyond a reasonable doubt' is needed."

While the findings discussed above are interpreted as reflecting problems of evidence, for sociology the interesting findings are those that don't fit neatly into that interpretation. The question is, to what degree do they fit the interactionist perspective?

Starting with the easiest variable, our finding that individual judges do significantly affect the dismissal decision (see note, Table 2) is consistent with prior research (Hogarth, 1971) and with the interactionist thesis (Becker, 1973) that those playing the role of societal reactor significantly affect the nature of the reaction. In the absence of additional data beyond the judges' identification, we can't explain what it is about individual judges that correlates with the dismissal decision. Future research should make this a priority concern.

Our finding that defendants charged with assault are more likely to be dismissed may reflect the lesser value placed on interpersonal violence when it occurs among minority groups. While we lack individual data on victims, our court observations revealed that almost all of the

assault cases prosecuted were assaults between persons of the lower classes who predominate in the catchment area served by this court. Like Garfinkel (1949) and Bensing and Schroeder (1962), we suggest that interpersonal violence evokes a lesser response when both the defendant and the victim are socially disadvantaged because there is less concern for disadvantaged victims. This finding is consistent with the interactionist thesis that the "value" of the offense, as perceived by the reactors, affects the determination of the societal response (Schur, 1971).

Finally, our finding that the defendant's release status prior to disposition affects the likelihood of dismissal is of interest, since we find being *pre-trial detained increases the likelihood of being dismissed*. Thirty-nine percent of those detained in jail while awaiting their final disposition are ultimately dismissed. Admittedly, this seeming inconsistency is possible while still operating within legal statutes. However, it is ideologically problematic to note that so many persons are detained while awaiting dispositions for charges for which they will ultimately not be convicted. Our observations suggest that some court agents are using court processes as sanctions. That is, they assume that defendants who have been detained already have been sanctioned. To save the court further expenditures of time and money, the detention experience is treated as having provided the necessary "taste of jail" to deter future crime. While our observations of the treatment of the detained affirm the appropriateness of the assumption that they have been sanctioned, the question is whether some of these dismissals are obscuring a kind of discrimination against the economically disadvantaged. To elaborate: if the defendant was detained because he couldn't post bail and his subsequent dismissal reflected a presumption of his innocence, his inability to post bail would have caused him to be severely sanctioned. The subsequent dismissal of his case obscures the fact that he has been punished unnecessarily. Ultimately, to determine whether this kind of discrimination is widespread, one needs to know the basis upon which the pre-trial release decision

was made. Again, future research should probe this question in greater depth.

To summarize for this first dependent variable, i.e., the dismissal decision, we find the assertions of the interactionist perspective to be modestly supported. While we do find factors associated with the alleged offense don't account for all of the explained variance, almost all of the explained variance can be interpreted to be a function of the strength of the evidence. Unless the strength of the evidence can be shown to be related systematically to extra-legal considerations, the argument that dismissal decisions are based on misappropriated discretion has to be seriously questioned. Furthermore, the variables examined here to indicate the defendant's social attributes (e.g., race, education) are found to have no significant effects. The above notwithstanding, we do find that the persons doing the reacting (e.g., judges), the values attached to specific types of crimes (e.g., assaults), the organizational imperatives of the court (e.g., dismissal of persons detained before trial) and statuses resulting from prior decision processes do significantly affect the societal reaction, controlling for the alleged deviant act. These effects, however, are all relatively small.

Table 2 also presents the regression coefficients for those exogenous variables that had net effects on the second endogenous variable (Y_2). The decision to adjourn a defendant in contemplation of a dismissal is increased if: (1) the defendant's most serious arrest charge is a drug charge; (2) the defendant's total number of arrest charges was lesser rather than greater; (3) the defendant was *not* charged with "resisting arrest"; (4) the defendant has less rather than more prior convictions; (5) the defendant had never been arrested prior to this arrest; (6) the defendant was released from custody pending his final disposition.

Our finding that defendants who have "cleaner" prior criminal records are more likely to be favored with this ACD disposition suggests differentiation on the basis of accumulated disadvantaged status. That is, those with heavy prior records, having previously been adjudicated guilty, have already accrued a disadvantaged

label. Whether differentiation on the basis of this prior disadvantaged status is discriminatory depends on whether the status of "prior convicted offender" was ascribed or achieved. To the extent that one's conviction for a prior crime was not entirely a function of the alleged offense, the negative effect of a prior record can be interpreted as partly discriminatory. Until such time as we can partial out achieved disadvantaged status from ascribed, the issue of infinite regress remains problematic.⁵

Our finding that defendants charged with resisting arrest are less likely to be favored with this disposition is consonant with the interactionist thesis that the organizational imperative to maintain good relationships among criminal justice personnel affects societal reactions. Blumberg (1967), Chambliss and Seidman (1971) and Rosett and Cressey (1976) underscore the importance of the police to the criminal justice system and the need to sanction those who counter police authority.

Our finding that defendants who are released while awaiting their final disposition are more likely to be given ACD dispositions is consistent with the Wald and Freed (1966) and Roballo (1974) thesis that defendants carrying the label of "pre-trial detainee" are processed with an additional negative status. The fact that a defendant was not released pending disposition signifies that the defendant was deemed a poor flight risk, a danger to society and/or economically disadvantaged. If a prior set of societal reactors responded negatively to the defendant, it might be organizationally functional to maintain consistency in decision making,

⁵ The problem of infinite regress is important because the law provides for consideration of decisions resulting from prior processing, e.g., ex-convict status. However, since we know that whether someone carries forward "ex-convict" status is not entirely a function of prior deviant behavior (Bernstein et al., 1977), there begins to be a meshing of legal and extra-legal considerations. Moreover, since discretion operates at every stage of the process, one can always argue that some prior process problem of infinite regress should be kept in mind to stimulate new lines of inquiry. It should not, however, be used to discount findings that fail to account for the entirety of processing.

thus the denial of the favorable ACD disposition. Alternatively, social typing (Schur, 1971) might be occurring wherein the defendant's release status prior to disposition is treated as a category defining a set of appropriate responses. Since being detained prior to disposition is the least favorable category, a negative response to those so categorized becomes understandable.

Finally, as before, we find those acting out the part of the societal reactors, i.e., the judges, have a significant net effect on the ACD decision.

To summarize, whereas the dismissal decision was largely determined by consideration of factors related to evidence, our analysis of the decision to favor a defendant with a "second chance" finds somewhat stronger evidence in support of interactionist assertions. Variables associated with the alleged offense neither account for all of the explained variance nor have the largest effects. Rather, it seems that organizational imperatives (e.g., deference to the police), the individuals who are reacting, and negative status labels carried forth from prior decision processes (e.g., prior criminal record) play the major role in determining whether a defendant will be adjourned in contemplation of dismissal. As before, we must reiterate the very notable lack of significant effects for the social attributes of the defendant here examined.

Finally, we present the regression coefficients for those exogenous variables that had net effects on the third endogenous variable (Y_3). According to Table 2, the likelihood that a convicted defendant will receive a *more severe sentence* is increased if: (1) the defendant is charged with robbery; (2) the defendant has a heavier record of prior convictions; (3) the defendant has been employed for a *longer* rather than shorter period of time and (4) the defendant is *white*. The likelihood of receiving a *less severe sentence* is increased if: (1) the defendant is charged with assault; (2) the defendant has no prior arrest record; (3) the defendant has maintained a "clean record" for a longer period of time and (4) the defendant was released from custody pending his final disposition.

Since the direction of these findings is

identical to those earlier noted and we have already provided interpretative comments, we limit our discussion to those findings upon which we have not previously commented. We interpret the finding that defendants whose most serious arrest charge is robbery are more severely sentenced to be a function of the high value placed on robbery offenses in this geographic area, at this point in time. Public concern for increasing robberies, especially violent robberies against the elderly and the handicapped, was extremely high when these data were collected. Thus, the value attached to the crime may explain the severe response to those so accused (Turk, 1969; Schur, 1971).

The finding that white defendants, as well as defendants who have been employed for longer periods of time, are more severely sentenced is unexpected. While these effects are smaller than those of other exogenous variables, they are statistically significant. Clearly, they may be due to chance, given the large number of variables considered. However, on the assumption that the findings are reliable, we advance the following as a possible explanation. According to some of our interviewees, some judges and prosecutors assume that nonwhites commit crimes because the nonwhite subculture accepts such behavior. These subcultural differences are considered by the judges and prosecutors, thereby making the offenses of nonwhites seem less pernicious. However, no comparable "account" is available for white defendants. Expectations for them are higher and, as such, their failure to meet such expectations may appear more noxious—thus, the more severe sanction. The same explanation was put forth for defendants holding steady jobs. While this line of interpretation is speculative, future researchers should make a priority of the collection of data that would allow us to estimate the degree to which the expectations and tolerances held for certain groups of deviants and deviance affect societal reactions.

To summarize, the severity of the sanction meted out to convicted defendants is not a direct result of the alleged offense. While none of the effects are large, the determination of the harshness of the re-

sponse is affected by consideration of the public concern for particular offenses, the expectations held for various groups of deviants, and the status labels that defendants carry forth from prior stages of deviance processing.

Conclusion

We began with the presumption of an imperfect correlation between deviant acts and the societal reaction to those acts. Taking the broad view that there are a variety of factors that account for that independence, we analyzed three sequential societal reaction decisions. Our data indicate, first, that the variety of contingency factors emphasized by interactionists as explaining the independence between deviant acts and the reaction to same explain more variance in later rather than earlier deviance-processing decisions. That is, extra-legal factors explain more variance in sentence severity decisions than in prosecution and adjudication decisions. However, since we have not here analyzed the entirety of decisions, i.e., decisions that precede the dismissal decision (e.g., arrest) nor those following the determination of sentence decision (e.g., parole), we limit our conclusion to an assertion that the amount of variance explained by characteristics of the deviant act varies with the decision being made and the point in time at which the decision occurs. This suggests that comparable data sets need to be analyzed for all the deviance-processing decisions in sequence. When this task is accomplished, we should be able to determine whether the independence between deviant acts and societal reactions increases or decreases as one moves through deviance-processing stages.

Second, the emphasis that interactionists place on the role of the deviants' social attributes in explaining variation in societal reactions seems very much overstated. Our finding that age, education, employment stability, marital status, and race have no effects on the first two societal reactions decisions, and only small effects on the third societal reaction decision, suggests that the theoretical focus requires considerable shifting. Spe-

cifically, we interpret our findings to suggest that greater attention to be paid to (1) organizational imperatives of the deviance-controlling agency, (2) the expectations and values of those participating in the decisions and (3) the role of accumulated disadvantaged statuses acquired in prior deviance-processing stages. While our results affirm the assertion that these factors significantly affect societal reactions, they do not provide the depth needed to construct a theory explicating the conditions under which these factors are more or less salient and when salience represents systemic discrimination. Future research should make this a priority.

Finally, in terms of sociological theory, while we find the thrust of the interactionist perspective and its core assertions are consonant with real world occurrences, the perspective is so broadly stated that it precludes the refinement necessary for the assertions to be linked into some useful theory. Accordingly, we suggest more empirical exploration be undertaken, with an eye toward the emergence of a grounded theory of deviance. To the extent that the results of empirical research can delimit the various stages of deviance processing, and the precise degree of, and bases for discretion at each stage, we can begin to articulate an empirically based theory.

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PATTERNS OF URBAN DOMINANCE: THE U.S. IN 1890

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The major thesis of this paper is that macroanalyses of urban social organization have unduly exaggerated the importance of trade and commerce as the foundations of a division of labor among cities. This division of labor and the capacity of cities to influence each other differentially are examined as resting upon a convergence among fiscal-commercial activities, plus intellectual, cultural and organizational activities, and transportation and communication. This multifaceted conception of urban dominance stresses how different kinds of activities are associated with different geographical spheres of influence, resulting in three distinct types or dimensions of dominance: national-international, regional and local. Examination of U.S. cities in 1890 indicates two different functional and geographical distributions of regional and national dominance. They are associated with different patterns of regional integration and with variations in the economic development of regions. The types of dominance are also seen to exhibit substantial historical continuity between 1890 and 1970, with the multifaceted measure providing much better predictions than the financial measure.

An intimate historical association among the development of technology, the increased complexity of social institutions, and the growth of cities—in both size and functions—has been described repeatedly (Mumford, 1938; Meadows, 1958:141-7; Weber, 1958). Thus, terms like “urban society” and “urban systems” provide shorthand ways of referring not only to the major changes that have occurred in cities, but in technology and in institutions as well. One result of these converging forces is an urban social organization that has been the object of a half century of continuing study, though from markedly different perspectives. At a microsocial level, following Wirth (1938:1-23), Thomas (1923) and others, the focus has been upon interpersonal relationships and situational meanings, with the urban society viewed as a generator of distinctive cultural values and social norms. At a macrosocial level, the pervasive perspective has been ecological and structural, stressing the interdependence, specialization and division of labor within an urban system (cf. Duncan et al., 1960; Gibbs and Martin, 1962: 667-77; Duncan and Lieberman, 1970).

While it has been claimed that distinctions between the two perspectives have been exaggerated (cf. Schnore, 1961:128-49), it is nevertheless true that

very few studies have attempted to view the interactional and cultural aspects of urban societies in structural terms or, conversely, to relate the structural features of urban systems to values and norms. (Some exceptions to these trends include Pred, 1966; Abrahamson, 1974:376-383; Fischer, 1975:520-32.) The most ambitious objective of this paper is to suggest the fruitfulness of integrating aspects of the two historically separated perspectives.

At a macrosocial level, efforts to analyze the distinctive features of urban social organization frequently have focused upon the concept of urban, or metropolitan, dominance. As utilized, dominance has had two interrelated referents: (1) a view of cities as the loci of major activities and (2) a view of large-scale urbanism as involving a division of labor among cities, with a city's location in the division of labor implying differential capacities to influence activities or set the conditions of operation in other cities (Hawley, 1971). Since Plato and Aristotle, theorists have speculated about such patterns of dominance (Wirth, 1937:493-509); but, with respect to contemporary urban systems, there are several pioneering studies that have come to be regarded as landmarks.

The first of these was probably Gras'

(1922) early study. He maintained that technology, economic systems and social organization tended to converge historically into five major constellations, or phases, and that these phases described the history of Western civilization. The final phase, termed the "metropolitan economy," involved large cities which were the loci of extensive trade and commerce. In comparing cities, Gras emphasized the dominance dimension which he viewed as primarily fiscal and economic in nature. "What counts most," he concluded, "is commercial dominance over a wide area" (Gras, 1922:184). The boundaries of this subordinated "hinterland" were not well delineated, though he did distinguish between types of trade according to whether they tied a city into a local (metropolitan) or an extended (national) economy.

McKenzie's important contribution placed dominance into an explicit ecological framework then being developed at the University of Chicago (cf. Faris, 1967). Based upon transportation developments, notably the automobile, McKenzie (1933:7) saw the contemporary city as "able to extend the radius of its influence . . . throughout surrounding territory." Like Gras, he stressed an economic interpretation of dominance, an emphasis which was highly compatible with the analogy drawn between plant and animal communities, on the one hand, and the metropolitan or human community, on the other. Thus, people striving to "maximize their livelihoods" in a metropolitan community became equivalent to a collective response or adjustment to habitat in a biotic community. Also, McKenzie saw the basic ecological features of the metropolitan community as the result of people trying to maximize their livelihoods while minimizing their efforts.

Bogue (1949) added clarity and precision to the study of urban dominance, while retaining an explicit ecological perspective. His detailed and extensive classification of American cities also emphasized the economic realm, utilizing wholesale and retail trade, services and manufacturing as the indicators of dominance. Aside from their ready availability,

he justified their use because they were indicators of human sustenance. In examining his detailed sectors and distance zones, Bogue also confronted the issue previously encountered by Gras and McKenzie, namely, the uncertain boundaries of a metropolitan area's dominance. He noted that some areas' influence transcended across adjacent metropolitan areas. Hence, some type of division of labor among cities in the national economy must occur, he concluded, and it must be analogous to the division of labor within metropolitan areas. Aside from offering this observation, however, Bogue did little to pursue its implications.

The ecological perspective on urban dominance, developed primarily during the second quarter of this century, continued to influence very strongly the direction of inquiry in the third quarter. In this later period, most of the research attempted either to document aspects of the Gras-McKenzie-Bogue paradigm (cf. Lieberman, 1961:491-6) or to clarify questions related to the measurement of dominance raised by their paradigm (cf. Carroll, 1963:166-73). The paradigm has been demonstrably fruitful, but it has remained conceptually narrow, with an overwhelming emphasis upon a commercial-economic conception of dominance, a conception which draws on a questionable "economic man" model (cf. Sjoberg, 1965:157-89). As a consequence of this emphasis, dominance theory has been viewed as applicable to an unduly limited range of problems. The strict commercial-economic interpretation of dominance also has helped to perpetuate the estrangement between ecological and other approaches to urbanism by implying that the ecologically salient concept of urban dominance was not relevant to analyses of cultural values, noneconomic institutions, or the like.

Multifaceted Dominance

We contend that financial, commercial or economic considerations have been overly emphasized. Their theoretical relevance is apparent and their utilization has been fruitful in clarifying a number of urban patterns. However, there are also

several decades of research and theory that suggest cities also to be important centers for intellectual and artistic creativity (Merton, 1964), the headquarters of nonfinancial as well as financial associations (Lieberson and Allen, 1963:319-25), centers for the innovation of new lifestyles (Becker and Horowitz, 1971:12-5), and so on.

The architects of the pervasive ecological model were themselves aware of noneconomic dimensions of urban dominance. Bogue (1949:23), for example, in the same paragraph that stressed sustenance indicators, explicitly noted the possible existence of dominance patterns that were less related to sustenance. Even more explicit was Burgess' (1925) view of the dominance of the central business district ("the nucleus") which he described as not only commercial, but intellectual, political and cultural. Over time, however, the commercial-financial emphasis of the ecologists led to the implication that the noneconomic aspects of dominance were little more than redundant by-products of the economic ordering of cities (cf. Duncan et al., 1960).

It is our hypothesis that while there is convergence among financial, intellectual, cultural and other aspects of dominance, their combination produces a ranking of cities that is different from that produced by one of them (such as the economic) alone. We further expect that this combined index of dominance will provide the best predictor of future city activities, even in a financial-commercial sphere. More fundamentally, we are beginning with the (traditional-ecological) assumption that there is a division of labor among cities and that a city's location in this division of labor determines its degree of dominance. However, we view this division of labor itself, and the consequent dominance of cities, as resting on a variety of activities rather than upon financial-commercial activities alone.

Types of Dominance

Related to the nature of dominance is the question of whether variations in the geographical scope of a city's sphere of influence should be considered a single

discrete variable. If so, then variations in geographical spheres can be viewed as the result of *quantitative* differences in the dominance-producing activities that are located in cities. If dominance over different sized geographical areas is associated with different kinds of activities, however, types of dominance should then be viewed as *qualitatively* different from each other. There are precedents for either view, though the quantitative assumption appears more congruent with a view of dominance as a limited, commercial-financial phenomenon.

Gras (1922), for example, saw local, as opposed to extended dominance as resting upon different types of trade (e.g., retail versus wholesale), and this distinction has resulted in two somewhat different rankings of American cities (Carroll, 1963:166-73). However, while McKenzie, Bogue, and later students of urban dominance sometimes made similar distinctions, the general tendency has been to view different geographical spheres as extensions along a unitary dimension. More of the same kinds of activities, it is therefore implied, would increase the geographical scope of a city's sphere of influence; and the kinds of activities singled out are commercial-financial. The compatibility of these two assumptions is illustrated by the Duncan et al. (1960:270) classification of San Francisco as a regional metropolis that is "nearly strong enough" in financial functions to qualify as a national metropolis.

Associated with the view of dominance as multifaceted, however, it is our hypothesis that different mixes of activities are associated with varying geographical spheres of influence. For example, the circulation of a city's daily newspaper in most instances, is, tied to a local metropolitan area. Differences in circulation, standardized for city size, might therefore reflect the local, but not extended dominance of most cities. On the other hand, scientific journals that are published in a city typically have a national and international audience. Variations among cities in this form of publication activity therefore might be indicative of extended dominance, but not local dominance.

It is our more specific contention that it

is fruitful to differentiate among three types of dominance: national-international, regional and local. The first type is named national-international in recognition of the fact that important relationships among nations tend to occur between their major national cities. Thus, major national cities are also simultaneously "world cities" (Hall, 1966). The second type of dominance, regional, involves influence over large geographical areas, but not entire nations. Historically, New England and the South are probably the most apparent examples of regions in the U.S. (cf. Vance and Sutker, 1954). While their boundaries may be fluid, the distinctiveness of regions is based upon specialized divisions of labor, common cultural orientations, and the regional or district organization of government and national organizations. We contend that while regional dominance might be viewed quantitatively as a lesser degree of national dominance, the different types of activities involved in each make them better viewed as two separate (qualitative) dimensions. For example, an increase in Atlanta's role as a Southern railroad center in 1900, or as a distribution center for the South in 1975, would not necessarily increase that city's national dominance. Finally, local dominance refers to the influence exerted over an immediately adjacent hinterland and corresponds roughly with SMSAs or urbanized areas. Conceptually, there is the possibility of viewing a quantitative ordering of local and regional dominance, but we would emphasize qualitative differences. For example, Los Angeles is obviously important today as a regional center in the West; but the metropolitan area has been described as "twenty-four suburbs in search of a city" (Smith, 1968: 252).

Methods and Procedures

The bulk of the data reported in this study are taken from a sample of 70 U.S. cities in 1890. To assess a theoretical perspective such as that presented in this paper, virtually any time period could be utilized. This paper is part of a larger project examining urbanization in the U.S. and this historical examination requires

data from an early phase of large-scale urbanization in the U.S. Prior to 1890, it was not possible to locate much of the required data relating to intercity postal and railroad service, church hierarchies, banking, etc. Even though a Census Bureau fire in 1921 destroyed portions of the 1890 census, it was the earliest time for which many potential indicators of dominance were available. When data are presented in this paper for later time periods, the same 70 cities provide the sample.

We began with a list of 81 cities, selected to be representative of both different regions in the country and varying city-size categories. A few of the selected cities eventually had to be excluded because they could not be placed into any region. In every case, these were demographically smaller cities, such as Peoria, Illinois. Extensive missing data later forced the removal of a few additional cities, and they too tended to be smaller in population size. Thus, the final sample of 70 cities slightly underrepresents smaller cities, though they are not absent from the sample. (A complete listing of sampled cities is included in Table 2.) In 1890, the sample is almost identical to the universe of (then) demographically major cities. It includes about three-fourths of that universe in 1970.

Initially, we attempted to construct regions utilizing criteria that would be relatively invariant over time: geographical land-use and Federal Reserve Districts. The immediate problem was that these two criteria did not produce congruent regions and we had no rationale for favoring either when they disagreed. A new criterion, transportation-communication patterns, was subsequently utilized as the final arbiter of regional boundaries. This specifically involved the number of railroad mail routes run weekly between all towns and cities in the U.S. (U.S. Postmaster General, 1890). Because passengers were also carried on these trains, the railroad mail routes simultaneously indicated patterns of transportation and communication.

Utilizing this criterion, any city with less than 50 percent of its total routes connecting with other cities in the proposed

region was excluded from that region. It was placed into another, when possible. Some cities, however, could not be placed into any region in 1890. Peoria, Illinois, for example, connected partially with the Midwest, partially with the Ohio Valley and, to a lesser extent, with the South. But 50 percent or more of the city's routes were not in any one region. By contrast, larger cities—such as Buffalo and Louisville—that provided points of connection between regions could be placed without exception into a single region.

It must be recognized that the exact boundaries of any geographical unit are difficult to specify precisely (cf. Gibbs, 1961). Further, the exact boundaries of regions in the U.S. have been shown to vary according to the criteria that are utilized (Green, 1955: 283–300). In the final analysis, therefore, they are arbitrary. There are two justifications for the regional boundaries utilized in this study. The first is procedural, namely, their construction in accordance with transportation and communication routes. Especially from an ecological perspective (Hawley, 1950; 1971), they provide a more relevant criterion than others that have been used often (e.g., geographic land use). Second, the regions as developed here correspond well with the regional boundaries typically described in historical research (cf. Sutter, 1973).

For the 70 cities in the nine regions, a large number of potential indicators of the three types of dominance were selected from varied sources. The items include conventional financial-commercial indicators (e.g., the dollar value of clearing-house exchanges), plus items that potentially relate to a city's intellectual role (e.g., scientific journals published), its role in relating the U.S. to other nations (e.g., import duties collected), the kinds of organizations located in the city (e.g., headquarters of national trade unions), etc. The final list of potential indicators was limited to about 20, however, because when the ratio of cases (i.e., cities) to observations (i.e., variables) exceeded about three and one-half to one, the factor solution was confounded by high collinearity in the correlation matrix (cf. Harmon, 1967). The 20 variables finally

included were selected for their theoretical potential as indicators of one of the three types of dominance, and an approximately equal number of indicators were selected for each type of dominance.

The variables were subjected both to orthogonal and oblique factor solutions which produced only very minor fluctuations in defined factors (cf. Abrahamson and Johnson, 1974: 521–32). In most cases, the orthogonal factors were retained for subsequent analysis. We hypothesized that the variables would sort into three factors associated with national-international, regional and local dominance. City scores on these three factors then would be utilized to test a number of more specific hypotheses.

At this point, however, two of the "methodological foibles" inherent to factor analysis must be noted. First, we recognize that our substantive labeling of these factors is a subjective process. Others might wish to argue with the labels we will apply, though we believe the offered interpretations to be theoretically well justified. Second, because factor analysis is essentially a data reduction technique, the factors that are obtained are totally dependent upon the mix of variables that are initially included. Therefore, despite our efforts to balance variables by type, we consider it inappropriate to interpret eigenvalues, or any other property of the factors, as indicating the strength or importance "in reality" of any factor (cf. Rees, 1971: 220–33). Thus, we are not proposing that factor analytic results alone show the three types of dominance to exist as separate types. Rather, we view the factor analysis primarily as a means for developing multiple item measures of each type of dominance to be used in subsequent analyses.

Results

The factor solutions resulted in a number of factors, three of which could be substantively associated with the three types of dominance previously discussed, i.e., national-international, regional and local. This portion of the results is presented in Table 1. Reading diagonally across the table, the boxed-in factor load-

ings indicate the items (from the left margin) that identify the substantive factors (presented across the top of the table).

National Dominance

Inspection of the items that load highly on the national dominance factor reflect both the national and international aspects of this dimension. However, subsequent examination of cities' factor scores on this orthogonal factor disclosed that a number

of small and otherwise "unimportant" cities with ports linking to Canada had very high scores—Port Huron, Michigan, for example. In the oblique solutions though, there was a more pronounced distinction between the national and international aspects of the dimension (see the last two columns in Table 1). In the oblique national factor, the two explicitly international items (international passengers and import duties) have the weakest load-

Table 1. Dominance Factors

Variables	National Dominance	Regional Dominance	Local Dominance	National Oblique	International Oblique
International passengers ^a	.97	.10	.02	.74	.61
Total import duties ^a	.91	.14	.01	.66	.63
National unions, home offices ^c	.91	.08	.11	.86	.45
Birthplace, famous entertainers ^d	.89	.05	-.01	.82	.48
Volume (\$) clearinghouse exchange ^e	.89	.15	.11	.84	.41
Scientific journals published ^f	.85	.08	-.05	.78	.45
Patents issued ^g	.81	.01	.08	.83	.38
Longest regional RR-mail ^h	.05	.81	.13		
Ratio-frequent/longest RR-mail ⁱ	.17	.67	-.14		
Catholic bishops ^j	.23	.50	-.01		
U.S. District Courts ^k	.11	.42	.34		
Public vehicles ^l	-.06	-.11	.87		
Special delivery letters ^m	.17	.31	.73		
Newspaper circulation ⁿ	.29	-.03	.41		
Suburban passengers ^o	.15	.09	.20		
Eigenvalue=	8.88	1.53	1.43		

^a No. of passengers leaving or entering city from foreign ports. (Bureau of Statistics, Treasury Department [Imports, Exports, Immigration and Navigation U.S. Gov't. Printing Office], 1890)

^b Total value of duties collected from foreign imports to city. (U.S. House of Representatives Documents [Statistical Abstract], 1899)

^c National trade unions with headquarters in the city. (World Almanac, 1892: 92)

^d Theater and musical entertainers in 1890 born in city. (American Almanac, 1892: 212)

^e Intercity clearinghouse transactions emanating from city (Annual Report of the Comptroller of the Currency, 1890: 74)

^f Scholarly and scientific journals published in city. (Butler [Education in the U.S.] Albany, N.Y.: Lyons 1900)

^g Patents issued to inventors residing in city. (U.S. Senate Documents [Patent Report], 1896)

^h Track mileage of the longest railroad mail trip between city and other cities in its region. (Postmaster General of the U.S. [Messages and Documents], 1890)

ⁱ Ratio of rail track mileage of most frequent intercity trip to longest intercity trip emanating from a city. (Postmaster General of the U.S. [Messages and Documents], 1890)

^j Catholic Church hierarchy located in city. Weights: 3=Presence of a Cardinal; 2=Archbishop; 1=Bishop.

^k U.S. District Court Judge residing and officiating within a city. (American Almanac, 1886: 68)

^l Public vehicles (taxicabs, pedicabs, etc.) for hire, licensed and operating in city. (U.S. Bureau of Census [Social Statistics of Cities], 1890: 71)

^m Special delivery letters mailed in city in 1890. (U.S. Postmaster General [Messages and Documents], 1890)

ⁿ Circulation of all daily newspapers published in city (Rowell [American Newspaper Directory], 1890)

^o Annual passengers from within 20 miles of city's geopolitical boundaries. U.S. Bureau of Census [Social Statistics of Cities], 1890: 133, Table 74)

Note: Where appropriate, items converted to per capita figures. All variables standardized prior to factor analysis.

ings among the defining items. By contrast, they have the strongest loadings on the oblique international factor.

Because this study focuses solely upon the U.S., we decided to de-emphasize the international aspect and utilize the oblique national factor as the measure of national dominance. (This oblique factor correlates only weakly with the orthogonal measures of regional and local dominance. The correlations are .29 and .27, respectively.)

Table 2 presents the standardized factor scores of all 70 cities on national dominance (oblique factor) in 1890. As would be expected, the highest scoring cities—in fact, the three highest—are located on the East Coast. The West Coast, by contrast, contains three of the five lowest scoring cities. More than location though, national dominance would be expected to correlate

with population size (Bogue, 1949; Duncan et al., 1960; Carroll, 1963: 166–173).

In order to assess the correlates of all types of dominance more completely, we selected a large number of variables relating to the characteristics of cities and their populations and, again using factor analysis, constructed potential (composite variable) correlates. The items that define each correlate (i.e., factor) are presented under the summary of results in each appropriate table. A multiple correlation analysis, with respect to national dominance, showed size-density to be the strongest independent correlate, followed closely by ethnic social organization and then by labor force modernization. These three variables alone are able to explain almost three-quarters of the variation in national dominance.

Table 2. National Dominance in 1890

City	National Dominance	City	National Dominance
New York	5.02	Charleston, S. C.	-.36
Boston	3.19	Syracuse	-.37
Philadelphia	2.53	Newark	-.38
Pittsburgh	2.08	Omaha	-.38
Chicago	1.79	Concord	-.39
San Francisco	1.18	Richmond	-.39
Cincinnati	1.16	Cheyenne	-.43
Baltimore	1.13	New London	-.44
St. Paul	.73	Trenton	-.46
New Orleans	.52	Burlington, Vt.	-.48
Cleveland	.48	Reading	-.50
Portland, Me.	.43	Duluth	-.50
Kansas City	.41	Des Moines	-.51
St. Louis	.39	Lexington	-.52
Denver	.33	Norfolk	-.53
Louisville	.31	Burlington, Ia.	-.53
Indianapolis	.30	Ft. Worth	-.55
Providence	.30	Los Angeles	-.56
Detroit	.26	Ogden	-.58
Buffalo	.21	Lincoln	-.58
Milwaukee	.16	Winona	-.58
Albany	.04	Nashville	-.60
Minneapolis	-.03	Columbia	-.65
Springfield	-.04	Pueblo	-.66
Atlanta	-.05	Dallas	-.67
Portland, Or.	-.09	El Paso	-.70
Jacksonville	-.11	Sioux Falls	-.72
Columbus	-.12	Binghamton	-.73
Birmingham	-.20	Natchez	-.74
Toledo	-.22	Oshkosh	-.75
Savannah	-.27	Seattle	-.80
Wilmington	-.30	Charlotte	-.81
Chattanooga	-.32	San Diego	-.85
Mobile	-.34	Port Huron	-.90
Topeka	-.35	Tacoma	-.97

Table 3. Correlates of National Dominance

Major Independent Variables	Beta	
Size-Density	.44	Total Multiple R=.88
Ethnic Social Organization	.40	Multiple R ² =.77
Labor Force Modernization	.24	(R ² of major variables=.71)

Items and Factor Loadings by Correlate					
Size-Density		Ethnic Social Organization		Labor Force Modernization	
Population Density	.84	Foreign Lang. Newspapers	.92	% Professional	.95
Street lamps/Mi.	.84	% Foreign Born	.43	% in Trade & Transp.	.95
Population Size	.83	Pop. Increase, 1880-90	-.25	% of Women	.92

It seems clear that the most nationally dominant cities in 1890 were large, established cities that had already received large numbers of immigrants prior to 1890 and whose labor force composition already was reflecting the growing urbanization and industrialization of the U.S. The highest scoring cities—New York, Boston and Philadelphia—are excellent examples of such large, “modern,” point-of-entry cities.

Regional Dominance

The regional dominance factor, presented in Table 1, is composed of two kinds of items. The first entails communication and transportation involving U.S. mail and train passengers. These items suggest a picture of regions as geographical areas based upon transportation and communication, the same criterion used elsewhere in this study to establish regional boundaries. The second type of item involves regional and district organizations. The Catholic Church hierarchy is the only religious group presented here, but the Episcopalian Church had an almost identical loading on the regional dominance factor. It was eliminated from the final analysis because of the previously discussed need to maximize the cases-to-observations ratio.

Table 4 presents the standardized factor scores of all cities on regional dominance, according to region. Here we made the assumption that the regional dominance factor reflected the degree of influence exerted by a city within the boundaries of the regions as indicated by the railroad mail routes. However, it must be recalled

that regional boundaries, no matter how constructed, are tentative and approximate. In this case, our assumption that regional dominance, as measured by that factor, operates within the confines of the railroad-determined regions must be considered especially tentative and approximate.

Because these regional dominance scores are standardized for the entire sample, it is possible not only to rank cities within regions, but to make comparisons across regions. Thus, it may be inferred, for example, that San Francisco was a more dominant city in the Pacific region than New Orleans was in the Southern region. This means that San Francisco was more of a locus for regional and district organizations, and a more important hub of regional transportation and communication.

In contrast to national dominance, where the correlates were able to explain an unusually high percentage of the total variation, their capacity to explain regional dominance is more moderate (multiple R² = .34). The strongest independent correlate is distance from a city high in national dominance (beta = .54). Distance involved railroad mileage between each city and one of the eight highest scoring cities on national dominance. Thus, again resembling prior findings, it appears that the regional dominance of cities was hindered by proximity to cities high in national dominance (Carroll, 1963:166-73). The other major correlates were the same as for national dominance: labor force modernization (beta = .43) and ethnic social organization (beta = .20). However, size-density—the major correlate of na-

Table 4. Regional Dominance in 1890

City	Regional Dominance	City	Regional Dominance
(Pacific)		(South)	
San Francisco	2.97	New Orleans	1.68
Portland	.92	Mobile	1.06
Ogden	.89	Charleston, S. C.	.71
Los Angeles	-.79	Richmond	.71
Tacoma	-1.23	Chattanooga	.43
San Diego	-1.27	Birmingham	.24
		Atlanta	-.20
(Rocky Mountain)		Savannah	-.27
Denver	1.57	Nashville	-.61
Cheyenne	.90	Columbia	-.76
Pueblo	.52	Charlotte	-.82
		Natchez	-.87
(Southwest)		Norfolk	-.90
El Paso	.18	Jacksonville	-1.33
Dallas	-.04		
Ft. Worth	-.25	(East)	
		Baltimore	1.30
(Midwest)		Buffalo	1.06
Kansas City	1.81	Albany	.29
Omaha	.48	New York	.27
St. Louis	.40	Pittsburgh	.26
Chicago	.24	Philadelphia	-.50
Lincoln	-.05	Trenton	-.50
Burlington, Ia.	-.23	Wilmington	-.67
Topeka	-.24	Syracuse	-.88
Milwaukee	-.25	Springfield	-.95
Des Moines	-.36	Newark	-.99
		Binghamton	-1.15
Northcentral		Reading	-1.46
St. Paul	4.08		
Minneapolis	.69	(New England)	
Winona	.59	Portland, Me.	.39
Sioux Falls	-.49	Burlington, Vt.	-.20
Duluth	-.59	Boston	-.23
Oshkosh	-1.27	Providence	-.27
		New London	-.57
(Ohio Valley)		Concord	-.89
Toledo	.89		
Louisville	.55		
Cincinnati	.29		
Indianapolis	.29		
Detroit	.14		
Cleveland	-.68		
Lexington	-.87		
Columbus	-.93		
Port Huron	-1.14		

tional dominance—was unrelated to regional dominance.

The difference in major correlates further supports the argument that national and regional dominance should be conceptually differentiated. However, the strongest case for their separation results from an examination of the geographical distribution of nationally and regionally dominant cities. It suggests that integration in 1890 followed two very different

functional and geographical patterns. In approximately one-half of the regions, a nationally dominant city also functioned as the primary regional integrator. This pattern characterized the South, North-central, Rocky Mountain and Pacific regions. In each of these regions, the dually dominant city tended to be in a central location geographically, either in the ecological center of the region (e.g., Denver in the Rocky Mountain region) or, as

illustrated by San Francisco, had a central coastline position with respect to other cities in the region (cf. Gibson, 1973). A similar pattern characterized the Southwest which contained a (weak) regional integrator, El Paso, but no nationally dominant city.

In the remaining regions, a regionally dominant city was not nationally dominant and these regionally dominant cities tended to be located at the corners, rather than centers of their regions. This pattern characterized New England, East, Ohio Valley and Midwest. The corner positions of these regionally dominant cities probably provided connectivity for the entire region to other regions or to Canada. By contrast, the center location of the dually dominant cities suggests that they may have provided greater integration and less connectivity for their regions. This supposition is supported by the data which indicate that the two different patterns were associated with markedly different degrees of regional integration. In the four regions where the regionally dominant city was not also nationally dominant, the mean score of the regionally dominant city on regional integration was only .77—less than a standard deviation from the mean of all cities. By contrast, in the four regions where national and regional integration coincided in the same city, the mean regional dominance score of the regionally dominant city was *very* high, specifically, 2.58.

Finally, these two patterns are associated, though not strongly, with different degrees of "maturity," or economic development. Per capita bank assets, for example, were higher in 1890 in those regions where national and regional dominance were exerted by different cities. A comparison of bank asset means shows values of .47 per capita in the differentiated dominance regions compared to .38 in the regions where national and regional dominance were exerted by the same city. This suggests an interpretation analogous to that hypothesized for entire nations by Berry (1961: 573–8), namely, that development is associated with the movement from a primate city to a hierarchical pattern of city dominance. However, Berry reports numerous exceptions

to this pattern at a national level of analysis, and this is also characteristic of our findings at the regional level. Thus, while differences in mean banking assets indicate that the differentiation of regionally and nationally dominant cities is associated with development, the pattern is not without significant exceptions. For example, in the Pacific region where San Francisco played the dual role, per capita banking assets were second highest in the nation.

Local Dominance

Local dominance, like regional dominance, primarily is composed of items relating to transportation and communication (see Table 1). Some of the same items load on both factors, probably reflecting the way in which metropolitan areas blended into extended hinterlands, especially in 1890. However, the clear difference between the two lies in the types of transportation and communication involved—short distance for local dominance and long distance for regional dominance. It also should be recalled that these factors are orthogonal, hence, unrelated to each other.

What stands out strongly from an examination of the distribution of local dominance scores, presented in Table 5, is the relative absence of variability. To illustrate, over two-thirds of all cities had local dominance scores which were less than one-half of a standard deviation above or below mean.

This tendency for most cities to score weakly on local dominance is probably due to the fact that there was no mode of transportation available in 1890 that compared in efficacy to the inter-city connections provided by the railroad. There was daily commuter train service to several of the larger cities, but it was expensive; horse-drawn vehicles and bicycles, the major alternatives, were impractical. Thus, cities generally lacked the transportation mode by which to integrate a metropolitan area.

Given the truncated variability of local dominance, it is not surprising that the correlates were least able to account for variation in it; the multiple $R = .35$ ($R^2 =$

Table 5. Local Dominance in 1890

City	Local Dominance	City	Local Dominance
Pittsburgh	6.42	Cincinnati	-.18
Jacksonville	2.80	Toledo	-.19
San Francisco	1.30	Providence	-.19
Indianapolis	1.22	Nashville	-.21
Denver	1.18	Binghamton	-.24
Lexington	.87	Newark	-.24
Springfield	.71	Duluth	-.25
Portland, Me.	.71	Lincoln	-.26
Cleveland	.59	Concord	-.27
Atlanta	.53	Port Huron	-.39
Columbia	.43	Milwaukee	-.39
Savannah	.37	Mobile	-.40
New York	.30	Ogden	-.41
Louisville	.27	Seattle	-.41
Minneapolis	.26	Charlotte	-.41
New Orleans	.24	San Diego	-.41
St. Paul	.24	Des Moines	-.42
Oshkosh	.19	Buffalo	-.43
Topeka	.18	Richmond	-.44
Norfolk	.17	Pueblo	-.44
Detroit	.11	Baltimore	-.48
Wilmington	.09	Reading	-.48
Kansas City	.04	Natchez	-.49
Burlington, Ia.	.01	Cheyenne	-.50
Los Angeles	0	Chattanooga	-.54
Syracuse	-.03	Charleston	-.54
Sioux Falls	-.03	Tacoma	-.56
Columbus	-.05	Burlington, Vt.	-.68
St. Louis	-.06	New London	-.68
Trenton	-.07	Philadelphia	-.75
Boston	-.12	Dallas	-.85
Portland, Or.	-.15	Birmingham	-.92
Ft. Worth	-.15	Winona	-.95
El Paso	-.17	Omaha	-1.10
		Chicago	-1.19
		Albany	-1.20

.12). The strongest independent correlate was size-density ($\beta = -.44$), indicating that while few cities effectively integrated their surrounding areas in 1890, the largest and most densely populated cities fared still worse. The only other substantial correlate of local dominance was ethnic social organization ($\beta = .38$). This may indicate that the "pull" of ethnic bonds operated as one of the few integrative metropolitan area forces. The relative invariance of local dominance and the corresponding inability to account for substantial variations in local dominance make further speculation unwarranted. However, note that of the four most locally dominant cities in 1890, all except Pittsburgh had extensive political unification with their metropolitan areas by 1970.

This ranged from the integration of specific functions (San Francisco and the Bay Area Authority) to complete consolidation (Jacksonville and DuVal County). Thus, local dominance appears to display considerable historical continuity. Pittsburgh is an exception, but its very high local dominance was probably due to the large adjoining population of Allegheny City which became part of Pittsburgh shortly after 1890.

Historical Changes

Changes in the U.S. as an urban system suggest that some changes in the nature of urban dominance, especially at the local level, must have occurred correspondingly. Developments in transporta-

Table 6. Relationships Predicting 1970 from 1890

Independent Variables	Dependent Variables			
	Voluntary Organization Headquarters, 1970		Federal Reserve Drafts, 1970	
	Zero Order <i>r</i>	Partial <i>r</i>	Zero Order <i>r</i>	Partial <i>r</i>
National Dominance, 1890	.567*	.553**	.498*	.499**
Federal Reserve Drafts, 1890	.237	.185 ^b	.160	.166 ^b

* $p < .01$.

* Controlling for Federal Reserve drafts, same date.

^b Controlling for national dominance, same date.

tion and communication, for example, undoubtedly have increased local spheres of influence and modified relationships among nearby cities (Hawley, 1971). However, historical analyses of what we have termed national and regional dominance suggest that there has been substantial continuity over time; that is, cities' dominance locations at the turn of this century appear to have changed little in ensuing years (Duncan and Lieberman, 1970). The capacity of various measures to reflect that continuity over time, therefore, can provide an additional criterion for evaluation of the measures themselves.

This assessment requires that the measures of dominance used for 1890 be pitted against each other in terms of their ability to predict future states of cities. The most critical test involves a comparison between a measure of multifaceted dominance and a conventional fiscal or economic measure. The former is readily available in the form of the national dominance factor. Our 1890 fiscal measure is the volume of Federal Reserve Bank drafts. The Federal Reserve Banks have been a widely used indicator of (national) dominance when the financial aspects of dominance were stressed. Their utilization was initially suggested by Gras (1922) and they were used shortly afterward as the criterion of metropolitan status by Park and Newcomb (1933). This early use simply differentiated between major urban centers and all others according to the presence or absence of Federal Reserve Banks. More variation was later built into the measure by focusing upon the monetary value of bank drafts drawn on Federal Reserve Banks outside the city's Federal Reserve District. Cities are then more

elaborately differentiated as (nationally) dominant cities according to this volume of banking activity (Duncan et al., 1960).

Our first dependent variable is the number of voluntary organization headquarters located in a city in 1970. If such primarily nonfiscal aspects of cities are derived largely from the fiscal structure, then city scores on voluntary organizations in 1970 should correlate about equally well with either fiscal dominance in 1890 (Federal Reserve banking) or multifaceted dominance in 1890 (the national dominance factor). The relationships, presented in Table 6, show that the multifaceted dominance factor provides unambiguously better prediction.

However, it might also be argued that if national dominance is primarily a fiscal phenomenon, then the historical predictability of nonfiscal considerations (such as voluntary organizations) is not highly relevant. Therefore, to offer a more critical test, we examine Federal Reserve activity itself in 1970. The 1890 (independent) variables remain the same. These results are also presented in Table 6.

The differences in predictability are equally large whether the dependent variable is fiscal or nonfiscal. In both cases, relationships with the multifaceted dominance factor are substantially higher. Therefore, we suggest that if dominance is to be viewed as reflecting an enduring structural arrangement, that structure is best conceived as fiscal, intellectual, cultural and organizational, rather than fiscal alone.

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AGE, FERTILITY EXPECTATIONS AND PLANS FOR EMPLOYMENT *

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Fertility, female labor force participation, and the relationship between them are key subjects in a number of theoretical and applied areas of sociology. Because sex role norms and the widespread use of birth control devices have given American women much control over their fertility and substantial choice in their labor force activity (or inactivity), understanding the development and interrelationship of labor force participation plans and fertility expectations assumes great importance in understanding actual labor force participation and actual fertility. As a step toward understanding this development, we describe and attempt to explain the effect of women's age on the relationship between their labor force participation plans and their fertility expectations. Using data from a national sample of young women aged 19 to 29 in 1973 (N=3,589), we find a strong, linear relationship ($r=-.96$) between women's age and the effect of their plans for labor force participation on the number of children that they expect to bear in their lifetime. An explanation of this finding (called the Learning Hypothesis) is advanced which survives tests against several plausible alternative hypotheses. Policy implications and productive paths for future research are discussed.

This paper reports an investigation of the effects of a woman's age on the relationship between the number of children she expects to bear in her lifetime and her long-term plans for labor force participation. The motivation to study women's plans for childbearing and paid employment comes from observing that American women now exert a great deal of control over both their fertility and their labor force participation (see Bowen and Finegan, 1969; Sweet, 1973; Ridley, 1958; Bumpass and Westoff, 1970). If women control their fertility and labor force participation, then understanding the relationship between their plans for paid employment and their plans for childbearing is a prerequisite to understanding their actual fertility and labor force activity.

Our motivation to study the effects of a woman's age on the relationship between the number of children she expects to bear and her long-range plans for labor force participation comes from two sources, one theoretical and the other policy-oriented. On the theoretical side, it is clear

that an adequate understanding of the relationship between labor force participation plans and fertility expectations must include knowledge of the etiology of that relationship. Several past studies have laid great stress on the time at which fertility and employment plans become linked, and certain analysts have argued that the timing of events in a woman's life prior to childbearing has important consequences for the development of her tastes for employment and motherhood (e.g., Lipman-Blumen, 1972; Rossi, 1968; Presser, 1971; Bumpass, 1969). Tien (1967) and Willis (1973) have argued that labor force participation plans and fertility expectations are not established until marriage; and Blake (1969) presents data which suggest that, by the end of high school, girls have already developed inter-related tastes and long-range plans for employment and fertility. For the moment, conflicts between Blake, Tien and others are not critical. Rather, the points to be culled from these studies are that temporal aspects of the development of the relationship between labor force participation plans and fertility expectations have been widely recognized as theoretically important and, to the best of our knowledge, there is no rigorous empirical analysis of the effects of women's age on this rela-

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tionship. This paper is written to help fill this gap.

Our second motivation to study the effect of women's age on the relationship between their labor force participation plans and fertility expectations derives from recent proposals to manipulate the birth rate in industrialized countries by motivating more women to plan active labor force participation throughout adulthood (see Blake, 1969; Davis, 1967; Hoffman and Nye, 1974). These fertility reduction schemes would seem to offer a technology for lowering fertility even in nations where effective birth control is already widely used, since they operate by altering women's motivation to bear children rather than their ability to avoid doing so. However, the efficacy of this strategy depends entirely on a strong negative effect of labor force participation plans on the fertility expectations of women who are *in the prime childbearing years*. Thus, for example, if a substantial inverse effect of labor force participation plans on fertility expectations does not develop until women are, say, 25 years old, then women who start their childbearing before they are 25 would have only limited ability to reduce fertility to accommodate labor force participation plans (though their work plans might make them regret their earlier fertility). So, the research reported here is also offered as a modest step toward evaluating some hard-nosed suggestions for implementing population policy.

Before reporting our findings, we review existing literature on which our research builds and derive the key hypothesis tested in our empirical analysis. Then we give a brief description of our data and statistical methods. And, finally, we report our findings and discuss their implications.

A Hypothesis about the Effects of Age on the Relationship between Labor Force Participation Plans and Fertility Expectations

That employment and childrearing are competing activities is demonstrated by the wide variety of conditions under which they are negatively related. Women

who have large families are less likely at any point to be in the labor force and tend to have worked a smaller proportion of their lives than women with fewer children (Sweet, 1973; Kupinsky, 1971; Mason, 1974). Employed women usually have smaller family-size expectations and ideals than their non-employed counterparts (Pratt and Whelpton, 1956; Ridley, 1958); and females who plan to be working at some time in the future plan to have fewer children than women with no such intentions to work (Whelpton et al., 1966; Ryder and Westoff, 1971).

More recent data suggest the same pattern. In 1974, 30 percent of the married, husband-present 25- to 34-year-old mothers with children under three years of age were in the labor force; in that same year, 78 percent of the childless husband-present married women in that same age group were labor force participants (U.S. Department of Labor, 1975:62).

The sheer difficulty of combining the roles of employee and mother suggests reasons for the strong, negative association between childbearing and labor force participation. Employed mothers must arrange and pay for child care during the hours that they are on the job and, upon returning home from work, they tend to find themselves with a full complement of household duties (see Vanek, 1974). Thus, for the woman who wants or merely expects to be employed after childbearing, limiting fertility offers an obvious rational strategy for coping with the combined demands of childrearing and employment.

While limitation of fertility can serve as a strategy for minimizing the workload of women who plan to be employed after childbearing, recent research on women's careers also suggests that fertility limitation may serve as a rational strategy for maximizing the quality of a woman's post-childbearing employment. That is, when a woman interrupts her labor force participation in order to bear and raise children (and most women still do), the skills and knowledge she uses on the job tend to become obsolete and/or forgotten through disuse. As skills and knowledge (her human capital) depreciate, the woman becomes less valuable to employ-

ers; and the wages she can earn and, possibly, other desirable features of the jobs she can command deteriorate (Ross, 1973; Mincer and Polachek, 1974). But the fewer children a woman bears, the less time she must take out of the labor force to raise her children to an age where they can be left to the care of others while the mother is at work, other things being equal. Thus, a woman who plans to be employed after childbearing might rationally plan to limit her childrearing-related interruptions in labor force participation by limiting the number of children she bears, and thereby minimize the extent to which this childbearing-related interruption degrades the quality of her employment after childbearing.

Although limiting fertility might serve as a rational strategy for women to reduce the adverse effects of their child care duties on the quality of their employment following childbearing, women are likely to follow such a strategy only to the extent to which they are aware of the deleterious effect of labor force participation interruptions on the quality of post-interruption employment. Accurate information about wages, the availability of jobs and other market conditions is known to be unevenly distributed, and many workers do not have the information necessary to make optimal work-related decisions (Gordon, 1972; McCall, 1970; Rees and Shultz, 1970). Building on these findings, we speculate that women learn about the labor market as they age, and that their information about the effects of interruptions in labor force participation improves as they pass through their late teens and twenties. We suggest that as women grow older, they increasingly realize that their satisfactions from future employment are likely to improve if they limit the number of children they plan to bear. That is, women become increasingly aware that (1) their childcare responsibilities will cause them to interrupt their labor force participation, (2) employment interruptions reduce their employability and wage potential and (3) that they can reduce the length of their child-care-related employment hiatus by reducing the number of children that they bear. Thus, we hypothesize that the effect of women's labor

force participation plans on their fertility expectations becomes increasingly negative as they age. We call this hypothesis the Learning Hypothesis, and the remainder of this paper is devoted to evaluating its worth as an explanation of the effect of women's age on the relationship between their labor force participation plans and fertility expectations.

Data and Method

Our strategy is to estimate the parameters of a model of labor force participation plans and fertility expectations separately for women in each of several different age groups. Once these parameters are estimated, age-related changes in the effects of labor force participation plans on fertility expectations can be ascertained by examining age-group differences in the relevant parameters of the model. A model which is appropriate for this strategy should have two properties which are worth mentioning in some detail at this point. First, the model should allow for the complex relationship between labor force participation plans and fertility expectations. The empirical and theoretical work of some researchers suggests or implies that fertility plans affect labor force participation plans. (Sweet, 1973; Tien, 1967; Bowen and Finegan, 1969; Cohen et al., 1970).¹ Other work suggests or implies that labor force participation exerts a negative effect on the number of children that women want, expect, consider ideal, or actually have (Pratt and Whelpton, 1956; Ridley, 1958; Whelpton et al., 1966; Kupinsky, 1971; Mason, 1974). Perhaps the most sophisticated works on the subject are those that suggest that women's preferences for employment and childbearing *both* affect each other (Blake, 1970; Terry, 1974). Waite and Stolzenberg (1976) estimate a model which allows (but does not require) causality to run in either or both directions between labor force participation plans and fertility expectations. They found evidence of reciprocal effects, though the

¹ Our statement about the implications of this work depends upon the assumption that fertility and labor force participation are not completely unplanned activities.

impact of labor force participation plans on fertility expectations was much greater than the effect running in the opposite direction.

A second important methodological point is that past theory and empirical research strongly suggest that women's plans for both fertility and labor force participation are related to a number of their background characteristics such as race, educational attainment, and marital status. Thus, we include some of these background characteristics in our statistical models, lest we mistake the effects of a neglected background factor which causes both work and fertility plans for a relationship between these variables.

The model of labor force participation plans and fertility expectations which was developed by Waite and Stolzenberg (1976) satisfies the two methodological requirements just mentioned; it allows (but does not require) one-way or reciprocal causation between women's plans for future labor force participation and expected family size and it includes, as common antecedents of both fertility expectations and labor force participation plans, several variables which have been shown to affect both work plans and childbearing expectations. We will use this model in the present analysis. We will also use the same data base that we employed in the earlier analysis (Waite and Stolzenberg, 1976).

Since the Waite-Stolzenberg model and the data used to estimate it were described in detail earlier in this *Review*, only brief descriptions are needed here. Data are drawn from the National Longitudinal Survey of Labor Market Experiences of Young Women (NLS) done by the U.S. Bureau of the Census and the Ohio State University. The sample started in 1968 with about 5,000 women who were then representative of U.S. females aged 14 to 24 years. By 1973, about 86 percent of the original sample was intact. After eliminating cases with missing data, we were left with an N of 3,589. Variables used in the model are described briefly below and more extensively in Waite and Stolzenberg (1976):

LFPP, or work plans, is a measure of the respondent's plans for labor force

participation at age 35. This variable equals one if the young woman plans to be employed, zero if she is not sure or plans to be out of the labor force.

FE, fertility expectations, is the total number of children which the respondent plans to have in her lifetime.

SIBS is the number of siblings the respondent had in 1968.

IFS, ideal family size, equals the number of children that the respondent considers "ideal" for a family.

BLACK, a dummy variable for the young woman's race, equals one if she is black, zero otherwise.

ED equals the number of years of schooling completed by the respondent.

MSP is a dummy variable for marital status which equals one if the young woman is married and living with her husband, zero otherwise.

AGE equals the respondent's age in years at the time of the 1973 interview.

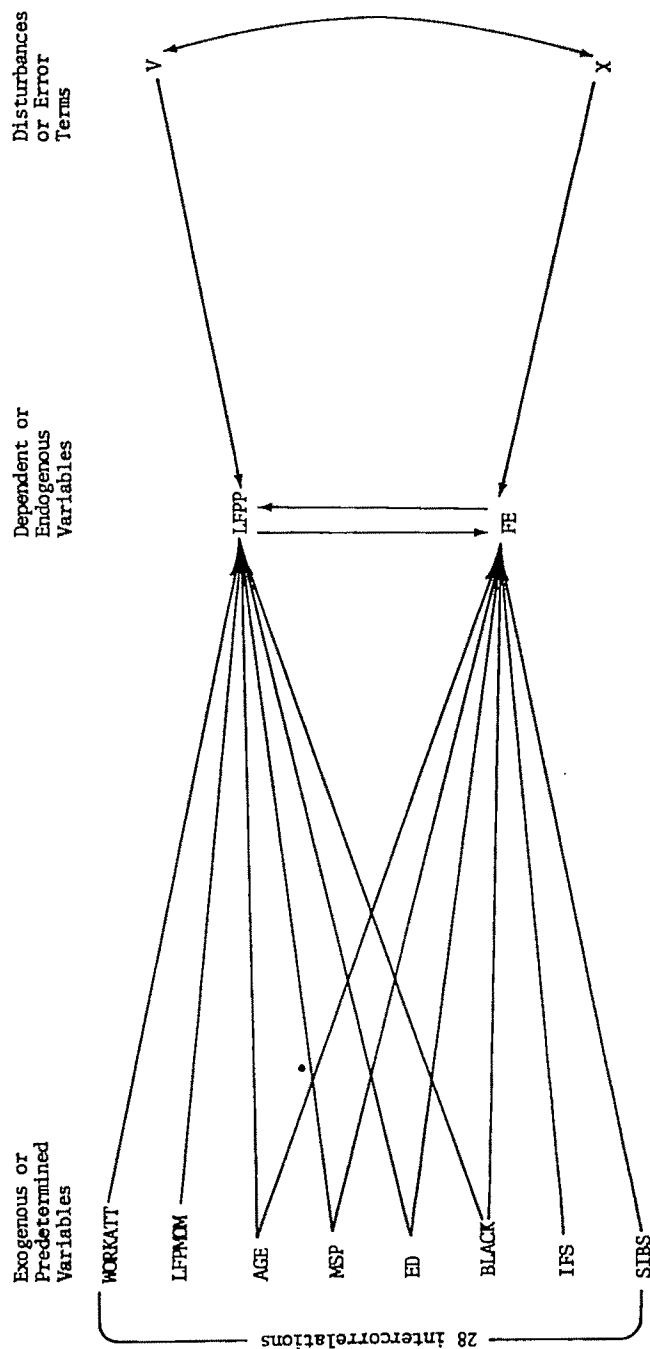
LEPMOM, the labor force participation of the respondent's mother when the respondent was 14 years old, equals one if the mother worked, zero otherwise.

WORKATT, work attitudes, is a nine-item scale of the young woman's beliefs about the costs and benefits of labor force participation by married women.

All correlations, means and standard deviations used in the present paper are reported in Suter et al. (1975).

The Waite-Stolzenberg model is presented pictorially in Figure 1 and mathematically in the two equations under the path diagram. For present purposes, the key feature of the model is that it allows, but does not require labor force participation plans and fertility expectations to affect each other. These effects are, of course, "net" of the effects of variables on the left side of the diagram (corresponding to the right sides of the equations).² Parameters of the model are estimated by two-stage least squares.

² The Waite-Stolzenberg model does not allow a *direct* effect of work attitudes on fertility but it does allow an indirect effect through labor force participa-



Corresponding equations:

$$\widehat{LFPF} = b_0 + b_1 \text{WORKATT} + b_2 \text{LFPDM} + b_3 \text{AGE} + b_4 \text{MSP} + b_5 \text{ED} + b_6 \text{BLACK} + b_7 \text{FE}$$

$$\widehat{FE} = c_0 + c_1 \text{AGE} + c_2 \text{MSP} + c_3 \text{ED} + c_4 \text{BLACK} + c_5 \text{IFS} + c_6 \text{SIBS} + c_7 \text{LFPF}$$

Figure 1. A Basic Model of Labor Force Participation Plans and Fertility Expectations

We feel obliged at this point to reiterate a commonly misunderstood point: the nonrecursive model used in this analysis *allows* work plans and fertility expectations, both measured in the same survey, to influence each other. However, this model also allows one-way causation or no causal influence between the two at all. The presence of simultaneity is, in some sense, an empirical question, and we do not force it to appear in our model.

Age and the Relationship between Labor Force Participation Plans and Fertility Expectations

In order to investigate the effects of a woman's age on the relationship between

tion plans. This restriction is necessary to allow estimation of coefficients of causal variables. Rather than contradicting students of fertility, such as Rertert and Bumpass (1974) and Scanzoni (1975), who have argued that sex role attitudes and desired fertility are related, we are building on their work by specifying a mechanism by which attitudes toward female employment are related to the number of children that women plan to bear. In calculations not reported here, we obtained further support for our use of WORKATT in the Basic Model by showing that substituting a different variable in its place produced only the most trivial changes in the estimated effect of LFPP on IFS. This result is the sort of thing that one hopes for when using two-stage least squares—changing the instrumental variables does not alter the all-important relationship between the endogenous variables. Further, we have found that IFS can be deleted from the Basic Model without producing changes in the conclusions reported in Waite and Stolzenberg (1976). However, deleting IFS reduces the efficiency of the instruments and, thus, greatly increases the instability of the estimates. Therefore, we find it prudent to leave IFS in the model. And, finally, we can bring one new piece of evidence to bear in support of our decision to exclude WORKATT from the equation for FE. Following Kohn and Schooler (1973, who follow Duncan et al., 1968), we calculated the residuals of the two equations in the Basic Model and then correlated these residuals with the exogenous variables. If our assumptions are correct regarding the exclusion of WORKATT from the equation for FE, then the correlation between WORKATT and the residuals from the equation for FE will be zero. The actual correlation between WORKATT and the residuals is $-.0088$, which seems sufficiently small to be regarded as no different from zero. Correlating WORKATT with the residuals is a weak test of our assumptions but, taken together with our substantive arguments and other empirical results, we believe that it completes a solid case in support of our decision to exclude WORKATT from the equation for fertility expectations.

her fertility expectations and labor force participation plans, we estimated the parameters of our Basic Model separately for women in five age categories: 19 to 20 years old, 21 and 22, 23 and 24, 25 and 26, and 27 to 29 years of age. The complete results of these regression equations are provided for reference in Suter et al. (1975). However, inasmuch as these regressions provided some 70 metric coefficients, 70 standardized coefficients, 70 standard errors, and 10 R-squared statistics, our discussion of these results will be facilitated by using less detailed tables in this article. Table 1 presents the metric coefficients for work plans in the equation for fertility expectations by respondent's age. Note that the coefficients for work plans are all negative and that they increase in absolute value strictly monotonically as age increases. Two features of these results are striking. First, the difference between the coefficient for 19- and 20-year-olds and the coefficient for 27-through 29-year-olds is substantial. For the youngest group, the coefficient is about $-.28$, indicating that when the effects of other variables in the Basic Model are held constant, women's plans to participate in the labor force at age 35 decrease their expected family size by an average of .28 children. For the oldest age group, the coefficient for work plans is about -1.44 more than five times as large as the effect of work plans on the youngest group. Thus, for 27–29-year-old women, plans to participate in the labor force at age 35 decrease expected family size by an average of 1.44 children, net of other factors included in the equation for fertility expectations. Inasmuch as the 27–29-year-old women in the NLS sample expected to bear an average of 2.5 children in their lifetime, the estimated effect of work plans on the expected fertility of these women reasonably can be regarded as enormous.

The second striking feature of Table 1 is the linearity of the relationship between women's age and the metric coefficient for work plans. Space limitations do not allow graphic presentation of this relationship, so we use the Pearsonian correlation to measure the degree of linear association between age and the effect of LFPP on

Table 1. Net Effects of Labor Force Participation Plans (LFPP) on Fertility Expectation (FE), by Age Group, with Corresponding Multiple R-Squared Statistics

Age Group	Partial Regression Coefficients		R ² for Equation	N
	Metric (Unstandardized) Coefficients	Beta (Standardized) Coefficients		
19-20	-.278 (.422)	-.111 (.168)	.430	663
21-22	-.646 (.290)	-.292 (.131)	.273	739
23-24	-.691 (.413)	-.308 (.184)	.295	682
25-26	-.804 (.475)	-.351 (.207)	.123	686
27-29	-1.439 (.627)	-.540 (.235)	.037	819

FE. The zero-order correlation between the midpoints of the age groups and the coefficients for work plans is $-.955$ and is significantly different from zero at the 2.5 percent level ($F = 30.75$). Thus, age explains 91.1 percent ($= [-.955]^2$) of the variance in the coefficient for work plans. We fitted a straight line to the metric coefficients reported in Table 1 by least-squares regression,³ obtaining the following parameter estimates (where b is the coefficient of work plans):

$$\hat{b} = 1.963 - 0.1154 \text{ Age } R^2 = .9111. \quad (1)$$

According to equation (1), the effect of work plans on fertility expectations changes by $-.1154$ expected children per year of age.

³ The coefficients of work plans in the different age groups have different standard errors. Thus, the ordinary least-squares assumption of homoscedasticity is violated in our estimation of the parameters of equation (1). This problem can be overcome by using the inverses of the squared standard errors of the coefficients for work plans as estimates of the main diagonal of the variance-covariance matrix of the residuals in a generalized least-squares (GLS) regression of b on age. Since the coefficients for work plans were estimated from disjoint subsets of the NLS sample, the off-diagonal elements of the variance-covariance matrix of the residuals are all zero. Following this procedure we obtain the following GLS regression of b on Age:

$$b = 1.6666 - .1033 \text{ Age } R^2 = .862. \quad (1a)$$

The F statistic for the GLS regression is 18.66, indicating that the hypothesis of "no relationship" between age and the coefficient for work plans can be rejected at the 2.5 percent significance level.

We have also derived an alternative method for estimating the relationship between age and the effect of LFPP on FE. The alternative method produces nearly identical estimates of the relationship between age and the effect of LFPP on FE, increasing our confidence in the estimates presented in equations (1) and (1a). These calculations are presented in Stolzenberg and Waite (1976).

Thus, our empirical findings are consistent with the Learning Hypothesis presented at the start of this paper. We now consider (and reject on empirical grounds) some alternatives to the Learning Hypothesis which are also consistent with the empirical results which we have just presented.

Testing Alternative Hypotheses

According to the Learning Hypothesis, as women grow older, they tend increasingly to realize that the rewards of their post-childbearing employment are likely to be greater if they have a small rather than a large family. Thus, as women age, the inhibiting effect of their labor force participation plans on their fertility expectations becomes stronger. Unfortunately, the NLS surveys of young women do not contain questions designed to determine young women's ideas about the effects of labor force participation interruptions on their employability and earning power. Thus, it is not possible to test all parts of the Learning Hypothesis. However, we

can provide some indirect support for our reasoning by showing that certain plausible alternative explanations of our findings are *not* true. We will consider two of these alternatives. First, we examine the possibility that our findings are caused by cohort differences in sex-role attitudes rather than age differences in knowledge of factors affecting employability and earning power. Second, we will address the possibility that we are mistaking the effects of women's life cycle stage for the effects of their age on the relationship between work plans and fertility expectations, thereby providing an erroneous interpretation of our findings.

For convenience, we will call the first alternative hypothesis the Liberation Hypothesis. According to this hypothesis, there has been a secular trend in women's ideas about proper role behavior for mothers of young children. As a result of this trend, it is more acceptable to members of recent birth cohorts than to those of less recent birth cohorts for a mother of young children to spend considerable time away from home in order to hold employment, assuming that responsible adult care is provided for the children in the mother's absence. Thus, according to the Liberation Hypothesis, women from younger cohorts who expect to be employed after childbearing expect to interrupt their labor force participation for less time per child than women from older cohorts who have similar expectations of labor force activity. As a result, members of younger cohorts find childbearing less detrimental to their future employability and earning power than women from older cohorts, and maximizing earnings and employability is less reason for members of younger cohorts to limit their fertility than for women from older cohorts. Because the data we utilize in this paper are cross-sectional, cohorts are indistinguishable from age groups. Therefore, if the Liberation Hypothesis is true, the relationship we find between age and the effect of work plans on fertility expectations is partially or even entirely the effect of cohort differences in sex-role attitudes, not developmental factors which have caused age differences in knowledge of

the effects of interruptions in labor force participation.⁴

Fortunately, the NLS data permit us to investigate the possibility that age differences in sex-role attitudes account for the relationship we have found between women's age and the effect of the labor force participation plans on their fertility expectations. The Liberation Hypothesis assumes that younger women are more liberal than older women in their sex-role attitudes and more tolerant of work outside the home by mothers of young children. Our data include four Likert-scale questions which deal with the effects of women's employment on the welfare of their families. These are items 1, 4, 6 and 8 in the work attitude scale, WORKATT, mentioned earlier and discussed in detail in Waite and Stolzenberg (1976). These items are presented in Table 2. The correlation between responses to these items and women's age are given in the right-most column of Table 2. Although the first and sixth items have correlations with age which are significantly different from zero at the one percent level, it is clear that none of these items has a strong enough association with age to account fully for the powerful relationship between age and the effect of labor force participation plans on fertility expectations. (Means and standard deviations of responses to these items for the age groups showed no non-linear associations.) A more rigorous test of the Liberation Hypothesis is available using multiple regression. Since the Liberation Hypothesis states that the rela-

⁴ One reader of this paper suggested that the effect of age on the relationship between work plans and fertility expectations might be due to cohort changes in number of children expected, with older women anticipating substantially larger completed families than their younger counterparts. One also might argue that younger women should be more likely to plan for later labor force participation than should older women. However, neither of these arguments holds up under close inspection. The zero-order correlation between Age and FE is .0364. The means and standard deviations of fertility expectations are very similar for all age groups, differing by only 0.10 child between the oldest and the youngest (2.52 versus 2.41). Also, approximately the same proportion in all age groups plan to work at age 35; .44 for those 19 and 20 versus .49 for those 27 to 29. The correlation between Age and LFPP is -.0140.

tionship between a woman's age and the effect of her work plans on her fertility intentions is due to cohort differences in sex-role attitudes, the hypothesis will be supported if the relationship between age and the coefficient for work plans in the equation for fertility expectations vanishes when the effect of these attitudes is held constant. In order to test the Liberation Hypothesis, we have computed several regression analyses which are reported in columns A and B of Table 2. In each regression, we allow the *coefficient* for work plans in the Basic Model to be caused by two independent variables: the midpoint of the age span of the women for whom the coefficient was estimated, and these women's mean value on a Likert-scale questionnaire item which measures attitudes about the effects of a mother's employment on the welfare of her family.⁵ The Learning Hypothesis predicts that the coefficient of age in these regressions will be about the same as the coefficient of age when no sex-role attitude measures are included in the equation (i.e., the coefficient will be about the same as the coefficient of age in equation [1]). The Liberation Hypothesis predicts that the coefficient for age will vanish, or at least be substantially reduced when sex-role attitude measures are included in the equation predicting the coefficient for work plans. Remembering that the coefficient for age in equation (1) is $-.1154$, it is clear from the regression results presented in column A of Table 2 that holding sex-role attitudes constant causes only trivial differences in our estimate of the effect of women's age on the coefficient for work plans in the Basic Model. The coefficient for age is changed only slightly when the effect of sex-role attitudes is held constant. Similarly, looking at column C of Table 2, notice that the correlation be-

tween age and the coefficient for work plans remains high when it is partialled on the various sex-role attitudes. So we have an empirical basis for rejecting the Liberation Hypothesis. We now turn to another hypothesis which competes with our explanation of age differences in the effects of labor force participation plans on fertility expectations.

A second alternative to the Learning Hypothesis is the hypothesis that it is a woman's life cycle stage, not her age, which determines the impact of her labor force participation plans on her fertility expectations. The concept of the life cycle, with regular, sequential stages of development, would seem to have enormous potential as an explanatory variable in the analysis of female labor force participation (Kish and Lansing, 1957; Waite, 1976). Life cycle stages are typically conceived as periods of time bounded by milestones in the life of an ideal-typical individual (Glick, 1947). In research on women's fertility and labor force participation, these milestones would reasonably include the woman's marriage, her first childbirth, and her final childbirth. These events mark obvious time-ordered discontinuities in the experiences to which a woman is exposed. One might conjecture that changes in life cycle stage—rather than a more-or-less continuous process of acquiring knowledge as one grows older, as suggested by the Learning Hypothesis—is the mechanism by which women become more knowledgeable about the demands of child care and, consequently, more likely to curtail their expected fertility to accommodate their plans for employment. And, since life cycle stages are sequential, phenomena which are caused by progress through the life cycle will also be correlated with age. Thus, it is plausible that the relationship that we have observed between age and the coefficient for work plans is spurious and can be accounted for by the correlation between age and life cycle state. We call this plausible alternative to the Learning Hypothesis the Life Cycle Hypothesis.

Since the mean age of women in the NLS sample increases strictly monotonically

⁵ Since we were able to estimate the Basic Model separately for only five age groups, we have only five age-specific coefficients for work plans. Thus, the number of independent variables that can be entered into a regression analysis of these coefficients is extremely limited. So we perform five separate regressions, each one regressing the coefficient for work plans on age and a different sex-role attitude indicator.

Table 2. Regressions of Age-Specific Coefficient for LFPP on Age and Each of Several Sex Role Attitude Indicators^a

Sex Role Attitude Indicator ^b	Metric Coefficient for Age ^c	Metric Coefficient for S.R.A. Indicator	Partial Correlation between Age and Coefficient of LFPP (Net of S.R.A. indicator) ^d	Zero-Order Correlation between Age and S.R.A. Indicator
	A	B	C	D
1. Modern conveniences permit a wife to work without neglecting her family	-.1064	-.3630	-.831	.0766
2. A wife who carries out her full family responsibility doesn't have time for outside employment	-.1033	-.9906	-.943	.0425
3. The employment of wives leads to more juvenile delinquency	-.1101	-.1746	-.843	.0938
4. Working wives lose interest in their homes and families	-.1133	-.3039	-.944	.0262
5. WORKATT (a nine-item Likert scale of attitudes about the costs and benefits of female labor force participation; described in text) ^e	-.1057	-.0891	-.919	-.0181

^a The dependent variable in each of these equations is the metric coefficient for LFPP in the second equation of the Basic Model, the equation for FE. The Basic Model is estimated separately in each of five different age groups, providing an N of five for each of the regressions reported in this table.

^b Items scored on a five-point Likert scale from "strongly agree" to "strongly disagree." The first item listed in this table was scored from one (for "strongly agree") to five (for "strongly disagree"). Items 2, 3, and 4 scored from five (for "strongly agree") to one (for "strongly disagree"). The value of an S.R.A. indicator for an age group is the mean score of the item for women in that age group.

^c Age is the midpoint of the interval which defines each of the five age groups for which the Basic Model was estimated.

^d Partial correlation of Age and coefficient for LFPP net of S.R.A. indicator.

^e WORKATT includes the first four sex-role attitude items listed in this table.

cally with each successive life cycle stage, the coefficient for work plans in the equation for fertility expectations must have a larger value for women in earlier life cycle stages if the Life Cycle Hypothesis is to remain plausible. Looking at columns A and B of Table 3, notice that the coefficients for LFPP are inconsistent with the Life Cycle Hypothesis: the absolute value of the metric coefficient for work plans reaches a maximum at stage 1, decreases from stage 1 to stage 3 and then increases substantially in stage 4. So we have an empirical basis for rejecting the Life Cycle Hypothesis.

It seems worthwhile to stress that we have *not* argued that there are no life cycle stage effects on the impact of work plans on fertility expectations. Rather, we have argued that we have not mistaken those effects (if indeed they exist) for the effects of age. As we mentioned briefly, the effects of life cycle stage on the fertility expectations-work plans relationship is of considerable intrinsic interest, and it would be desirable to investigate those effects in some detail. However, women

have considerable ability to choose when to move from one life cycle stage to another, making it necessary for any research on life cycle stage effects to be designed to avoid biases caused by self-selection into the various life cycle stages. Unfortunately, the most efficient research design for investigating life cycle stage effects is not the most efficient design for investigation of age effects, and we must defer our investigation of life cycle stage effects until some later time.

The Effects of Fertility Expectations on Labor Force Participation Plans, by Age and by Life Cycle Stage

In an earlier analysis, Waite and Stolzenberg (1976) found that NLS women's fertility expectations had only very slight effects on their plans for labor force participation at age 35. We concluded that this finding was surprising, but that it did not contradict earlier research which found that mothers of preschool-age children are less likely to participate in the labor force than women who do not have

Table 3. Effects of Labor Force Participation Plans (LFPP) on Fertility Expectations (FE), by Life Cycle Stage, with Related Statistics ^a

Life Cycle Stage	Partial Regression Coefficient of LFPP in Equation for FE ^b		R ² of Equation for FE	Mean Age	N ^c
	Metric (Un- standardized) Coefficients	Beta (Standardized) Coefficients			
	A	B	C	D	E
1. Never married and no children ever born	-1.052 (.398)	-.392 (.148)	.369	21.6	894
2. Married with no children ever born	-0.490 (.292)	-.237 (.141)	.208	23.1	676
3. Married with some children and expecting to bear more children	-0.309 (.394)	-.130 (.166)	.387	24.4	775
4. Married with some children and no more children expected	-0.980 (.394)	-.519 (.209)	.090	26.0	797

^a All variables defined in text.

^b Partial regression coefficients estimated by the method of two-stage least squares. Statistics reported in columns, A, B, and C in this table obtained by estimating the equation for fertility expectations described in Figure 1 separately for women in different life cycle stages. Standard errors of coefficients in parentheses.

^c 447 women were divorced, separated, widowed, not "spouse present," did not report their marital status, or were never-married mothers. These women were excluded from the life cycle stage analyses.

young children (see Sweet, 1968). Rather, we argued that

if childbearing reduces female labor force participation primarily by putting intense pressure on mothers to stay home with their preschool-age children, then finding of negligible effects on fertility expectations on work plans might well be due to the high probability that women in our sample expect that their children will be of school age by the time the respondents themselves are 35 years old. (Waite and Stolzenberg, 1976:249)

If this explanation of our earlier findings is correct, then we should find no large life cycle or age differences in the effects of fertility expectations on work plans. Consistent with this expectation, we find that the effect of fertility expectations on labor force participation plans is uniformly small in all life cycle stages and all age groups in which we estimated the Basic Model. In the five age groups, the metric coefficients are, from youngest to oldest, $-.038$, $-.013$, $-.016$, $-.006$ and $-.050$. Standardized coefficients in those age groups are, respectively, $-.096$, $-.029$, $-.036$, $-.015$, and $-.135$. In the four life cycle stages defined earlier in this paper, the metric coefficients are, from first stage to last, $-.046$, $-.012$, $-.033$ and $.032$. In those four stages, the standardized coefficients are, respectively, $-.123$, $-.025$, $-.079$ and $.060$. (Space limits our presentation of standard errors, R-squared's, etc.; see Suter et al., 1975, for those data.) These findings leave Waite and Stolzenberg's earlier conclusions undisturbed.

Summary and Conclusions

The research reported here was undertaken to describe and explain the effect of young women's age on the relationship between their fertility expectations and plans for future labor force participation. We found that the effect of labor force participation plans on fertility expectations varies from a mild inhibiting effect for 19- and 20-year-olds to a strong negative influence for 27- through 29-year-olds.

To explain this finding, we advanced the Learning Hypothesis. According to the Learning Hypothesis, the inverse ef-

fect of work plans on fertility expectations increases as women age from 19 to 29 because their knowledge of the demands of motherhood and their information about the labor market improve during that time; as women grow older, they become increasingly aware of the extent to which childbearing and childrearing are likely to interrupt their labor force participation and, consequently, reduce the quality of their subsequent employment. As a result, we reasoned that the extent to which women limit their expected fertility to accommodate their employment plans increases as they grow older. The Learning Hypothesis is tested only indirectly in this paper because we lack the data necessary to perform a direct test. However, the hypothesis is consistent with the facts available to us at this time. Further, we test two plausible alternatives to the Learning Hypothesis and are able to reject them both, thereby increasing our confidence in our explanation of age differences in the effect of labor force participation plans on fertility expectations.

If the Learning Hypothesis is true, it would appear to have important implications for certain fertility reduction strategies which have been advanced in recent years. For example, it has been suggested that the birth rate in the United States and other nations can be lowered by increasing employment opportunities for married women (Blake, 1969; 1970; Davis, 1967). The logic behind this proposal is to offer women a choice between careers as mothers and careers as labor force participants. However, our findings suggest that the success of this strategy in reducing *actual* fertility may depend heavily upon the age at which women bear children. If women tend to do their childbearing at an age when the effect of labor force participation plans on fertility expectations is high, then job opportunities which induce females to plan future labor force participation would seem likely to have a large impact on the number of children that women actually bear. But if women tend to do their childbearing at an age when labor force participation plans have only a weak effect on fertility expectations, then economic and social conditions which induce females to plan future em-

ployment seem likely to have only a weak effect on actual fertility, regardless of how much these work plans subsequently might lead mothers to wish that they had been less fertile. However, if the Learning Hypothesis is correct, then the relationship between women's age and the effect of their labor force participation plans on their fertility expectations is not fixed developmentally, but varies as a function of their knowledge of the labor market and the demands of childbearing and childrearing. Thus, it seems possible to use mass educational campaigns to speed up the process by which women acquire this knowledge and, thereby, to lower the age at which labor force participation plans exert a significant impact on fertility expectations. We suspect that an educational campaign of this sort might be used to increase the efficiency of a program designed to reduce population growth by increasing the proportion of women who plan to be employed after childbearing. However, we are acutely aware that we are unable to provide a direct test of the Learning Hypothesis, and so we dare say only that these inferences seem reasonable in light of our findings, but that they must await further, more direct testing before being accepted, let alone applied as part of a population policy.

Finally, a few additional words about our methods and models are in order. Careful readers undoubtedly have remained aware throughout this paper that our measure of fertility expectations, FE, has two components, actual past fertility and expected future fertility. One reasonably might elaborate the models we have utilized here and elsewhere to untangle the three-way relationship between these two fertility expectation components and labor force participation plans. Indeed, consideration of the role of past fertility led us to consider the Life Cycle Hypothesis in this paper, and we suggest that construction of more elaborate models which distinguish past from future fertility would be a logical next step in studying the process by which women's fertility and work plans become interrelated.⁶

However, the present analysis was designed primarily to answer theoretical questions which currently exist in the sociological and, to a lesser extent, the economic and family planning literatures on childbearing and female employment. We have tried to confront those issues in their most basic form as they have appeared in the literature.

Similarly, we believe that another next step would be to include measures of current or past labor force participation in the model. We have left labor force participation experience out of the present analysis because past research implies that the most important effects of work experience are probably not linear and additive but, rather, a series of interaction effects. For example, we would expect that the effects of employment interact strongly with school enrollment status in determining future labor force participation. For those who are enrolled in school, nonparticipation in the labor force probably does not indicate a low propensity for future labor force participation. But for those who are not enrolled in school, nonparticipants are probably less likely to be part of the labor force in the future than women who presently work. Further, according to Mason's (1974) "role hiatus hypothesis," it is not employment or even rewarding employment *per se* that induces women to develop a taste for labor force participation, but it is employment in the absence of marriage that makes career women out of working girls, and so on. The point being made is simply that it is not trivial to add "work experience" to an analysis of fertility and labor force participation plans. Rather than attempt too much in the space of a single paper, we have left considerations of the role of work experience and of the separate effects of past and expected future fertility to later, more complex analyses. We have spent our effort here in understanding some basic features of the effect of age on the link between labor force participation plans and fertility expectations.

⁶ This would be a theoretical as well as an empirical next step, since, to the best of our knowledge,

there is no work whatsoever on the effects of labor force participation plans on parity progressions; but see Hout (1977) for an analysis of other factors related to parity progressions.

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ECOLOGICAL APPROACH TO MIGRATION REEXAMINED*

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The organizational model of migration asserts that demographic responses are the result of organizational change and that they are influenced only indirectly by technological and environmental factors. Previous research has validated the usefulness of this model in developing an understanding of the population migration process in relatively less-developed ecological units. The application of this model to more complex ecological units in this paper, however, calls into question its generalizability and leads to the consideration of an alternative model which postulates that migration is a direct response to environmental conditions and that the influences of technology and organization on migration operate indirectly through their effects on the environment. Support for the environmental model using correlation and path techniques leads to the suggestion that alternative sets of factors influence migration differently at more complex levels of development. Whether organizational or environmental factors influence migration most directly appears to be determined by which is more strongly influenced by technology.

Sly (1972) has developed and tested an ecological model of migration which builds principally upon the works of Hawley (1950), Schnore (1958) and Duncan (1959). According to this model's two major propositions, migration is a response to organizational change, and technology and environment only influence migration to the extent that they elicit organizational change. As Sly

(1972:619) argues, "environment and technology do not operate directly on migration, but effect migration through changes in organization . . . variations in the migration rates of small and large aggregates alike can be explained by observing the effects of variations in external factors on population organization." Despite the viability of this organizational model, Frisbie and Poston (1975) have argued against the temptation to assert causality within complex ecological systems on the grounds that "the direction of causality in ecological systems is surely a function of the point of analytical departure." Thus, although the findings of Frisbie and Poston are supportive of the organizational model, they argue that

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even though population size may seek an equilibrium with organization, population size must also seek an equilibrium with environmental and technological factors, particularly when the latter are themselves responses to organizational change.

Sly, however, does point out that his test of the organizational model is not wholly adequate; he confines his attention to the migration of black males from 253 Southern cotton belt counties, and suggests that further testing of the model should include consideration of areas with different and more complex sustenance organizations. Thus, although Frisbie and Poston included all nonmetropolitan counties of the coterminous United States in their analysis, they did not include the more urban and more industrialized areas of the United States. They also urge that the type of ecological analysis employed be extended to include consideration of urban areas.

The primary objectives of the present paper are to build upon the works of Sly and Frisbie and Poston by (1) directly testing the organizational model on a group of manufacturing centers and (2) altering the point of analytical departure by posing an alternative model which suggests that migration may be a demographic response to environmental conditions created by organization and technology. If the organizational model can be accepted under the vastly more complex conditions of our sample of areas, its two major theoretical propositions will have gained substantial support; rejection of the alternative model would lend still greater support to the organizational model; while rejection of both models, or the organizational model, would lend support to the notions of the complexity of causality in ecological systems.

The ecological complex as a frame of reference has been discussed in several places (Duncan, 1959; Gibbs and Martin, 1959; Schnore, 1966; Hawley, 1968; Sly, 1972; Frisbie and Poston, 1975) and will not be systematically reviewed here again. Instead, a short discussion will be provided to explain (1) how the sample of areas was selected and (2) why and how the particular indicators employed were selected.

Sample

As noted above, recent research viewing components of population change as demographic responses to changes in sustenance organization has focused upon agricultural and nonmetropolitan areas, and the researchers performing this work have urged others to examine the same questions in more complex areas. Central cities of Standard Metropolitan Statistical Areas seem ideally constituted for this purpose. Their sustenance organizations are complex and continuously undergoing change (Kasarda, 1972; Zimmer, 1975). Similarly, central cities have experienced marked population change, and a considerable amount of their white population change can be attributed to migration (Hawley et al., 1964; U.S. Bureau of the Census, 1971; Morrison, 1974). Yet, cities are vastly different in character and have developed markedly different sustenance organizations (Galle and Williams, 1963; Kasarda, 1972; Yu, 1972). In order to exercise some control over the type of sustenance organization development which has occurred in these areas, we decided to focus upon those with a strong manufacturing base. In part, this decision was predicated on the notion that these areas would have the most developed sustenance organizations (Gibbs and Martin, 1959; 1962; Hawley, 1971) and, in part, on the assumption that a group of manufacturing cities would have experienced vastly different migration experiences (Taeuber, 1972; Berry, 1973).

With these assumptions in mind, we selected central cities of SMSAs having at least 30 percent of their jobs classified as manufacturing (U.S. Bureau of the Census, 1966a). These criteria yielded a sample of 90 cities, 77 located in the industrialized Northeast and North Central states; only one city (San Jose) in the West was included.

Selection of Variables and Operationalization

Ecologically, migration is viewed as a component of areal population change and, as such, it can be considered the other than natural increase or decrease in

areal population size between two points in time. As Sly (1972:615-6) notes, measures of net migration are ideally suited for the type of ecological analysis performed. Estimates of the net migration of the white population for the cities in our sample were obtained from the U.S. Bureau of the Census (1971). These estimates were calculated using the vital statistics method; however, no adjustments were made for boundary changes between 1960 and 1970 and all annexed populations were counted as net migration.¹ Thus, the estimates are biased and tend to overstate net in-migration and understate net out-migration.

This bias, however, does not have any systematic effect upon the data for three reasons. First, one-half of the cities did not annex any new population during the intercensal period and where annexation did occur, it tended to be relatively small—less than ten percent of the 1960 population in another 36 cities. Second, by limiting our attention to the white population, we have reduced further the impact of annexation on the estimates to the extent that each annexed population was nonwhite. Third, the calculated rates of net migration actually used in the analysis further reduced the impact of annexation to the extent that the populations were differentially affected by the out-migration of whites during earlier periods (U.S. Bureau of the Census, 1972). The zero-order correlation coefficient between the percent of population annexed and the net migration rate for the 90 cities was only .032.

To identify relevant independent variables for inclusion in the analysis, we have relied largely upon the work of others (Coe, 1959; Blanco, 1963; 1964; Goldstein and Mayer, 1964; Ullman, 1965;

Greenwood, 1969; Karp and Kelly, 1971; Hansen, 1972; Hawley, 1972; Greenwood, 1973; Winsborough, 1974; Shaw, 1975). The theoretical definitions which guided our classification of these variables as organizational, environmental or technological factors are the same ones employed by Sly (1972) and Frisbie and Poston (1975).

Under the organizational rubric we included (1) the index of industrial dispersion (Sly, 1972) and (2) the percent of the white population which was unemployed in 1960. Under the environmental rubric we have placed three factors: (1) the percent of the central city population which is nonwhite in 1960, (2) the total density of the central city in 1960 and (3) the percent of central city jobs held by noncentral city residents in 1960. In addition to these factors, two technological variables have been included: central city commuting efficiency in 1960 and central city value added by manufacturing in 1960.

The sustenance organization of central cities can be viewed as consisting of four major types of niches: manufacturing, retail, wholesale, and selected services. Since each of the cities in our sample has a manufacturing-oriented sustenance organization and since manufacturing has decentralized in these cities, the index of industrial dispersion measures the access to nonmanufacturing niches (Sly, 1972:620-1). The index was calculated over the four major categories noted above, using data taken from the U.S. Census of Business (U.S. Bureau of the Census 1966a; 1966b; 1966c; 1966d). Although a number of alternative organizational dimensions could have been selected, we decided to focus on industrial dispersion because of its theoretical significance (Gibbs and Browning, 1966) and the fact that a number of studies have already demonstrated its empirical relationship to other organizational dimensions (Rogers, 1957; Clemente and Sturgis, 1972; Kasarda, 1974).

Our second indicator of the sustenance organization of cities taps the dimension of number of niches mentioned by Gibbs and Martin (1959), Sly (1972) and Gibbs and Poston (1975). It is merely the white unemployment rate which gives us an

¹ For convenience of interpreting the migration rates, we multiplied each by a negative one. Since the vast majority of cities lost population as a result of migration, this statistical procedure allows us to refer to those cities having the largest negative net migration rates as having the highest migration. This transformation has been taken into account in the hypotheses presented below. Thus, the hypothesis stating an association between migration and the unemployment rate indicates that as the unemployment rate increases the net out-migration increases.

index of the extent to which the existing sustenance organization is able to absorb the available potential workers in the city. Thus, the index provides a good idea of the extent to which there is an equilibrium between the size of population and the organization of an area (Hawley, 1950:77-9).

The three environmental factors included appear to be straightforward adoptions from previous ecological studies. The percent of the central city population which is nonwhite is an indicator of the competition which whites face for niches, while the percent of central city jobs held by noncentral city residents is another measure of external threat to the white central city population (Blalock, 1956; Heer, 1963). The third environmental variable (density) taps the dimension of "environmental deterioration" of cities, and a number of studies have indicated that high density creates situations from which whites flee (Winsborough, 1974; Humphrey and Krout, 1975).

The two indicators of technology which we employ were picked to be closely related to our indicators of organization and environment. If we assume that the major tool in the organization of sustenance activities is money, then value added to manufacturing is one measure of technological input to the sustenance organization. Moreover, value added is affected by the supply-demand curve for goods produced by the sustenance organization, and in this sense can be viewed as a measure of technological adaptability. The final technological factor considered is the central city commuting efficiency. This index was obtained by taking the total number of in-commuters for each city, subtracting the number of out-commuters, and dividing by the total of all commuters; the measure is synonymous to the measure of migration efficiency (Shryock, 1964; Galle and Williams, 1972). The measure is of particular value because a number of studies have documented relationships between commuting and migration (Goldstein and Mayer, 1964; Termote, 1975). Furthermore, it is a direct measure of not only the extent to which technology allows competition

from outside the central city but also of the extent to which the city population can compete in other environments.

Hypotheses

Applying these data to Sly's model generates seven hypotheses relating each of the independent variables to the rate of net migration:

- (1) The higher the index of industrial dispersion, the lower the net-migration rate.
- (2) The higher the unemployment index, the higher the net-migration rate.
- (3) The higher the commuting efficiency, the higher the net-migration rate.
- (4) The higher the percent value added to manufacturing, the lower the net-migration rate.
- (5) The higher the percent of central city jobs occupied by non-central city residents, the higher the net-migration rate.
- (6) The higher the percent nonwhite, the higher the net-migration rate.
- (7) The higher the density, the higher the net-migration rate.

If the model which views net migration as a direct response to organizational change and only an indirect response to technology and the environment is to gain support, we should find (1) that migration is more closely related to each of the organizational variables than each of the environmental and technological variables and (2) that controlling for the organizational variables will reduce markedly the associations between the environmental and technological factors and migration. The mean and standard deviation for each variable included in the analysis are presented in Table 1.

While the model does imply a number of other hypotheses, like Sly (1972:621-3) we will not deal with these except to note that "if the environmental and technological factors operate as predicted, we would expect them to be more closely associated with the organizational indicators than with migration" (Sly 1972:622).

Table 1. Means and Standard Deviations for the Dependent and Independent Variables, 1960

Variable	Mean	Standard Deviation
IND	.44	.10
WUR	5.01	1.67
CEF	.33	.31
MVA	59.53	22.09
CCN	11.17	10.12
CJO	41.39	23.38
DEN	6705.44	3747.98
WNM	26.39	10.61

IND =index of industrial dispersion for central cities, 1960

WUR =percent of white labor force unemployed in central cities, 1960

CEF =central city commuting efficiency, 1960

MVA =percent value added by manufacturing in central cities, 1960

CCN =percent nonwhites in central cities, 1960

CJO =percent jobs in central city, held by people living outside central cities

DEN =population per square mile in central cities, 1960

WNM =white net migration of central cities, 1960-1970

Data Analysis

Table 2 shows the zero-order correlation matrix for the variables. Other than the fact that each of the variables operates in the predicted direction with the rate of net migration, there is little support for the organizational model. For instance, each environmental and technological variable is more closely associated with the rate of net migration than are the organizational factors. Similarly, each of the environmental and technological factors should be more closely associated with the organizational factors than with migration; but, in general, this does not seem to be the case. In fact, of the ten comparisons which can be made to determine this, only one (that between value added and the migration rate and value added and the

index of industrial dispersion) shows a closer association between a technological factor and an organizational factor than a technological factor and migration (.403 versus .594).

While these observations tend to mitigate against acceptance of the organizational model, it is possible that when the interrelations between variables are taken into account those relations between environmental and technological factors and migration will reduce substantially below those of the organizational factors and migration. As a first step to testing this, we followed the procedures employed by Sly of examining the association between each of the environmental and each of the technological factors and migration, partialing for each of the organizational factors singularly and then in

Table 2. Zero-Order Correlation Matrix of the Independent and Dependent Variables

Variable	Organization		Technology		Environment		Population	
	IND	WUR	CEF	MVA	CCN	CJO	DEN	WNM
IND	1.000	-.079	-.027	.594 ^a	-.368 ^a	-.332 ^a	-.263 ^b	.289 ^b
WUR		1.000	-.051	-.063	-.055	.174 ^c	.234 ^b	-.200 ^c
CEF			1.000	-.033	.270 ^b	.633 ^a	.120	-.313 ^b
MVA				1.000	-.306 ^b	-.561 ^a	-.408 ^a	.403 ^a
CCN					1.000	.316 ^b	.307 ^b	-.678 ^a
CJO						1.000	.400 ^a	-.551 ^a
DEN							1.000	-.450 ^a
WNM								1.000

^a F ratio significant at the .001 level.

^b F ratio significant at the .01 level.

^c F ratio significant at the .05 level.

Table 3. Zero-Order Correlation between the Five Independent Variables and Net Migration Rates, and First- and Second-Order Partial Correlations Controlling for Index of Industrial Dispersion and White Unemployment Rate in Central Cities, 1960

Correlation Coefficient between Rate of Net Migration and:	Zero- Order	First- Order Partial	Second- Order Partial
CCN	-.678 ^a		
Controlling IND		-.618 ^a	
Controlling WUR		-.671 ^a	
Controlling IND & WUR			-.646 ^a
DEN	-.450 ^a		
Controlling IND		-.383 ^a	
Controlling WUR		-.394 ^a	
Controlling IND & WUR			-.355 ^a
CJO	-.551 ^a		
Controlling IND		-.472 ^a	
Controlling WUR		-.446 ^a	
Controlling IND & WUR			-.428 ^a
CEF	-.313 ^b		
Controlling IND		-.261 ^b	
Controlling WUR		-.278 ^b	
Controlling IND & WUR			-.276 ^b
MVA	.403 ^a		
Controlling IND		.280 ^b	
Controlling WUR		.361 ^a	
Controlling IND & WUR			.283 ^b

^a F ratio significant at the .001 level.^b F ratio significant at the .01 level.

combination. Examination of the second-order partials (Table 3) is all that is necessary to indicate that the organizational model has little viability in this context. For instance, although all of the correlations between environmental and technological factors and migration reduce, with the exception of value added, none of the partialled associations are reduced enough to decrease the probability of their acceptance. Again following Sly, we produced a path diagram as a final test of the model. Although not presented, the diagram offers little support for the model since the standardized partial regression coefficients linking the organizational factors to migration were small (-.100 and -.120, respectively), and since the values of all the corresponding coefficients linking environmental factors and migration exceeded those mentioned above by a substantial margin.²

² The path diagram was employed to allow for the simultaneous consideration of all variables and to specify how the variables are seen as operating. The cross-sectional nature of the data employed indicates that the time referent is inferred.

Thus, the data strongly suggest that the organizational model does not work within the context of more industrialized areas and is not nearly as generalizable as previously thought. Duncan (1961), however, has implied an alternative model which stresses the importance of environmental factors in more complex areas although he has not dealt specifically with the issues of demographic responses. In addition to this, a number of other researchers have emphasized environmental factors as being most salient in migration's contribution to population decline in cities (Jacobs, 1961; Butler, 1964; Walpert, 1966; Hawley, 1971; Deutschman, 1972; Morrison, 1974). According to the model implied by these studies, migration is a response to environmental conditions largely created by technological and organizational conditions within the city. This, in turn, implies that technology and organization do not directly affect migration, but that their impact on migration operates indirectly through their effect on the environmental conditions they create as illustrated in

Figure 1. The same seven hypotheses stated above can be used to test this model; however, we will now expect hypotheses one, two, three and four to reduce as controls are introduced.

Partial support for the model can be observed within the correlation matrix (Table 2). First, each of the environmental indicators is more closely associated with migration than are the organizational and technological factors. Second, each of the organizational and each of the technological factors correlate more highly with at least one of the environmental indicators than with migration; however, one of the relationships between an organizational indicator and an environmental indicator (white unemployment rate and central city jobs held by non-city residents) and one of the relationships between a technology indicator (commuting efficiency) and an environmental indicator (density) are not significant. This support for the model justified further analysis so we proceeded following the same strategy employed to test the organizational model.

Table 4 presents the partial correlation coefficients between each of the organizational and each of the technological factors and migration partialing for the environmental factors one at a time and then in combination. By the level of the third-order partials, considerable support for the model is attained. Three of the four possible relationships are not only reduced below the level of significance, but are nearly zero. The only association which does not reduce (and actually increases) is that between the unemploy-

ment rate for whites and migration. Thus, while the model is not totally supported, the data fit this model considerably better than they fit the organizational model and considerably better than Sly's (1972:624) original data fit the organizational model.

Although there is support for the environmental model at the level of the third-order partials, the relations between the first- and second-order partials relative to the third-order partials makes most salient Frisbie and Poston's (1975:782) point about the complexity of cause in ecological systems. For instance, if we examine those partial associations considering the relation between the white unemployment rate and migration, we can see that at the first level of control, two of the environmental indicators (the percent of city jobs held by non-city residents and density) reduce the primary association below the level of significance; yet the control for the percent nonwhite actually strengthens the association. At the second level of control, we can see that when the percent nonwhite is controlled with each of the other two environmental indicators the relationship is strengthened, yet the two operating together reduce the association between the white unemployment rate and migration to nearly zero. This would seem to imply that when all three environmental indicators are considered simultaneously, the association between white migration and the white unemployment rate would reduce substantially. However, this is not the case, and it is clear that the percent nonwhite is a more salient factor than the other two and that the white unemployment rate does have an independent effect on white migration.

Finally, following Sly (1972:626), we consider all of the variables in a path model (Duncan, 1966; Heise, 1969; Land, 1969). This diagram is useful for several purposes.³ First, it lends support to the

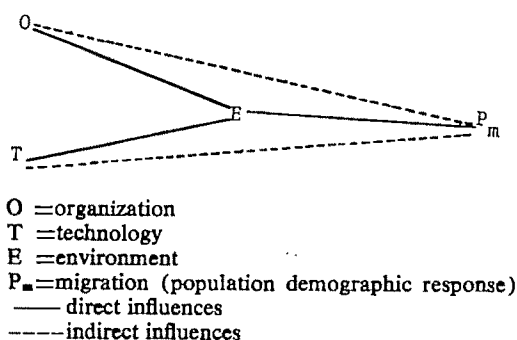


Figure 1. Effect of technology and organization on migration through environment.

³ While the path diagram presented below is suited to our purpose of examining the interrelations between variables, it does introduce the multiple regression consideration of multicollinearity. In this case, the problem does not appear to pose a substantial limitation on the analysis. First, five of the six intercorrelations between exogenous variables are extremely low; second, none of the standardized regression coefficients in the diagram appear to be

Table 4. Zero-Order Correlation between the Four Independent Variables and Net Migration Rates, and First-, Second- and Third-Order Partial Correlations Controlling for Percent Nonwhite, Density and Percent Central City Jobs Held by Workers Living outside Central Cities, 1960

Correlation Coefficient between Rate of Net Migration and:	Zero- Order	First- Order Partial	Second- Order Partial	Third- Order Partial
IND	.289 ^b			
Controlling CJO		.135		
Controlling CCN		.063		
Controlling DEN		.198 ^a		
Controlling CJO & CCN			-.065	
Controlling CJO & DEN			.095	
Controlling CCN & DEN			-.089	
Controlling CJO, CCN & DEN				-.004
WUR	-.200 ^a			
Controlling CJO		-.127		
Controlling CCN		-.319 ^a		
Controlling DEN		-.109		
Controlling CJO & CCN			-.258 ^b	
Controlling CJO & DEN			-.057	
Controlling CCN & DEN			-.251 ^b	
Controlling CJO, CCN & DEN				-.221 ^a
CEF	-.313 ^b			
Controlling CJO		.056		
Controlling CCN		-.184 ^a		
Controlling DEN		-.292 ^b		
Controlling CJO & CCN			.090	
Controlling CJO & DEN			-.005	
Controlling CCN & DEN			-.182 ^a	
Controlling CJO, CCN & DEN				-.010
MVA	-.403 ^a			
Controlling CJO		.136		
Controlling CCN		.280 ^b		
Controlling DEN		.270 ^b		
Controlling CJO & CCN			.044	
Controlling CJO & DEN			.069	
Controlling CCN & DEN			.182 ^a	
Controlling CJO, CCN & DEN				-.005

^a F ratio significant at the .001 level.

^b F ratio significant at .01 level.

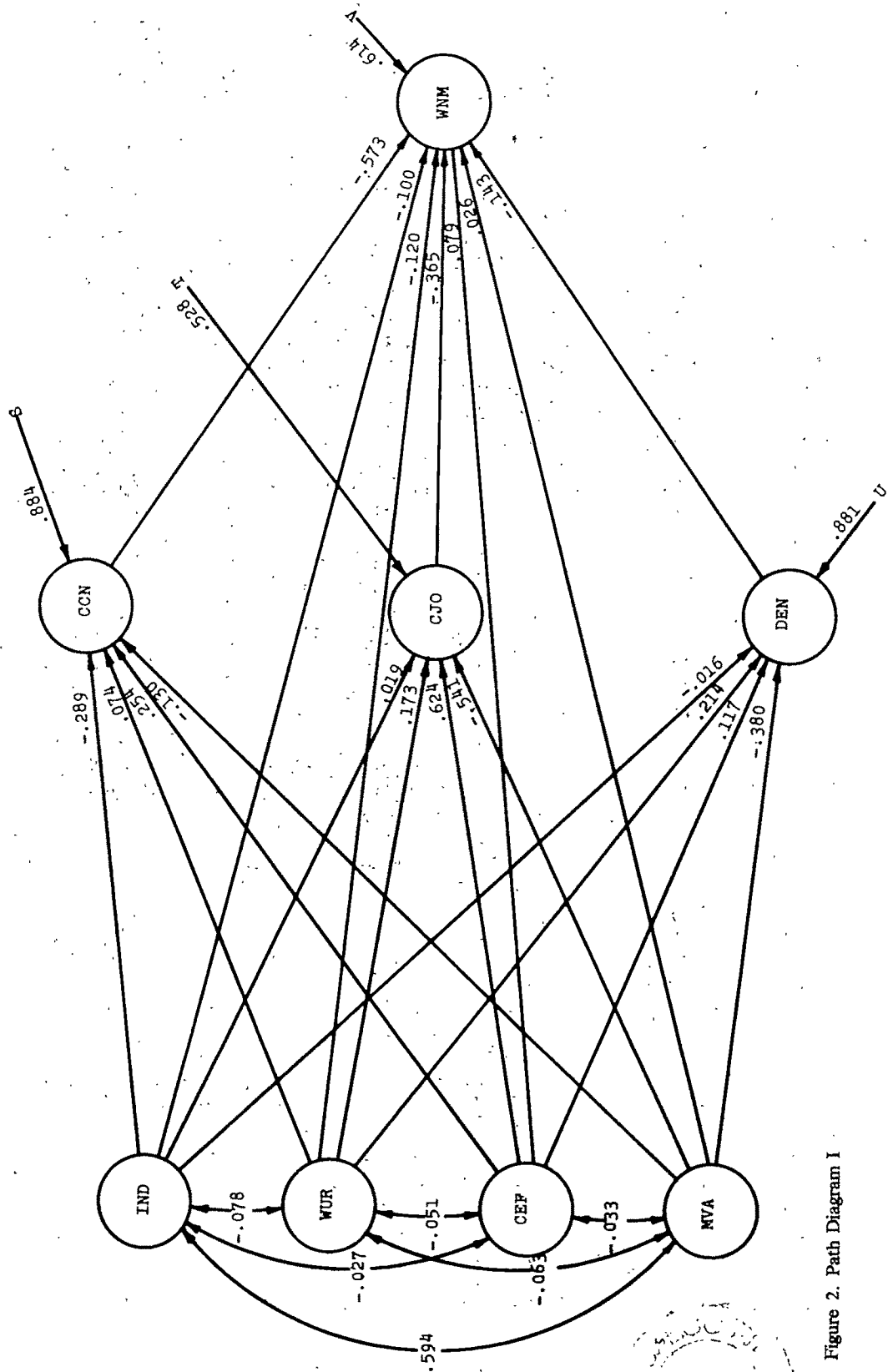
^c F ratio significant at .05 level.

environmental model. Nearly 40 percent of the variance in the dependent variable is explained, and each of the path coefficients linking an environmental indicator to migration is stronger than any of the paths linking the organizational and technological indicators to migration. Second, the model clearly illustrates that although all the dimensions of the environment effect migration, the magnitude of their impact varies markedly. The percent of the central city population which is nonwhite, for instance, has about

four times the impact on migration that density does. Similarly, density has less than one-half the impact on migration that the percent of central city jobs held by non-city residents does.

Third, and perhaps most important, is the total pattern of relationships. For instance, although the paths connecting each of the organizational and each of the technological variables to migration are weaker than the paths from the environmental indicators to migration, the paths from the organizational variables to migration are, in several cases, also stronger than the paths from the organizational indicators to the environmental ones. While this could be merely a func-

inconsistent; third, a comparison of the direct and indirect effects of each of the exogenous variables produced no inconsistent results.



tion of the limited number of organizational (and/or environmental) indicators employed (or the particular ones used), it does suggest the possibility that organizational factors do exert a greater independent influence on migration than is suggested in the schematic diagram of the model. Finally, the path diagram also suggests that technological influences on migration operate more through the environment than do organizational influences and that the influences of technological factors on the environment are far more generalized than are the influences of the organizational indicators on the environment. That is, with few exceptions, the values of the path coefficients linking the technology indicators to the environmental indicators are stronger than the corresponding paths from the organizational indicators to the environmental ones.

Discussion

The data presented and analyzed here indicate that population demographic responses, and migration in particular, are not always a direct result of organizational conditions as suggested by the organizational model of migration. The data do indicate that within the context of these modern industrial cities environmental factors are considerably more important than sustenance organization in determining whether a population will increase or decrease as a result of migration. Yet, while the data highlight the importance of environmental conditions in determining the population migration of cities, they also suggest that organizational factors make a contribution to migration which is independent of their indirect effect through the environment. In short, the data highlight the complexity of ecological systems and the importance of the point of analytical departure.

Thus, although it is tempting to reject the organizational model and assert the supremacy of the environmental model, the differences in the nature of the areas studied by Sly (1972) and those analyzed here could indicate the compatibility of the two models and suggest that demographic

responses are elicited more directly by different sets of factors at different levels of areal industrialization. While this proposition cannot be tested with the data at hand, and while the samples are not strictly comparable, it is insightful to draw some comparisons between the path models used by Sly (1972:626) and the one presented here. For instance, he analyzes the migration experience of areas at two different points in time (1940-1950 and 1950-1960) during which their industrial development surely changed. During the earlier period, his environmental indicators had little impact on migration while his organizational indicators did; by the 1950-1960 period, both of the environmental indicators exerted a relatively strong influence on migration. The coefficients from both environmental indicators exceeded those from one of the organizational indicators and the strongest influence on migration was from one of the environmental indicators.

When these observations are coupled with the fact that the organizational indicators we employed influenced migration, but not nearly to the extent that the environmental indicators did, the suggested changing importance of rubrics in determining population change with industrialization seems to gain greater viability.

Similarly, although our areas and populations are not strictly comparable with those studied by Frisbie and Poston (1975), their work demonstrated the importance of organizational factors in determining nonmetropolitan population change. The fact that they explained only about 25 percent of the variance with their organizational factors, however, could suggest that the impact of these were mediated through environmental conditions. Thus, it seems possible that at some point in development population change may become more responsive to environmental than to organizational conditions.

One problem for future research may be to attempt to identify this point and/or to develop further evidence demonstrating the changing importance of the rubrics. Some evidence has been presented to

indicate that the point of the change in the importance of the rubrics may be determined by the influence which technological factors have on the environment, but this inference is based solely on the stronger and more pervasive impact that our technological indicators had on the environmental indicators. For instance, the direct effects of the two technology indicators are negligible (the largest is only .079); yet the total indirect effects of each is substantial with commuting efficiency's indirect effect accounting for about 15 percent of the variance in the migration estimates and value added to manufacturing about 11 percent.⁴ In short, at some point in development, technological factors appear to begin to operate more on the environment than on organization; and when this happens, migration begins to respond more directly to the former than the latter.

Finally, this study, along with those of Sly and Frisbie and Poston, indicates the complex nature of ecological change and the important role which the ecological approach can play in helping to develop a better understanding of population change. Moreover, the analysis presented here helps to demonstrate that the ecological approach is potentially more than a simple heuristic device for ordering and presenting variables. What is suggested, however, is that greater conceptual clarity needs to be developed in order to specify not only the relations between rubrics, but also how factors within rubrics are related. This theoretical development could follow the procedures reviewed by Gibbs and Poston (1975). In this sense, it would be beneficial to specify clearly the structural and processual dimensions of each rubric and then begin to specify (1) the internal operations of rubrics as well as (2) the structures and processes which link rubrics. This type of work would help to explain how and why populations respond

to different sets of factors at different levels of development and it could suggest how and why different demographic responses (migration, fertility, etc.) are elicited.

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⁴ The direct effects can be observed in the path diagram on the lines connecting central city commuting efficiency and value added to manufacturing with the estimates of net migration. The estimates of variance explained by indirect effects were calculated from estimates of the indirect effects using the technique suggested by Finney (1972).

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THE EXPORT OF RAW MATERIALS AND ECONOMIC GROWTH: A CROSS-NATIONAL STUDY*

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World-system and dependency theories suggest that the specialization of some countries in the export of raw materials and lightly processed goods is an important cause of their underdevelopment. The rare empirical evidence in support of this hypothesis is inconclusive because the variables and the designs used are defective. A diachronic multiple regression test using a new version of the theoretical independent variable also fails to provide support for the hypothesis. The test suggests that internal processes play a role that should not be ignored in the economic growth of nation-states.

The study of economic development is a natural meeting ground for social scientists of the several disciplines. Yet, few practitioners of one social science venture far into the preserves of sister disciplines. Sociologists sometimes give cursory attention to economic variables but rarely include them in the models they test. One economic variable of sociological interest is a country's propensity to participate in international exchange markets as a supplier of raw materials. First, this propensity denotes the occupancy of a particular position in a "world-system" (see Wallerstein, 1974a) made up of nation-states. Second, the ability to export goods that are more or less processed depends highly on a country's own internal division of labor and its degree of organization. The latter characteristics, in turn, constitute

an important aspect of the social differentiation of the country or nation-state.

The specialization of countries in the export of raw materials or unprocessed goods has been of interest to theorists of economic development for a long time. For the present purpose, it will suffice to trace back to Lenin the importance attributed to this variable in economic processes. He called the "capture of the most important sources of raw materials" one of the "four principal manifestations of monopoly capitalism" (Lenin, 1917:123 in Selsam et al., 1970:315). Most theorists treat specialization in supplying of raw materials as a position in a world division of labor. The conceptualization nearly always implies a notion of unequal exchange (Prebisch, 1950; Emmanuel, 1972; Baran, 1956; Magdoff, 1969; Frank, 1971; Galting, 1971; Benot, 1973; Amin, 1973; Wallerstein, 1974a). That is, an inherent inequality between the exchange partners is thought to operate more or less consistently to the disadvantage of the raw materials exporters. This perspective suggests the plunder of poor countries by a handful of rich industrialized nations, the exploitation of the many by the few (Lenin, 1917:124). In other words, it pos-

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its surplus value extraction by a macro-class that must set the stage for a new, transnational version of the class struggle. The new version of the class struggle, in turn, heralds the future demise of capitalism in its latest worldwide expression (see, in particular, Wallerstein, 1973; 1974b). This particular version of the relationship between national export specialization and international power relations was already present, though in embryonic form, in Lenin (1917:126) and, possibly, also in Marx (1853:663) and in Selsam et al. (1970:315). It has sociological implications far beyond the more immediate matter of economic growth to which we now turn.

In the past few years, a large number of social scientists and other writers have affirmed or used the idea that specialization in the export of raw materials has adverse consequences for the national economy of the exporter with specific reference to areas of the third world (notably Frank, 1967; O'Connor, 1971; Galeano, 1971, with references to Latin America; N'Krumah, 1967; Berman, 1974 with respect to Africa). Some theorists who are not specifically Marxist have adopted the idea (see, e.g., Heilbroner, 1972:219).

Scholars differ in the degree to which they attribute the specialization of the poor countries in the export of raw materials to the deliberate active intervention of the rich countries. Marxist writers have had a tendency to infer such intervention from a demonstration that it is in the interest of the rich (capitalist) countries to so intervene (Jalee, 1965; Magdoff, 1969; Dean, 1971). Frank (1967) writes of the "development of underdevelopment." Recently, some theorists have focused on the demonstration of a "gatekeeper effect" which involves a rather less conspiratorial view of the rich countries' intervention in the economic affairs of the poor ones. The gatekeeper effect can be summarized as follows. Developed countries maintain underdeveloped countries in their position of raw materials suppliers by controlling the latter's access to the outside world. The developed countries control the underdeveloped countries' access to the capital, the technology and other resources necessary for industri-

alization. This forced inability to industrialize, in turn, perpetuates the relationship by preventing the poor countries from becoming either self-sufficient or exporters of processed goods. The gatekeeper role is assigned variously to the indigenous bourgeoisie (Berman, 1974) directed by foreign capitalist interests and more or less allied to the national military elites (Bourricaud, 1966; Galtung, 1971:85) to transnational corporations acting directly (Petras, 1968; O'Connor, 1970; Seidman, 1970), to international organizations, principally banks (O'Connor, 1970; Payer, 1971) or to a mixture of all of these (Levin, 1966). A few writers have formulated this effect in purely structural terms (i.e., with no implications regarding the gatekeepers' volition—Berman, 1974; Baldwin, 1966). A very small number imply that the managerial incompetence of the government of the countries concerned perpetuates specialization in the export of raw materials or the ensuing adverse economic consequences (Leff, 1968; Tugwell, 1974).

Several processes can be identified as underlying the assumed negative effect of specialization in the export of raw materials on economic health. Empirical evidence in support of the presumed relationship therefore can be brought to bear on the underlying processes or on the relationship itself. In this paper, I summarize the most important underlying processes and review the previous empirical research. I then present and discuss findings of a direct test of the relationship which uses different measures and more up-to-date methods than did the previous research.

Processes

Theoretical explanations of the presumed relationship between specialization in the export of raw materials and national economic welfare pertain to one or a combination of the following processes.

(a) The gradual institution of the European-centered world division of labor in the sixteenth century is elaborately described by Wallerstein. The capitalist world-system rests on more (and more complex) processes than that resulting in

the specialization of areas into exporters of raw and transformed goods. Nevertheless, Wallerstein (1974a:86-126) devotes the better part of one chapter to tracing the intricate linkages between types of commodities produced, systems of labor control, strength of the state apparatus, availability of urban laborers (117-8), the success of indigenous bourgeoisies and the establishment of core capitalism. Wallerstein also describes the varying ability of both peasants and urban workers in the European core areas to share in the ensuing windfall. Hence, *the assignment of what prefigured the present "third world" to the task of providing raw materials (bullion, then food, then materials for industrial transformation) in the sixteenth century is a prime (though not perhaps a primary) cause of the contemporary world inequality*. That this relationship is historically drawn out and causally complex should not obscure its existence, earlier described in more structural terms by Galtung (1971).

(b) *The international demand for raw materials is fundamentally and increasingly inelastic*. The industrialized countries who are the principal buyers of raw material experience very moderate population growth. At the same time, their capacity to perform more and more processing operations on the same quantity of raw material is increasing constantly (Magdoff, 1969). Improvements in the productivity of raw material exporters resulting in increased supply confront a nearly fixed demand. This must result, for the raw material exporters, in lower per-unit prices rather than in growth of income (Furtado, 1965; Gomez, 1966; Maizel, 1968; Galeano, 1971:218).

(c) Logically linked to the last explanation but different in its consequences is the observation that *raw materials prices tend to undergo wide fluctuations*. Such fluctuations tend to make rational economic planning difficult or impossible for the private entrepreneur and the government planner. This state of affairs is aggravated by the fact that the external trade flows of underdeveloped countries implicate a larger portion of their GNPs than is typical for developed countries. This explanation of the unfavorable conse-

quences of raw material export specialization is broadly and critically discussed by Glezakos (1973). (d) Fourth, to the extent that the production of raw materials is financed by nonindigenous interests who repatriate their excessive profits (Gomez, 1966), *the exports of raw materials may result in actual net outflows of cash* (Berman, 1974:6). Finally, Singer (1950), Young (1970), Galtung (1971) and others speculate that *specialization in the export of raw materials results in allocation of national resources that are less than optimal from the standpoint of the exporter's economic growth*. In particular, they allege that such specialization impedes investments in economic and social sectors most likely to have multiplier effects on the exporter's economy.

The net overall thrust of the collection of arguments summarized above is to deny the validity of one of the major tenets of classical macroeconomic theory. According to the Law of Comparative Advantage (Samuelson, 1970:645-67), each actor in the world division of labor must concentrate on what it *does best*, whether or not any other actor does it better. Classical economic theory states categorically that this strategy is in the interest of all the actors.

By contrast, a large and growing body of theory astride several disciplines asserts that a country's specialization in the export of raw materials or unprocessed goods adversely affects its economic well-being. In particular, this body of theory views such specialization as a major obstacle to economic growth and development.

Previous Research

In spite of its high degree of acceptance in the literature, the idea that specialization in raw materials export has adverse effects on economic growth and development is supported by little empirical evidence. What little research on the matter does exist is methodologically unsatisfactory or fails to support the hypothesis.

Some researchers have investigated the link between specialization in raw materials export and economic growth from the viewpoint of price instability. Naya (1973)

finds no statistically significant relationship between the percentage of export in raw materials and the instability of the income of a country derived from exports. His study, using regression analysis, is based on a cross section of seventeen Asian countries presumed to have experienced greater than average export income instability. Glezakos (1973), also using cross-sectional regression analysis, fails to show a significant relationship between the export price instability of forty countries and their growth rates. This result agrees with earlier research by Coppock (1962) and McBean (1966).

Quantitative tests lending support to the hypothesis are scarce. Jalee (1965) and Rollins (1956) construct descriptive tables showing that countries that specialize in the export of raw materials are not rich. Galtung (1971) provides positive support for the hypothesis with cross-sectional bivariate correlations based on sixty countries. His dependent variables are: (a) Gross National Product per capita and (b) percentage of the population employed in nonprimary sectors of the economy. Galtung's measure of export specialization, the Trade Composition Index, expresses as a ratio the balance between the relative degree of processing of the imports and exports of a country. The Trade Composition Index has two defects as a measure of export specialization: (a) as is the case with many compound ratio variables, there are theoretically contradictory ways to get the same score on this measure; (b) Galtung (1971:102) himself signals the second defect: "There is no doubt that this index is a crude measure among other reasons because the variable *degree of processing*, so crucial to the whole analysis, has been dichotomized in 'raw materials' vs. 'processed goods' and because the basis for the dichotomization is the division made use of in U.N. trade statistics." In other words, the dichotomization of products into raw and processed betrays the actual continuity of the theoretical variable *degree of processing*. Any item that reaches a market is the product of human labor on Nature (Galtung, 1971:86) and an investment of human culture into a natural object. Hence, a measure of degree of processing

should admit of an infinity of values except zero. Ideally, each individual item should have its own score denoting this labor and this cultural investment. This is difficult to achieve because of obvious limitations on the availability of data. However, it is possible to minimize the costs of categorizing by using finer categories than Galtung's dichotomy. Finer categories tend to make the observed effect of the measure on other variables less dependent on the exact location of each cutting point between categories.

Galtung expresses reservations about his own use of the U.N. Trade Statistics as a source of data for his index. However, the Standard International Trade Classification used by the U.N. is not fatally inappropriate for this purpose if one makes more use of its features than Galtung does especially if one uses it more critically.

Quite aside from the weakness of the Trade Composition Index, Galtung's test of the hypothesis is inconclusive because of its design. The positive correlation he reports between Trade Composition Index and *percentage of the labor force in non-primary sectors* ($r=.77$) is partly tautological. If, for example, a large percentage of a country's labor force is employed in extractive industries, one would expect that it will be reflected to some degree in the composition of its export which will include some quantities of raw materials. Therefore, percent labor force in non-primary sectors and Trade Composition Index may be, to some extent, two measures of the same internal development variable.

Galtung also reports a high positive correlation between Trade Composition Index and Gross National Product per capita ($r=.89$). Both measures are for 1967-1968. Hence, the test is synchronic rather than diachronic. Such a test leaves open the matter of causation, at least from a narrow, empirical-quantitative viewpoint. Galtung provides rich theoretical argument in support of an interpretation that makes trade composition (or the degree of processing of export) the cause and GNP per capita the consequence. However, it is desirable to test the hy-

pothesis diachronically as well, as he points out (Galtung, 1971:103). Furthermore, since Galtung's test depends on bivariate correlations, the matter of causality cannot be resolved through interpretation of the interrelations of other variables with Trade Composition and with the dependent variables. In particular, it would be useful to test the hypothesis with a design that would include variables chosen to contribute to the elimination of three alternative interpretations of Galtung's findings: (a) National specialization in the export of goods at a low level of processing is a by-product of poverty; it has no direct effect on it. (b) Countries with a large population enjoy a comparative advantage in specializing in the production and export of labor-intensive, low-processed goods. Such countries are primevally poor. (c) While the link between specialization in the export of low-processed goods and poverty is real, such specialization and poverty result from processes *internal* to the country rather than from the operation of the world-system.

I report below on a diachronic test of the hypothesis based on a measure of degree of processing of export that minimizes the difficulties involved with Galtung's Trade Composition Index. In the model tested I include variables designed to facilitate choice between the original hypothesis of dependency and world-system theories and third interpretation above.

Methods

Both substantive and methodological considerations make it advisable to test the hypothesis through a *panel design*. Substantively, it is clear from the literature that the effect of specialization in the export of raw materials on economic variables is viewed as structural and cumulative. Therefore, it must take time; no *instantaneous causation* is considered. The adoption of a panel model is also desirable from a methodological standpoint because it helps solve the problem of *reciprocal causation*. In the real world, things show a tendency to be their own cause. This is particularly true of wealth. It is important

to guard against the possibility that one will attribute to a variable of theoretical interest an effect that is caused by itself over time and that is itself a cause of the theoretical variable. Hence, economic underdevelopment may perpetuate itself *and* force a country into specialization as an exporter of raw material. Such a specialization, in turn, may have *no* independent economic consequences for the underdeveloped country. Cross-sectional designs fail to distinguish between such a situation and one where the specialization causes underdevelopment. With a panel design, by contrast, a measure of the dependent variable at the early point in time can be included in the model. This allows the dependent variable to cause itself over time and apportions the remaining effect among other independent variables included in the model. Therefore, all the equations referred to in this paper are based on a panel design and include a measure of the dependent variable at the early point in time (called the "lagged dependent variable" [see Chase-Dunn, 1975]):

Measures

The literature mentions a variety of ways in which specialization in the export of raw materials impedes economic development. Finsterbusch (1973) has shown that Gross National Product per capita is highly correlated with a multiple indicator measure of the somewhat diffuse concept of development. In the first equation presented, I use *GNP per capita* 1970 as a dependent variable. However, there are well-known difficulties of interpretation associated with ratio variables (Kuh and Meyer, 1955; Briggs, 1962; Schuessler, 1974). Therefore, in a second equation, where (raw) GNP 1970 is the dependent variable, I use a logarithmic transformation of this variable (base 10) because the error variance of this measure may be correlated with other variables in the equation. This second equation also includes a measure of population size in 1955 in order to make the dependent variable in this equation, (raw) GNP, comparable to the dependent variable in the first equation, GNP per capita. It can be shown

that, in a regression with panel design, introducing a measure of population size taken at the *same time* as the dependent variable is nearly equivalent to standardizing the dependent variable by population size. On the other hand, population size measured at the late point in time (1970) of my diachronic model and the same measured at the early point (1955) are highly correlated ($r=.96$). Hence, the inclusion in the model of *population size* measured in 1955 allows one to interpret the dependent variable (raw) Gross National Product as a measure of social welfare, functionally equivalent to GNP per capita. It should be noted that the inclusion of this population measure does *not* in any way make equation (2) an adequate test of the effect of population size on economic growth. I attach no substantive meaning to the estimate associated with this measure because of the extreme complexity of the matter (see Easterlin, 1967).

The data for Gross National Product, 1970 and 1955, come from the International Bank for Reconstruction and Development's 1971 *World Tables* and from its *World Atlas* for 1970.¹ The data for the measure of population size in 1955 were obtained from the Inter-University Consortium for Political Research *World Book of Political and Social Indicators II, Section I* 1971.

Measuring the variable of theoretical interest, degree of processing of export is

¹ *Coding of Gross National Product (GNP)*—Millions of U.S. dollars. This variable was coded from the International Bank for Reconstruction and Development 1971 *World Tables* (Table 4) for 1955. For the year 1970, I used the World Bank's *World Atlas*. There are some minor differences between the two sources' methods for computing countries' GNP. For the years 1950 to 1965, the *World Tables* present the GNP at factor cost expressed in constant 1964 dollars. The *World Atlas* provides figures for 1970 GNP at market prices, using a multi-year period for a base. GNP at market prices includes indirect taxes and is consequently approximately 20% higher, on the average, than GNP at factor cost for the same year. The differences seem, however, to affect all countries about equally. Using a multi-year period for a base for the U.S. dollar reduces temporary distortions. Neither of these differences between the 1970 data and data for previous years is likely to affect findings at the aggregate level. However, one should exercise caution when taking the figures literally for any given country.

a theoretical problem in its own right. Galtung (1971:86) formulates the hypothesis in these terms: the higher the level of processing of the total export of a country, the greater its economic growth. Boulding (1970:20-2), in his discussion of "social species," likens the "direct organization of human ideas and knowledge" which goes into the making of an automobile (an act of processing) to the transmission of genetic information. Likewise, in a previous critique of the labor theory of value, Boulding (1953:164-6) had expressed the idea that raw materials are transformed into products through the injection of social organization in the form of information. Stinchcombe (1968:281-2) remarks that the processing of information generates structural growth. At the same time, he makes it clear that the information content of processed goods (e.g., general cargo) is higher than that of raw materials (e.g., "bulk goods"). Hence, the degree of processing of a marketable product can be expressed in terms of the amount of (social) information invested in it. In this sense, any product is a vehicle for a certain amount of socially organized information.

To measure the degree of processing of a country's export is to assign to the commodities which comprise that export weights that express the amount of social information invested in them. This operation involves two distinct steps. (a) The first is aggregation of commodities. Theoretically, each individual item contains a different amount of information than any other item. The problem here is to make use of the information available to regroup items in a way that comes satisfactorily close to respecting the underlying continuity of the processing dimension (i.e., to improve on Galtung's simple dichotomization). (b) The second step is devising a weighting system expressing the amount of social information invested in the commodities aggregated in step (a) above.

Step (a) of my coding procedure consists in aggregating the breakdowns of countries' exports provided by the *U.N. Yearbook of International Trade Statistics* at the SITC (revised) two-digit "division"

level. The two-digit level is the finest one at which enough countries report the composition of their export. Choosing an even finer level would have caused the loss of cases below the absolute minimum number necessary for regression analysis with significance tests. At the same time, little improvement in precision would have been gained by adopting a finer level of aggregation. The SITC two-digit divisional level classification contains an embryonic scale of degree of processing in crude form. For example, it places: "Textile fibers (*not* manufactured into yarn, thread or fabrics) and their waste" in division 26, while division 65 contains "Textile yarn, fabrics, made up articles and related products." Likewise, the SITC two-digit level distinguishes between division 28: "Metalliferous ores and metal scrap," on the one hand, and division 67: "Iron and Steel" and division 68: "Non-ferrous metals" on the other. However, this classification does not array products according to their degree of processing with enough consistency that we may use it as a *de facto* weighting scheme. Instead, I assign to each division a weight from a scale ranging from 1 (for cattle) to 9 (for transportation equipment). The scale itself is based on a scheme whose basic conceptual unit is the *minimal transformation to which an object must be subjected in order to become a commodity* (i.e., marketable). The scheme takes into consideration comparison between transformations, distance between SITC divisions in terms of number of transformations, additivity of transformations (e.g., transport equipment contains refined metals "transformed" from ore) and the homogeneity of the contents of each division. The measure of degree of processing is computed by multiplying the value of the content of each SITC division expressed as a percentage of total by the weight assigned to that division and summing across. The scores on this measure, called *level of transformation of export*, range, in 1955, from 574 (West Germany) to 128 (Bolivia). Little information seems to have been lost by ignoring imports, since level of transformation of export for 1955 has a rank order correlation of .85

with Galtung's 1967-1968 Trade Composition Index. I use *level of transformation of export*, 1955, as my independent variable of main theoretical interest.²

I use level of transformation of export to try to ascertain the *direct* effect of specialization in the export of raw materials on economic growth for the 1955-1970 period. The problem here is to estimate the influence of a country's position in the world-economy, as denoted by this measure, *over and above* the possible effects of processes internal to the country.

World-system and dependency theories posit that processes internal to each nation-state are consequences of location in the world-system, or they treat such processes as causally secondary. Alternative interpretations of Galtung's findings, (a) and (c) above, depend on the realism of the assumption that internal factors are secondary. Hence, a more complete test of the hypothesis than the one offered by Galtung would incorporate some control for the possible effects of *internal* factors on economic growth.

Following Galtung's general reasoning, the most relevant internal factor here seems to be a country's initial information processing capacity. Galtung argues strongly that raw materials specialization hinders the economic development of poor countries *primarily* by denying them the beneficial "intra-actor effects" that accrue to their industrialized trade partners. In particular, Galtung (1971:87) speculates that raw materials specialization places countries at a relative disadvantage with respect to the expansion of their educational systems. Most of the other intra-actor effects discussed by Galtung can be subsumed under the category of internal differentiation corresponding

² A major conceptual defect of this measure is that it counts only material products. According to the logic of considering processing as an investment of information into objects, the export of pure information—in the form of services—also should be counted. However, it is difficult to combine such exports (for which data are, at any rate, spotty) with commodities. I think that the distribution of scores would not have been much altered by the adjunction of this dimension. I am grateful to the anonymous reviewer who drew my attention to this point with which I fully agree.

to a given capacity for processing information.

Galtung's implicit concept of information processing capacity rejoins the conceptualizations of Duncan (1964:39-41), Boulding (1966), Stinchcombe (1968), Delacroix (1974) and Lenski (1976:556, 558). Lenski and Lenski (1974), for example, make the existing store of generalized information available to a group and the group's changing ability to increase and utilize it the cornerstone of their theory of social change.

Galtung (1971:87) mentions education as one of the concrete manifestations of information processing capacity whose expansion is adversely affected by raw materials specialization. His assumption that educational expansion contributes to the economic development of countries is supported by arguments or findings by Solow (1957), Schultz (1963), Denison (1962), Meyer et al. (1973), Harbinson (1973), Hannan (1974) and, indirectly, by Inkeles and Smith (1974).³ Accordingly, I make *secondary-school enrollment* in 1955 stand for general information processing capacity. I choose secondary-rather than primary-school enrollment because, if this variable has an independent effect on economic growth, it is more likely to be discernible over the time period considered (15 years) than would be the case with primary enrollment. In addition, secondary-school enrollment has lower correlations with population size than does primary enrollment and lower correlation with GNP than does university enrollment. In equation (1) where the dependent variable is GNP per capita, the measure of secondary-school enrollment is expressed as a ratio of the secondary-school-age population. This measure is taken from the 1971 edition of the *UNESCO Yearbook*. In equation (2), which includes a measure of population size and where the dependent variable is (raw) GNP, secondary-school enrollment in 1955 is entered without a control for the appropriate age population. The reason for this omission is that population size

measured in 1955 has a correlation of .99 with the measure of secondary-school age group measured at the same time. Hence, the measure of population size serves the dual purpose of simulating GNP per capita growth (see above) and of controlling for secondary age population.

Finally, it could be argued that equations (1) and (2) constitute an inadequate test of the hypothesis because of the shortness of the lag on which they are based (1955-1970). Wallerstein envisages a process unfolding over several centuries. Galtung seems to hypothesize a slow incrementation of negative intra-actor effects whose aggregate consequence is the purported link between specialization in the export of raw materials and a slow economic growth. Given the existing data limitations, not much can be done to extend the lag between independent and dependent variables measurement. However, it is possible to gain an idea of the *indirect* effect of raw materials specialization on economic growth. There seems to be a consensus around the idea that educational expansion contributes to economic growth. If raw materials specialization has a negative effect on educational expansion, then it must also *indirectly* hinder economic growth though no direct relationship appears for the lag covered. Ideally, one would estimate the effect of the level of transformation of export measured at an earlier point on secondary-school enrollment in 1955. Data limitations make this unfeasible. The next best solution is to make use of what data are available by estimating this relationship for the 1955-1970 period. The results of this operation are presented in equation (3) which is of the same general form as equation (2).

The cases over which the equations presented and mentioned in this paper are computed vary according to the availability of data. Slightly more than half the countries included in each set would have been considered underdeveloped in 1955. All sets exclude the U.S. Close inspection of the residuals associated with each equation shows that while equations (1) and (2) give very similar predictions, equation (2) gives a better prediction for poor countries.

³ While these authors undoubtedly would not agree on the exact nature of the causal path between education and economic growth, they all present evidence in support of the existence of such a link.

Table 1. Pearson Correlations*

	1	2	3	4	5	6	7	8	9
1. Log GNP 1970									
2. Log GNP 1955	.97								
3. GNP/capita 1970	.54	.49							
4. GNP/capita 1955	.47	.47	.94						
5. Level of transformation of export 1955	.70	.65	.61	.49					
6. Secondary enrollment 1970	.71	.65	.82	.74	.68				
7. Second enrollment 1955	.65	.59	.85	.79	.72	.88			
8. Secondary enrollment 1970 as a ratio of secondary age population	.73	.70	.24	.08	.55	.43	.47		
9. Secondary enrollment 1955 as a ratio of secondary age population	.58	.56	.23	.08	.57	.44	.48	.92	
10. Population size 1955	.41	.45	-.12	-.14	.18	.24	-.04	.87	.50

* Pairwise deletion, for all cases for which data were available on level of transformation of export 1955 (N=59, maximum).

Findings and Discussion

The findings directly relevant to the test of the hypothesis are easy to summarize. For the 15-year period covered, initial wealth is the main factor in economic growth as evidenced by the high autoregression terms in equations (1) and (2) ($\beta = .75$ and $.90$, respectively). The estimates of the effect of level of transformation of export on economic growth do not come close to statistical significance in either equation (1) or (2). This lack of significance is not an artifactual consequence of the high autoregression terms in these equations since, in both equations, secondary-school enrollment has a significant positive effect ($\beta = .17$, very close to the $.05$ level in equation [1]; $\beta = .10$, above the $.05$ level in equation [2]).

Equation (3) is a less than perfect test of the plausibility of an indirect linkage between raw materials export specialization and economic growth via information processing capacity. If level of transformation of export affected secondary-school enrollment negatively, it also would indirectly affect economic growth negatively since secondary enrollment has a positive influence on the latter. In equation (3), the beta estimating the effect of level of transformation of export on secondary-school enrollment is negative but very small ($-.01$) and nowhere close to statistical significance.⁴

With all their limitations, these findings do not add up to a confirmation of the widespread belief that national specialization in the role of raw materials supplier to the world-economy is a primary cause of underdevelopment. Rather, these new findings lend some support to interpreta-

⁴ The findings attached to level of transformation of export tend to be very robust. They are not sensitive to small variations in the cases or to time lags over which the equations are computed. Furthermore, controlling for secondary-school-age population in equations (2) and (3), does not change the overall pattern of findings in spite of the multicollinearity this procedure creates. Logging the GNP measures in equation (3) or substituting GNP/capita and secondary enrollment as a ratio of the appropriate age group does not make the coefficients associated with level of transformation of export significant. Finally, logging all the variables to make equations (2) and (3) fully interactive does not alter the conclusions reached.

Table 2. Effects of Degree of Processing of Export on Economic Growth and Secondary-School Enrollment ^a

Equation 1	GNP/Cap 1955	Second Enrollment as a Ratio 1955	Level of Transformation of Export 1955
GNP/Cap 1970	.75	.17	.10
N=49 ^b	(0)	(.06)	(.11)
Rsq=.90	2.05	1070.57	.001
Constant=-.2515			
Equation 2	Log GNP 1955	Secondary Enrollment 1955	Level of Transformation of Export 1955
Log GNP 1970	.90	.10	.06
N=48	(0)	(.04)	(.23)
Rsq=.95	.90	.5 x 10 ⁻⁴	.4 x 10 ⁻³
Constant=1.25			
Equation 3	GNP 1955	Secondary Enrollment 1955	Level of Transformation of Export 1955
Secondary enrollment 1970	.09	.57	-.01
N=47	(.12)	(.00)	(.85)
Rsq=.95	.12 x 10 ⁻⁴	.7 x 10 ⁻⁴	-161.60
Constant=123110.35			

^a In each equation, the first row shows the standardized (beta) coefficients, the second row shows the corresponding significance levels and the third row the unstandardized (b) coefficients.

^b N in equations (2) and (3) includes cases not in equation (1) and vice versa.

tion (c) above of Galtung's previous findings: while there is a positive association between raw materials specialization and initial poverty (evidenced by correlations of .51 and .62, respectively, between GNP/capita 55 and GNP 55, on the one hand, and level of transformation of export in 1955, on the other), the former seems to have little consequence over the short run.

In fact, these findings inadequately express a situation vastly more complex than suggested by the original hypothesis. Strict space limitations prevent me from exploring this adequately but four main points should be kept in mind:

(1) In the short run, wealth leads to wealth. It is difficult to show the effect of anything on national wealth. However, equations (1) and (2) show that *something* does affect national economic growth positively, even in the short run. This something, represented by secondary enrollment, is an internal process rather than an external one as predicted by world-system theory.

(2) The exceptions to the rule that wealth leads to wealth further contradict world-system theory. Equation (2) which gives the best prediction for Turkey, *underpredicts* the economic growth of Libya while it *overpredicts* that of Britain. Libya became *more specialized* in the export of one raw material between 1955 and 1970. Britain's highly processed export in 1955 did not prevent its subsequent relative downfall.

(3) The implicit picture (made explicit by Galtung) of a world neatly divided between poor raw materials exporters and rich processed goods exporters is substantially incorrect. Chile, usually thought of as an underdeveloped country, exports mostly *refined* copper products (70% in 1955) and very little ore (6%). On the other hand, everyone knows that the U.S. and Canada are big exporters of raw agricultural products. Even greater reliance on raw commodities on the part of rich countries is not unexceptional. Iceland tripled its GNP between 1955 and 1970. Yet, in 1955, Iceland's export consisted of very lightly processed or unprocessed fish to the tune of 85%. Argentina, whose agricultural export was only 60% of total

(foodstuff), only doubled its GNP during the same period.

(4) There is no compelling *empirical* reason for ignoring the influence of internal processes on economic development or for treating these processes as mere consequences of world-system position. Chase-Dunn (1975) and Robinson (1976) have recently shown some associations between measures of dependency and national income distribution. Nevertheless, some internal *institutional* factors of economic growth enjoy a degree of freedom from the constraints imposed by initial poverty and by world-system position. Thus, in 1955, the Pearson correlations between secondary enrollment expressed as a ratio of the appropriate population on the one hand and GNP per capita and level of transformation of export on the other were .79 and .71, respectively (N=49). While these associations are not negligible, they indicate that these variables had only about half of their variance in common. This suggests a freedom to maneuver not often acknowledged by exogenetic theories of economic development. Yet, as the OPEC countries have shown recently, some producers of raw materials do maneuver. In particular, they sometimes succeed in obtaining higher payments for their exports of unchanged quality. The conditions under which the terms of trade can be modified through the deliberate actions of exporters are not well studied. Nevertheless, Tugwell (1974) has described the process by which the government of oil-producing countries find themselves in a progressively more favorable bargaining position *vis-à-vis* the foreign corporations operating on their soil. His demonstration puts into question the widespread assumption that the objective interests of indigenous elites in raw materials exporting countries is to act as gatekeepers on behalf of alien capitalist interests (see Galtung, 1971). What *kinds* of national elites will emerge may depend in part on a country's place within the world-economy. The role these elites will play in their countries' economic development may be a joint product of exogenous variables, that more or less facilitate the *management of dependence*, as suggested by Tugwell, and of evolving

internal factors. Among the latter, the information processing capacity of a country, expressed in such things as the overall educational status of its population, must limit the range of available policies.

Finally, the transformation of wealth accruing from exports into industrial development must be influenced by national informational capacity which seems largely independent from the degree of processing of a country's exports.

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COMMENTS

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Durkheim did not view individual subjective states as positable antecedent conditions of social phenomena (Pope et al., 1975:418-9; Pope 1975:374, for full discussion). Moreover, he did not subscribe to the basic tenet of the voluntaristic theory of action that social phenomena are to be dealt with "as they appear from the

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Zweckrationalität, Norms and Stability in Weber

Parsons (1976:362) also challenges our interpretation of the place of normative elements in *Zweckrationalität* (instrumentally rational action). First, we did not state that *Zweckrationalität* never involves a normative orientation; rather we (Cohen et al., 1975:235) concluded that "*Zweckrationalität* is not always, or even usually, a normative category of action." Against this, we place Parsons' (1976:362) reaffirmation of his position that *Zweckrationalität* is "one mode of normatively oriented action." If Parsons did not find our earlier analysis convincing, perhaps he would consider Weber's explicit commentary more so. We quote from Weber a passage which deals with economic action and the market (bear in mind Parsons' [1975a:666] statement that Weber "used the conception of economic rationality as the point of reference for formulating" his concept of *Zweckrationalität*):

Many of the especially notable uniformities in the course of social action are not determined by orientation to any sort of norm which is held to be

valid, nor do they rest on custom, but entirely on the fact that the corresponding type of social action is . . . best adapted to the normal interests of the actors as they themselves are aware of them. This is above all true of economic action. . . . The dealers in a market thus treat their own actions as means for obtaining the satisfaction of the ends defined by what they realize to be their own typical economic interests, and similarly treat as conditions the corresponding typical expectations as to the prospective behavior of others. The more strictly rational (*zweckrational*) their action is, the more will they tend to react similarly to the same situation. In this way there arise similarities, uniformities, and continuities in their attitudes and actions which are often far more stable than they would be if action were oriented to a system of norms and duties which were considered binding on the members of a group. . . . The fact [is] that orientation to the situation in terms of the pure self-interest of the individual and of the others to whom he is related can bring about results comparable to those which imposed norms prescribe, very often in vain. . . . (Weber, 1968:30; emphasis added)

In contrast to Parsons' (1976:362) claim that Weber's rational action is normative because it entails the selection of means to ends, Weber specifically denied that selection of means to ends is necessarily normative. Indeed, Weber deliberately *contrasted zweckrational* action with action that is oriented toward norms. Weber's entire passage seems to have been written to preclude precisely the kind of misrepresentation offered by Parsons.

Parsons (1976:362) has asserted that Weber brought in "normative orientation . . . in broaching the problem of how 'social relations' become regularized." However, in the above passage, Weber explained order in terms of the individual's rational pursuit of his self-interest coupled with his expectation that others will do likewise. In contrast, Durkheim (1960a:203-4) in his discussion of "Organic Solidarity and Contractual Solidarity" wrote:

If interest relates men, it is never for more than some few moments. It can create only an external link between them. . . . If we look further into the matter, we shall see that this total harmony of interests conceals a latent or deferred conflict. For where interest is the only ruling force each individual finds himself in a state of war with every other since nothing comes to mollify the egos, and any truce in this eternal antagonism would not be of long duration. There is nothing less constant than interest. Today, it unites me to you; tomorrow, it will make me your enemy.

For Durkheim, the external links created by individual interests at best only temporarily mask an "eternal antagonism." Consequently, he tried to explain social order in terms of the

moral rules which control individual interests. Weber, on the other hand, emphasized that the extent to which normative consensus underlies given social orders varies tremendously; if such consensus underlies some orders, in others such consensus is limited or even absent. While Durkheim considered moral rules the *only* force regulating man's unsocialized desires, Weber emphasized nonnormative bases of order (e.g., usage, custom, habit and self-interest—see Weber, 1968:29, 31, 946-7) as well as normative.

STRICT CONSTRUCTIONISM IN THE INTERPRETATION OF WEBER AND DURKHEIM

Parsons (1976:361) also renewed a charge initially (1975b:106) leveled against "Classic on Classic" (Pope, 1973), rejecting our "strict constructionist" approach as too literal. Parsons (1976:364) implies that our failure to locate convergence is due to our failure to use an interpretive perspective broad enough to capture the essential areas of convergence. However, central to our critique was an analysis *in the very terms posed by Parsons himself*; that is, we assessed the extent to which Weber and Durkheim converged on the voluntaristic theory of action. Ironically, then, his complaint is actually a rejection of his own interpretive perspective.

CONVERGENCE ON THE VOLUNTARISTIC THEORY OF ACTION

In 1937, Parsons argued that Weber and Durkheim converged on the voluntaristic theory of action. Now he (1976:363) asserts that his demonstration of convergence derives from "a conceptual scheme which is somewhat different from (and broader than) those employed by Durkheim and Weber individually, and in some respects broader than can be inferred from their works taken together." Clearly, Parsons' argument has changed: Weber and Durkheim do not converge on a single theoretical perspective; instead, it is only in Parsons' own theoretical framework that Weber and Durkheim come together. This thesis differs from the original; we leave to others the evaluation of the new convergence thesis.

Parsons (1976:364) asserts that in contrast to the situation in 1937, it is now important to "reassess" the differences between Weber and Durkheim and observes that he himself may have "been remiss in failing to point out more explicitly and continuously what some of these are." We regret that Parsons has not taken this

opportunity to identify the most important differences and trace their implications, if any, for his convergence thesis.

"Some thirty-eight years after the publication of *The Structure of Social Action*," Parsons (1976:364) writes, "I 'still' adhere to the thesis of convergence, not in the sense of diminishing confidence, but, on the contrary, of enhanced confidence." He (1976:362) states he "could easily write a large volume of comment" (presumably refutation) about our "assertions and interpretations." However, he offers merely the two examples with which we dealt above, evaluation of which only reinforces our earlier judgments. We can only conclude that the "large volume" would be no more convincing than the two examples.

We have identified essential theoretical divergences between Weber and Durkheim; Parsons has not questioned their fundamental nature. Although it is true, as Parsons (1976:364) states, that one can always find dimensions of similarity and of difference between two theorists, Parsons has yet to demonstrate fundamental similarities between Weber's and Durkheim's theoretical perspectives. Our (Pope et al., 1975:417) original assertion—that the theoretical differences between Weber and Durkheim overwhelm the similarities—stands.

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METHODS OF ANALYSIS IN THE STUDY OF THE WORLD-ECONOMY*

(COMMENT ON RUBINSON, ASR,
AUGUST, 1976)

A longstanding critic of modernization theory and the sociology of development, Bernstein (1971:154), has written, "[i]n criticizing modernization theory the intention is not to replace one abstract and rigid schema with another, and the broad generalizations advanced are not to be understood as the necessary and universally applicable elements of an ideal type." In a recent article, Rubinson (1976) purports to test several hypotheses on income inequality generated from the recent literature challenging the conventional modernization approach to development. As a critique of modernization literature, Rubinson's paper is substantively correct, but his analysis contains certain characteristics which tend to rigidify the notion of a world-economy and to leave open possible interpretations which are not at all inconsistent with the modernization and sociology of development approach. The inconsistencies do not arise because of Rubinson's theoretical misconceptions of the nature of the world-economy nor because his intentions are out of line with the critique of modernization theories. The problem is that Rubinson applies conventional methods, mostly suitable to the individual level, static analysis of modernization, to an entirely different problematic. This new problematic, the dynamics of world capitalism, by definition calls for an historical analysis of the unique structural patterns of each country, both developing and de-

* I would like to thank Alejandro Portes and Reynaldo Cue for their comments on this paper. The expressed opinions, of course, are my own.

veloped. The use of aggregate economic data and the lack of detailed class analysis specific to each country lead to inferences about income distribution drawn deductively from a general theory of external control. This type of analysis reifies the conceptual tools of a world-system (e.g., core-periphery), fails to identify the true mechanisms of the internal conditioning of dependent societies and treats the spread of world capitalism in a deterministic manner (Portes, 1976). My comments are intended to point out the problems with Robinson's application of conventional methodologies to the dialectics of the world-economy.

The Use of Static Analysis

Rubinson begins with three hypotheses derived from his excellent theoretical and historical discussion of the mechanisms of control and the relations to the dominant mode of production in the world-system. These hypotheses are: (1) the greater the strength of the state, the more equal the income distribution; (2) the greater the degree of direct foreign control over production, the more unequal the distribution of income; (3) the greater the reliance of a state's production on external resources, the more unequal the distribution of income. Let us examine his analysis beyond its support for these three hypotheses.

Rubinson uses the support for the three above hypotheses to draw conclusions about change within the world-economy. This effort is questionable, as it implies concepts similar to those used in the traditional modernization arguments of development. The world-economy is a dynamic system working under the logic of monopoly capitalism. As a dynamic system, static data and analyses are misleading. Rubinson (1976:649) claims to test the hypothesis that "at early stages of economic development, income inequality is likely to increase but then the effects of development change to reduce inequality." His method is to fit a second-order polynomial to his data, the correct test for a curvilinear trend. The trend is not substantiated. Let us examine more closely the meaning of this test. With cross-sectional data on measures of economic development and inequality, the regression line runs from high inequality and low economic development (dependent nations) to low inequality and high economic development (developed nations). The only way that we can speak of "early stages of economic development" and *changing* income inequality with these data is to assume that the regression line, whether linear or not, represents a functional relationship between these nations on some continuum of

economic development. This continuum implicitly suggests that the underdeveloped nations follow the same course of development as the advanced nations; i.e., they go from one end of the regression line to the other, their distribution of income changing linearly. The assumption that underdeveloped nations will follow a path similar to that of the advanced nations is equivalent to the assumption used in the modernization approach and is actually a central theme of the critique of that literature (Seers, 1969; Bernstein, 1971; Portes, 1974).

Reification of Concepts

In relation to the meaning of the regression line with cross-sectional data, Rubinson, in essence, uses his set of independent variables as means of classifying nations along the developmental continuum from underdeveloped to developed and he shows this classificatory scheme to predict degree of income inequality. As a descriptive effort this is worthwhile and helpful; but as a statement about the development of each nation or about the workings of the modern world-system, the classification scheme is reminiscent of the dichotomies formulated by the modernization literature. Instead of traditional-modern, where degree of structural differentiation and modernity are the bases of classification, we have core-periphery with degree of state strength and equality of income distribution as the classificatory scheme. In both Rubinson's analysis and the modernization literature, definitional categories have become so reified that they are offered as means of explanation. Underdevelopment becomes an act of omission rather than commission. This argument says that nations are underdeveloped not because they are exploited by their relationship to the mode of production in the world-economy or by an international bourgeoisie controlling the strings of economic development but because they lack sufficient state strength, economic expansion, trade balances and protection from foreign investment. Clearly, these omitted characteristics are often evident; however, they are not the cause of underdevelopment nor income inequality but are merely descriptions of the latter's consequences.

Determinism and Lack of Internal Analysis

A third issue relates to the implicit notion of a developmental continuum in Rubinson's method of analysis. While the peripheralization of the third world resulted from similar processes of the spread of capitalism, these processes did not occur in the same fashion

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Parsons (1976:362) also challenges our interpretation of the place of normative elements in *Zweckrationalität* (instrumentally rational action). First, we did not state that *Zweckrationalität* never involves a normative orientation; rather we (Cohen et al., 1975:235) concluded that "*Zweckrationalität* is not always, or even usually, a normative category of action." Against this, we place Parsons' (1976:362) reaffirmation of his position that *Zweckrationalität* is "one mode of normatively oriented action." If Parsons did not find our earlier analysis convincing, perhaps he would consider Weber's explicit commentary more so. We quote from Weber a passage which deals with economic action and the market (bear in mind Parsons' [1975a:666] statement that Weber "used the conception of economic rationality as the point of reference for formulating" his concept of *Zweckrationalität*):

Many of the especially notable uniformities in the course of social action are not determined by orientation to any sort of norm which is held to be

valid, nor do they rest on custom, but entirely on the fact that the corresponding type of social action is . . . best adapted to the normal interests of the actors as they themselves are aware of them. This is above all true of economic action. . . . The dealers in a market thus treat their own actions as means for obtaining the satisfaction of the ends defined by what they realize to be their own typical economic interests, and similarly treat as conditions the corresponding typical expectations as to the prospective behavior of others. The more strictly rational (*zweckrational*) their action is, the more will they tend to react similarly to the same situation. In this way there arise similarities, uniformities, and continuities in their attitudes and actions which are often far more stable than they would be if action were oriented to a system of norms and duties which were considered binding on the members of a group. . . . The fact [is] that orientation to the situation in terms of the pure self-interest of the individual and of the others to whom he is related can bring about results comparable to those which imposed norms prescribe, very often in vain. . . . (Weber, 1968:30; emphasis added)

In contrast to Parsons' (1976:362) claim that Weber's rational action is normative because it entails the selection of means to ends, Weber specifically denied that selection of means to ends is necessarily normative. Indeed, Weber deliberately contrasted *zweckrational* action with action that is oriented toward norms. Weber's entire passage seems to have been written to preclude precisely the kind of misrepresentation offered by Parsons.

Parsons (1976:362) has asserted that Weber brought in "normative orientation . . . in broaching the problem of how 'social relations' become regularized." However, in the above passage, Weber explained order in terms of the individual's rational pursuit of his self-interest coupled with his expectation that others will do likewise. In contrast, Durkheim (1960a:203-4) in his discussion of "Organic Solidarity and Contractual Solidarity" wrote:

If interest relates men, it is never for more than some few moments. It can create only an external link between them. . . . If we look further into the matter, we shall see that this total harmony of interests conceals a latent or deferred conflict. For where interest is the only ruling force each individual finds himself in a state of war with every other since nothing comes to mollify the egos, and any truce in this eternal antagonism would not be of long duration. There is nothing less constant than interest. Today, it unites me to you; tomorrow, it will make me your enemy.

For Durkheim, the external links created by individual interests at best only temporarily mask an "eternal antagonism." Consequently, he tried to explain social order in terms of the

moral rules which control individual interests. Weber, on the other hand, emphasized that the extent to which normative consensus underlies given social orders varies tremendously; if such consensus underlies some orders, in others such consensus is limited or even absent. While Durkheim considered moral rules the *only* force regulating man's unsocialized desires, Weber emphasized nonnormative bases of order (e.g., usage, custom, habit and self-interest—see Weber, 1968:29, 31, 946-7) as well as normative.

STRICT CONSTRUCTIONISM IN THE INTERPRETATION OF WEBER AND DURKHEIM

Parsons (1976:361) also renewed a charge initially (1975b:106) leveled against "Classic on Classic" (Pope, 1973), rejecting our "strict constructionist" approach as too literal. Parsons (1976:364) implies that our failure to locate convergence is due to our failure to use an interpretive perspective broad enough to capture the essential areas of convergence. However, central to our critique was an analysis *in the very terms posed by Parsons himself*; that is, we assessed the extent to which Weber and Durkheim converged on the voluntaristic theory of action. Ironically, then, his complaint is actually a rejection of his own interpretive perspective.

CONVERGENCE ON THE VOLUNTARISTIC THEORY OF ACTION

In 1937, Parsons argued that Weber and Durkheim converged on the voluntaristic theory of action. Now he (1976:363) asserts that his demonstration of convergence derives from "a conceptual scheme which is somewhat different from (and broader than) those employed by Durkheim and Weber individually, and in some respects broader than can be inferred from their works taken together." Clearly, Parsons' argument has changed: Weber and Durkheim do not converge on a single theoretical perspective; instead, it is only in Parsons' own theoretical framework that Weber and Durkheim come together. This thesis differs from the original; we leave to others the evaluation of the new convergence thesis.

Parsons (1976:364) asserts that in contrast to the situation in 1937, it is now important to "reassess" the differences between Weber and Durkheim and observes that he himself may have "been remiss in failing to point out more explicitly and continuously what some of these are." We regret that Parsons has not taken this

opportunity to identify the most important differences and trace their implications, if any, for his convergence thesis.

"Some thirty-eight years after the publication of *The Structure of Social Action*," Parsons (1976:364) writes, "I 'still' adhere to the thesis of convergence, not in the sense of diminishing confidence, but, on the contrary, of enhanced confidence." He (1976:362) states he "could easily write a large volume of comment" (presumably refutation) about our "assertions and interpretations." However, he offers merely the two examples with which we dealt above, evaluation of which only reinforces our earlier judgments. We can only conclude that the "large volume" would be no more convincing than the two examples.

We have identified essential theoretical divergences between Weber and Durkheim; Parsons has not questioned their fundamental nature. Although it is true, as Parsons (1976:364) states, that one can always find dimensions of similarity and of difference between two theorists, Parsons has yet to demonstrate fundamental similarities between Weber's and Durkheim's theoretical perspectives. Our (Pope et al., 1975:417) original assertion—that the theoretical differences between Weber and Durkheim overwhelm the similarities—stands.

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METHODS OF ANALYSIS IN THE STUDY OF THE WORLD-ECONOMY*

(COMMENT ON RUBINSON, ASR, AUGUST, 1976)

A longstanding critic of modernization theory and the sociology of development, Bernstein (1971:154), has written, "[i]n criticizing modernization theory the intention is not to replace one abstract and rigid schema with another, and the broad generalizations advanced are not to be understood as the necessary and universally applicable elements of an ideal type." In a recent article, Robinson (1976) purports to test several hypotheses on income inequality generated from the recent literature challenging the conventional modernization approach to development. As a critique of modernization literature, Robinson's paper is substantively correct, but his analysis contains certain characteristics which tend to rigidify the notion of a world-economy and to leave open possible interpretations which are not at all inconsistent with the modernization and sociology of development approach. The inconsistencies do not arise because of Robinson's theoretical misconceptions of the nature of the world-economy nor because his intentions are out of line with the critique of modernization theories. The problem is that Robinson applies conventional methods, mostly suitable to the individual level, static analysis of modernization, to an entirely different problematic. This new problematic, the dynamics of world capitalism, by definition calls for an historical analysis of the unique structural patterns of each country, both developing and de-

* I would like to thank Alejandro Portes and Reynaldo Cue for their comments on this paper. The expressed opinions, of course, are my own.

veloped. The use of aggregate economic data and the lack of detailed class analysis specific to each country lead to inferences about income distribution drawn deductively from a general theory of external control. This type of analysis reifies the conceptual tools of a world-system (e.g., core-periphery), fails to identify the true mechanisms of the internal conditioning of dependent societies and treats the spread of world capitalism in a deterministic manner (Portes, 1976). My comments are intended to point out the problems with Robinson's application of conventional methodologies to the dialectics of the world-economy.

The Use of Static Analysis

Rubinson begins with three hypotheses derived from his excellent theoretical and historical discussion of the mechanisms of control and the relations to the dominant mode of production in the world-system. These hypotheses are: (1) the greater the strength of the state, the more equal the income distribution; (2) the greater the degree of direct foreign control over production, the more unequal the distribution of income; (3) the greater the reliance of a state's production on external resources, the more unequal the distribution of income. Let us examine his analysis beyond its support for these three hypotheses.

Rubinson uses the support for the three above hypotheses to draw conclusions about change within the world-economy. This effort is questionable, as it implies concepts similar to those used in the traditional modernization arguments of development. The world-economy is a dynamic system working under the logic of monopoly capitalism. As a dynamic system, static data and analyses are misleading. Rubinson (1976:649) claims to test the hypothesis that "at early stages of economic development, income inequality is likely to increase but then the effects of development change to reduce inequality." His method is to fit a second-order polynomial to his data, the correct test for a curvilinear trend. The trend is not substantiated. Let us examine more closely the meaning of this test. With cross-sectional data on measures of economic development and inequality, the regression line runs from high inequality and low economic development (dependent nations) to low inequality and high economic development (developed nations). The only way that we can speak of "early stages of economic development" and *changing* income inequality with these data is to assume that the regression line, whether linear or not, represents a functional relationship between these nations on some continuum of

economic development. This continuum implicitly suggests that the underdeveloped nations follow the same course of development as the advanced nations; i.e., they go from one end of the regression line to the other, their distribution of income changing linearly. The assumption that underdeveloped nations will follow a path similar to that of the advanced nations is equivalent to the assumption used in the modernization approach and is actually a central theme of the critique of that literature (Seers, 1969; Bernstein, 1971; Portes, 1974).

Reification of Concepts

In relation to the meaning of the regression line with cross-sectional data, Rubinson, in essence, uses his set of independent variables as means of classifying nations along the developmental continuum from underdeveloped to developed and he shows this classificatory scheme to predict degree of income inequality. As a descriptive effort this is worthwhile and helpful; but as a statement about the development of each nation or about the workings of the modern world-system, the classification scheme is reminiscent of the dichotomies formulated by the modernization literature. Instead of traditional-modern, where degree of structural differentiation and modernity are the bases of classification, we have core-periphery with degree of state strength and equality of income distribution as the classificatory scheme. In both Rubinson's analysis and the modernization literature, definitional categories have become so reified that they are offered as means of explanation. Underdevelopment becomes an act of omission rather than commission. This argument says that nations are underdeveloped not because they are exploited by their relationship to the mode of production in the world-economy or by an international bourgeoisie controlling the strings of economic development but because they lack sufficient state strength, economic expansion, trade balances and protection from foreign investment. Clearly, these omitted characteristics are often evident; however, they are not the cause of underdevelopment nor income inequality but are merely descriptions of the latter's consequences.

Determinism and Lack of Internal Analysis

A third issue relates to the implicit notion of a developmental continuum in Rubinson's method of analysis. While the peripheralization of the third world resulted from similar processes of the spread of capitalism, these processes did not occur in the same fashion

throughout the periphery. That is, the periphery is not a homogeneous mass related to the core through standardized mechanisms of control. Instead, capitalism has penetrated each country within the context of its own unique history. Class formations, patterns of state control and the influence of foreign investment are not only the consequences of this penetration but are the results of the *synthesis* of historical internal social relations interacting and conflicting with the forces of foreign economic and political control. It is, in fact, the nature of the world-economy that the search for wage levels where sufficient surplus can be extracted leads to a wide range of economic, political and social relations. This heterogeneity cannot be justly served by an analysis which lumps widely different countries together in an averaging type of analytical procedure. Regressing characteristics of those countries named in Appendix 3 (p. 657) says little about how the penetration of capitalism has affected income inequality or, indeed, what relationship each country bears to world production. This averaging technique leads to broad generalizations which offer little explanatory power or practical application. For example, Robinson (p. 644) writes, "Since the state is relatively weak [in the periphery], workers cannot effectively use access to the state to apply political leverage to meet demands for higher wages." However, leaving aside the fact that Robinson's treatment of state power offers few insights into actual power relationships, one is immediately reminded of the situation in Chile under Allendé and contemporary problems in Argentina where a highly organized labor force continually pressured the state for wage increases, to the point of causing a drastic state reaction. In short, the absence of any analysis specific to each country fails to identify those mechanisms of control which prevent development and encourage income inequality (Petras, 1975).

Methodology and Theory

General questions should be raised about the appropriate methodologies to be used in studying emerging paradigms of development such as the world-system. The ahistorical, cross-sectional regression analysis represented in the Robinson paper can lead to erroneous theoretical interpretations of change for reasons noted above. This type of analysis is a major contributor to the "economistic" thinking (Amin, 1974) of many developmentalists; i.e., the underdeveloped nations are just a little late in starting the race for advancement so let us help by giving them just a little more of what they

lack. We can see that the regression coefficient in this cross-sectional analysis is easily translated into this kind of policy thinking; a one-unit increase in state control of resources will move a country farther up the income equality continuum and, therefore, the developmental continuum. As Robinson (p. 652) writes, "these measures of the strength of states in relation to other states do have independent, though small, effects in reducing the degree of income inequality." However, changing position in the world-economy does not result from incremental change but from transformation of a nation's social and economic structure, a concept Robinson recognizes in his discussion but does not represent in his method of analysis. One method for studying this transformation is to examine inductively the historical patterns of countries which have changed their position in the world-economy. The ready examples are those nations which have recently moved to a position in the "semi-periphery" (Wallerstein, 1974), a concept Robinson ignores totally, lumping these nations into his developmental continuum. Only with nation-specific and true comparative studies of structural transformations can the mechanisms of control which produce income inequality within the world-system be identified.

Longitudinal analyses (e.g., Chase-Dunn, 1975) offer a more appropriate test of hypotheses relating to the world-economy. But, while techniques of analyzing longitudinal data will identify trends and covarying relations in time, we are still left with the likelihood of making economic inferences from these trends (Cardoso, 1973). The problem is that these techniques often lead to interpretations which "de-dialecticize" structural change. The dynamism of the world-economy is derived from its dialectic form. A central question which needs to be debated is how can the sophisticated statistics required to sort out many of the complex relations in a world-system be applied creatively and effectively to a dialectical system? Certainly, we cannot criticize sophisticated statistical technology on the grounds that it is too complex, but we can criticize it according to how it is managed. The analysis in Robinson's paper is one case where the benefits derived from the power of statistical technology may be outweighed by the costs to the theoretical framework, i.e., the crucial issues of structural transformation and change, which is the root of the world capitalist system.

Finally, let me comment on the use of "proxy variables" in this analysis. Clearly, all measurement is only a representation of reality and the task is to push continually to minimize

the latitude of this approximation. The question, however, is not simple. In efforts to measure social reality, we are placing a certain conceptual framework on that reality. The conceptualization of social reality in a dialectic system like world capitalism implies a holistic approach. The use of aggregate economic measures to represent external control, power relations and class formations tends to compartmentalize this holism. Therefore, when a measure of the proportion of state revenues to total GNP is offered as a proxy for state power, one must be extremely cautious in attributing much explanatory power to it. The issue is not only how much this revenue amounts to but to whom does it go and why?

In conclusion, these comments have only hinted at many of the complex issues underlying ongoing attempts to study the structure of the world-system and unequal relationships within it. These issues will arise continually as more studies of specific hypotheses are undertaken. Researchers must be careful that their methods truly address the theoretical notions of a world-system and do not compartmentalize, rigidify and reify the concepts derived from them. My sole purpose in these comments has been to identify and to call for debate on those problems of using existing methodologies, largely designed for the analysis of comparative statics, to study alternative paradigms of national development and underdevelopment.

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CROSS-SECTIONALISM: MISMATCHING THEORY AND MODEL

(COMMENT ON RUBINSON,
ASR AUGUST, 1976)

According to Rubinson, economic wealth or growth can explain only inadequately inter-country differences in equality of income distribution. His introduction of international context effects seems promising, but the cross-sectional model used provides more answers to the wrong questions. The analysis also contains several statistical and operational problems, but primary attention must be given to the assumptions underlying the cross-sectional model used since at present it is probably the dominant comparative paradigm.

Rubinson's (1976:640, fn. 1) theoretical argument proposes an "effect" of the "position of states in the world-economy" on within-state income distribution. More specifically,

the basic idea is that the greater the economic dominance and influence that states . . . have in the world-economy, the more equal the distribution of power within states, and consequently, the more equal the distribution of reward.

As stated, the idea seems sound. But in passing from idea to model, Rubinson adds, I think, a theoretically unjustified provision about equality seen cross-sectionally. Clearly, an increase in relative power may well improve a nation's internal equity without affecting its equality of income distribution relative to other nations.

Theoretical arguments discussed by Rubinson focus, among other things, on the "mechanisms affecting income distribution . . . class formation . . . production and occupational structure . . . [and] class relations" (pp. 643-4). Each of these involves changes in parameters within countries: "This paper compares

the distribution of income within states and studies the way in which the position of states in the world-economy affects this income distribution" (fn. 1). Thus, we should expect a comparison of this within-country relationship *among* countries. Interestingly, Robinson's analysis of historical evidence does exactly that, though limited by the data problems he notes (pp. 644–5).

What is not expected is the subsequent empirical examination of that relationship with cross-sectional analysis of cases. In other words, Robinson's theoretical reasoning specifies a relationship between variables that is appropriate *within* cases; the theory does not directly concern cross-sectional comparison of variables. As Lieberman and Hansen (1974) and Kuznets (1963) (both cited by Robinson) demonstrate, the cross-sectional relationship between two variables can be quite different from their relationship within cases. It was a surprise to see these particular articles cited since it is in these works, among others, that one finds explicit challenge to the validity of the cross-sectional paradigm used. (See also Kuznets, 1962; Riley, 1973; Schaie and Labouvie-Vief, 1974.)

In the second part of that same historical analysis, Robinson finds a system-specific factor that influences the relationship between economic growth and income distribution but he does not seem to notice it. He offers evidence that in certain (highly developed) countries, the distribution is more or less stable over time while in other (less developed) countries, high degrees of inequality accompany high growth rates. He does not attempt the identification of a general factor (level of development?) affecting the relationship. Yet, the "primary function of comparative inquiry" is the identification of such general factors (Przeworski and Teune, 1970: 13). Suggesting that the world-economic model may be more appropriate than a developmental model at this point (p. 646) seems premature in light of the untested possibility of such a "context effect, whereby the context operates in an interactive manner" (Przeworski and Teune, 1970: 69). Assuming an otherwise correctly specified model, such an effect would mean that the within-country relationship changes systematically with increases in the independent variable. Regressing within-country slopes on mean power measures for each country could show a systematically changing sensitivity of income distribution to further changes in power.

It is not all together fair to ask for such an intensive analysis with the type of "soft" data available, but then neither is it fair (or logical)

to call on single-point, cross-national data, no matter how "hard," to make causal inferences—unless it can be shown or assumed that the relationship between the variables is equivalent for all cases (Somers, 1971: 384). This means that Robinson has had to assume all countries develop along the same schedule; that if country A comes to occupy country B's position of power, it also will have the *same* income distribution, regardless of A's former distribution. Robinson's theory does not predict this developmental equivalence. To assume such equivalence mismatches theory and model, not an unusual event in cross-national comparisons.

To further clarify this mismatch, consider a relationship between two variables that is strongly positive and linear, *within all cases*. This condition does *not* require the slopes or intercepts to be equal, but only $b_{yx} > 0$ for each country. Seldom are theories more explicit in any case and Robinson's is no exception. To require the same slope within each case, allowing for different historical backgrounds (or intercepts), would orient the regression lines all in the same parallel direction. To further require the relationship be equivalent across cases would require all regression lines to merge into one.

We now select single points on each regression line requiring only that each $b_{yx} > 0$. Even when the theoretically predicted within-country relationship holds, these cross-sectional data coordinates need not show a relationship. If there is a cross-sectional relationship, it need not have the same slope or even the same sign (see Przeworski and Teune, 1970: ch. 3, esp. 64–8). Thus, if both within- and across-country slopes are the same, a rather special condition obtains: the same condition of equivalence required for a cross-sectional test to be appropriate for causal hypotheses. This condition is not part of Robinson's theory, and the context effect mentioned above suggests the condition does not hold.

The hypotheses described (p. 647), in short, were not tested. We do not know what the effect of increasing political power (etc.) is on inequality of income distribution within a country, because no data on covariation of these variables within a country are offered. Even if we want to assume that the functional relationship is the same for all countries operating *in vacuo*, the fact is that they are not. Nation-states occasionally learn from their own and others' history; the experience is diffused in countless ways. Controlling for date of independence (p. 648) is an unsatisfactory and generally irrelevant answer to this challenge. An analysis of covariance, as in Schuessler (1971:

238-46), would have been a more appropriate model for the hypotheses (if not the data) considered in this article.

Turning to the theory itself, we see that "power and control in the world-economy" (p. 641) is a function of (1) internal political control, (2) economic penetration abroad and (3) independence from the external market. We may accept for the moment that a "strong state structure" will enhance a nation's relative position in the world-economy for the reasons given (p. 642), but "direct economic penetration" abroad is difficult to achieve while simultaneously maintaining independence from "external markets." Generally, it does not seem consistent with the benefits derivable from exploiting international comparative advantage to propose that a nation will increase its "dominance and influence" in the world-economy by becoming independent from it. Absolute independence implies no contact. Without international trade, we also may wonder how strong the structure of a modern state can be. One questions whether there can be such a thing as independence *and* prosperity in today's industrial world-economy (Cooper, 1968).

It is at best a peculiar conceptualization of power within a "system," the components of which are isolated, yet Robinson's operationalization of "weakness" seems to advocate such a conceptualization: "Just the fact of having to import or export puts the state . . . in a position of less power and control in the world-economy" (p. 654). Given the perennial concern among industrialized countries over balance of payments, it seems more plausible that it is not *lack* of activity but more activity of one kind (exporting) than of another (importing) that implies power. The same sort of questions come up in Robinson's (pp. 651-2) separate regressions of foreign reserves and external public debt; each alone may only indicate scale rather than quality of activity. It would seem that some sort of *net* measure would be more appropriate to the operational needs here unless we insist that *any* economic activity abroad weakens a state.

Admittedly, Robinson qualifies his argument about economic penetration and external control by distinguishing between "raw products" and manufactured items (e.g., p. 642). To test whether the amount of a certain product exported (or imported) affects inequality, he includes the value of the particular product in the regression. However, the preceding argument he cites by Galtung (p. 655) concerns not *amount* of value but *specialization*, particularly in food and raw materials: the amount of a product-type exported (or imported) may

merely reflect scale and efficiency or surplus (as in the U.S.). A more appropriate operationalization for his purposes might be one where such exports are calculated as a proportion of all exports or of GDP.

Robinson's empirical results and his interpretation of them are at once instructive (e.g., his commendable concern with "overall patterns") and perplexing (e.g., his inconsistent approach to statistical inference). The key to interpreting the several tables is in the regression coefficients: if a given independent variable affects inequality as predicted, then we should see (1) a negative effect on the Gini coefficient (for "overall" inequality) and (2) a *negative* effect on the share of income controlled by the wealthiest sector (or Q_5) and a *positive* effect on the lowest (Q_1) (p. 647). The latter indicates the direction of change of relative distribution of income from the richest to the poorest sectors.

For the first independent variable (economic development), Robinson claims the model is supported. But the model predicts *positive* effects on the bottom quintile(s) and negative effects on the top. In fact, both are affected negatively: the share of income accruing to the middle groups is increased by (significantly) reducing both the richest *and* poorest groups' shares (p. 649). Nor is this an isolated instance of Robinson's finding support for an hypothesis when that is not clearly indicated in the data.

Also, we see that the standardized regression coefficient for government revenue on overall inequality (Gini) goes from -0.289 ($p < .05$) in Table 2, to -0.406 ($p < .05$) in Table 6 when debits and capital formation are controlled, even when n decreases from 47 to 39, respectively. In light of conclusions drawn from much less in the article, a change of this magnitude and significance deserves some explanation. One might conclude, for instance, that the effect of government revenue on inequality does *not* remain the same, demonstrating that the various mechanisms of power and control in the world-economy have *interdependent* effects on the degree of material inequality. In fact, Robinson concludes the opposite (p. 653).

Several disturbing practices harm the article's statistical credibility. The text compares regression coefficients that do not reach significance (even at .10) with those that are significant (at .05) and therefore can be generalized (e.g., pp. 650, 652). Such comparisons seem to put significance in a (sometimes appropriate) secondary place. But the comparison (p. 650) of the relative independent effects of government revenues and economic

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development on inequality apparently depends on a standard multiple regression decomposition of statistically significant contribution to explained variance. Such uses of significance seem inconsistent.

Less serious but still disturbing are such lapses as, in Table 5, the coefficients for foreign reserves: "In all cases the slopes are greater than their standard errors" (p. 652). Having thus attracted our attention to it, we notice the coefficient for Q_4 . Other hypotheses were found "supported" even when some of the relevant standard errors were greater than the regression coefficients. (All together, $SE_b > b$ for about 15% of the coefficients reported, or at least one such coefficient in nearly 80% of the regression equations.) Moreover, how does one interpret the eight pairs of (standardized and unstandardized) coefficients that are of opposite signs? (e.g., Table 3, the coefficients for government revenues on the Gini index).

Rubinson's article illustrates that cross-sectional comparisons may be both misleading and missing the point of comparative social inquiry. The crucial unit of comparison is not the case, whether nation, economy or individual, but the functional relationship operating within and among the units of analysis or cases. Theoretical reasoning ordinarily specifies relationships that are operating within each case (Blalock, 1972: 426) as, for example, that between power and inequality within nations. Further, rather than assume that the relationship between power and inequality is equivalent from one nation to another, changes in that relationship should be the focus for comparison. In Rubinson's case, we may attribute the use of the cross-sectional model to lack of longitudinal data (Lieberson and Hansen, 1974: 524); however, the frequency of cross-sectionalism in comparing nations, even when time-series data archives are relatively readily available, suggests other basically logical reasons for its current dominance. Repeated (mis)use of the model in empirical research, in spite of its known limitations, may have led to its unconscious acceptance logically and theoretically. (See Nisbet's [1976: 97-8] criticism of Parsons [1966].)

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REPLY TO BACH AND IRWIN *

One of the major concerns of the social sciences has been to describe and explain the origins and development of the modern world. During the past twenty years, work on this problem has been dominated by the theoretical approach known as modernization or development theory. In this conceptualization, the countries in the world exist at different stages of modernization or development, and the

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basic problem is to specify the conditions under which countries proceed from a stage of underdevelopment to a stage of development. More recently, this same problem has been reconceptualized by what is known as the world-system perspective. In this conceptualization, the countries in the world are seen as located within a single economic division of labor. And, as in any division of labor, groups that perform different roles produce different class structures and different political forms. The basic problem is to study this system's process of production and exchange, the creation and utilization of power, and the organization and maintenance of inequality. Countries are not independent entities in this system, but they represent political structures within different economic roles in the single division of labor (Wallerstein, 1974).

In the paper critiqued by Bach and Irwin, what I did was to approach a problem dealt with extensively by modernization theorists, the explanation of inter-country differences in income inequality, from the world-system perspective. I argued that much of the difference in inequality could be explained as a consequence of the position of countries within the world-system. From this perspective, I argued that the basic mechanisms affecting income distribution were class formation, the production and occupational structure, and class relations. I argued that these factors were not exogenous variables, but were themselves a function of the position of countries within the world-system, such that the creation of certain forms of class formations in some countries created certain complementary forms in other countries. From this theoretical analysis, I generated some testable hypotheses, tested them by cross-national regression analysis, and found that the hypotheses were confirmed. I concluded by arguing that the world-system approach seemed more valid than the modernization one.

Neither Bach nor Irwin directly question the theoretical analysis. Rather, they are both concerned with the methodological approach used in the empirical analysis, and they raise the issue of the relationship between the theory and the method. Bach argues that the world-system framework is a "new problematic" which "by definition calls for an historical analysis of the unique structural patterns of each country." Irwin argues that no theory about countries is testable through cross-national comparison, and that only an analysis of covariance design is appropriate for comparative research.

Their basic concern seems to be that statisti-

cal methods, in this case cross-national regression analysis, implicitly assume specific substantive theories. Neither Bach nor Irwin are raising the more usual issue that any statistical method is based on certain statistical assumptions, for example, about the nature of the error process or the shape of relationships among variables. Rather, they are raising issues about the *philosophy* of method. They are arguing that for each substantive theoretical approach, there is a specific type of method required. The question they raise is fundamental, but I disagree with their answers.

Bach and Irwin raise three specific issues: (1) that a regression analysis assumes a developmental or modernization theory, (2) that countries are unique and therefore analysis must be on a case-by-case basis and (3) that longitudinal analysis is preferable to cross-sectional analysis.

Regression analysis and development theory. Both Bach and Irwin assert that using a regression technique assumes that nations must develop along a set path; and since the world-system approach argues that nations do not, then regression is an inappropriate method for testing this theory. But all a regression analysis does is to describe the relationship between variables, in this case by minimizing the sum of the squares of the errors. The variables that are included in the model and the interpretation of the process relating the variables are derived from the substantive theory of the researcher. They are not contained within the statistical method of minimization. Just because a line can be drawn along which countries can be ordered does not imply that all countries must develop along a set path. That a regression analysis does not assume a developmental theory should be obvious from the fact that such analyses are used in all kinds of fields to test many different kinds of theories.

Irwin misunderstands Robert Somers' discussion of the assumption of developmental equivalence. (Somers' choice of this term is unfortunate here, since he is not discussing the substantive theory of development.) Somers is simply pointing out that in the use of a cross-section regression model for causal analysis, the researcher is assuming that *if* a unit were to change its values on the independent variables in the model, then it would change its value on the dependent variable in the manner described by the regression coefficients. This assertion is, of course, true; for it represents exactly that hypothetical experimental manipulation which lies behind *all* nonexperimental research. This same assumption applies to both cross-sectional and longitudinal designs and to all

forms of nonexperimental causal analysis. The point which Irwin misses is the hypothetical *if*. Somers is not saying that the unit *has* to change its values on the independent variables; rather, he is saying *if* it does change on those values, *then* it would change its value of the dependent variable. I think it is this misunderstanding of the nature of nonexperimental research which allows Irwin to argue that because I employed regression, I "had to assume all countries develop along the same schedule."

The claim that regression analysis implies developmental or modernization theory seems quite widespread to me (cf. Moul, 1974). I suspect this idea arose because modernization theorists often have used regression analysis to test their hypotheses, while other types of theorists of development have not; but this is certainly no reason to think that regression implies a specific theory of development. This claim is just incorrect.

Uniqueness and historical analysis. Both Bach and Irwin also raise the issue of the uniqueness of countries. Bach argues that any analysis must be on a case-by-case basis and comparing across countries is invalid. Every country is unique; but that is not to say that relationships among variables are unique. This issue had been discussed at length by Zelditch (1971), so I will not pursue it here at any length. Simply put, if one's interest is in knowing everything about a country, then one should research the history of that country. But if one's interest is in theory, then one should study relationships among variables; and the fact of uniqueness is irrelevant toward that purpose.

However, I do not think that Bach really wants to do history but rather, the theory of underdevelopment. He understands that the process of peripheralization has been basically the same (in many respects, not all) across countries since the emergence of the capitalist world-system in the sixteenth century. I think what Bach objects to is the fact that modernization theory does not fit the historical data for countries, and that such theorists almost totally have neglected to see whether their theories fit such data. But that is no reason to imply that there is a specific "historical methodology." Historical data, if we want to use that term, is merely older than contemporary data; but that does not give it any different logical status. I think that we presently have only one empirical methodology. Historical and contemporary analyses all have the same logical status, and all must confront the same problems. In terms of the logic of empirical inquiry, I see no difference between the empirical logic in the analyses of Wallerstein or Bar-

rington Moore and the research designs to which Bach objects. The important issue is how well one does the research, not the logical status of different kinds of research.

Irwin, too, emphasizes the uniqueness of countries, but in a very different way. He makes two claims. First, he asserts that any theory about relationships among variables in countries cannot be tested by comparing across countries, but only within countries. Second, he asserts that comparative research consists *only* in the identification of system-specific factors which alter the relationship among variables across countries. From these two assertions, he argues that my theory could not be tested by my research design. His first assertion is incorrect, and the second one confuses theory and method.

Irwin says that since my theory asserts that state strength reduces inequality within countries, then that hypothesis cannot logically be tested by comparing the relationship between these two variables across countries. Does he also think that a hypothesis relating social class to educational aspiration among individuals cannot be tested by comparing individuals? He says that my hypothesis logically can only be tested longitudinally within countries. Here Irwin is misunderstanding the basic idea of a unit of analysis. A unit of analysis, as he correctly notes, is simply an instance of the replication of a process (Zelditch, 1971: 282). Therefore, to test the relationship between state strength and inequality, one can use any instance in which these two variables occur, providing the scope conditions of the theory are met. A set of valid units for comparison, then, may be found in time (a time-series analysis), in space (a cross-sectional analysis) or in both time and space (a panel analysis). Any theory can be tested in any or all of these ways, because what is being compared is the relationship among variables. There is no one proper or improper unit of analysis. As Zelditch (1971:282) states, "one cannot assume that units are somehow out there in the real world, independent of our conceptualization of them." From the point of view of the logic of research, testing my hypotheses by comparing the relationships across countries is equally as valid as testing them by comparing the relationships across time periods.

Consider Irwin's claim that my hypothesis relating state strength to inequality cannot be tested by comparing across countries but only over time within a country. If we use this latter design and assume that the proper lag for this relationship is ten years, then the unit of analysis is, for example, Chile from 1935 to

1944, from 1945 to 1954, from 1955 to 1964, etc. The test of the hypothesis consists in comparing the relationship between state strength and inequality across time periods. This comparison is a valid test of the hypothesis; but it is no more or less valid than comparing the relationship between the variables across countries. In the first case, the comparison is over time; in the other case, the variation is over space. From the logic of empirical research, both comparisons are equally valid, since both types of comparisons are comparing instances of the replication of the process.

For Irwin to claim that only comparison within a country over time is a valid comparison, he needs to have a *theory* which asserts that only variation over time, as opposed to space, meets the conditions for the process to operate. Irwin does not have such a theory, nor does the theory I developed make such a claim. Irwin is taking what must be a theoretical assertion and turning it into a methodological fiat. If one were to make a theoretical claim that variation over time within a country met the conditions for the process to operate, whereas variation across countries did not, then one is making quite strong claims about the uniqueness or particularness of countries. But why should this be true? Is Chile from 1935 to 1944 really the same as Chile from 1965 to 1974? In many ways, Chile is obviously not the same, for the form of regime differs, the occupational structure differs, and many other features differ. Similarly, countries differ in their form of regimes and their occupational structures; but none of those differences are what matters for the logic of comparison. The only thing that matters is that what is being compared, time periods or countries, is a replicate of the process.

Irwin claims to find empirical evidence for his assertion that the relationship between state strength and inequality varies radically across countries. He points out that "the standardized regression coefficient for government revenue on overall inequality (Gini) goes from -0.289 ($p < .05$) in Table 2, to -0.406 ($p < .05$) in Table 6. . . ." But if one wants to compare causal effects across equations, then one must compare the unstandardized coefficients (Blalock, 1967). In Table 2, that coefficient is -0.006 ; and in Table 6, that coefficient is -0.007 . That difference is not significant.

Finally, let us consider Irwin's claim, taken from Przeworski and Teune (1970), that comparative research consists only in the identification of system factors which alter the relationships among variables across countries. This is the basis for Irwin's claim that only an

analysis of covariance design is appropriate for cross-national research. From my previous argument, it should be clear that there is no *a priori* methodological basis for such a claim. Such a statement is a theoretical assertion. But Przeworski and Teune are taking a theoretical claim and, like Irwin, converting it into a universal methodological directive. This is a confusion between theory and method. One could have a theory about how relationships among certain variables differ under certain conditions. In this case, one appropriate test of such an hypothesis would be to use an analysis of covariance design. I have, in fact, done such an analysis elsewhere (Rubinson, 1977); but this is just a case of matching a specific hypothesis to a specific research design. There is no reason to claim that this is either the goal of comparative research or the only appropriate research design for such research. I believe, however, that Przeworski and Teune make this assertion because they implicitly believe that countries really are unique, and therefore comparisons across countries are not useful. They, like Irwin, present no theoretical reasons for such an assertion. Rather, they make methodological claims about an issue which is theoretical.

Cross-sectional versus longitudinal designs. A third point which both Bach and Irwin make is that longitudinal designs are better than cross-sectional designs. I also agree with this assertion; and if I had had sufficient longitudinal data, I would have employed a longitudinal design. However, recognition of the superiority of longitudinal over cross-sectional designs does not provide an *a priori* basis for an assertion that the results of cross-sectional designs are not correct. This is an empirical question which must be decided instance-by-instance. The conditions under which inferences derived from cross-sectional and longitudinal analyses converge have been discussed elsewhere, and there is no need to repeat them here. I want to make just one comment on this issue, for I think it is misunderstood by many researchers.

Many researchers seem to feel that the use of a longitudinal research design, in which the independent variables are measured in time before the dependent variable, is sufficient to prove the hypothesized causal ordering of the model. This is not correct. There are an innumerable number of misspecifications which will "fit" any set of longitudinal data, just as there are an innumerable number which will "fit" any set of cross-sectional data. A longitudinal design is no panacea for research. A longitudinal design is only *one* step better than a cross-sectional design. More important than one's research design is one's theory, for no

model is better than the theory from which it is derived.

Miscellaneous issues. Both Bach and Irwin raise a number of smaller and diverse issues. I will answer a few of them in the remainder of this comment.

Irwin wonders what the conclusions from certain of my analyses would have been if I had changed the order of entrance of the independent variables in the equations. He is assuming, here, that I used some type of stepwise procedure for estimation. I did not. All independent variables in all equations were entered simultaneously, which is consistent with the theoretical model. He is correct when he points out that one coefficient I said was larger than its standard-error in Table 5 was not. But this correction does not change the substantive conclusions that I drew from the analysis. Irwin also wonders how to interpret those pairs of standardized and unstandardized coefficients which are of opposite signs. They should be interpreted for what they are—printing mistakes. The verbal exposition in the paper clearly specified the sign of those effects. Finally, both Bach and Irwin confuse hypotheses that I derived from modernization theory with those I derived from the world-system perspective. Thus, Bach says that I slip into the perspective of modernization theory when I test the hypothesis that “at early stages of economic development, income inequality is likely to increase but then the effects of development change to reduce inequality” (Rubinson, 1976: 649). Here I am explicitly testing the modernization hypothesis, not my own. Similarly, Irwin thinks that in my discussion of Table 1, I am incorrectly finding support for my own hypothesis, when I am actually showing that the modernization hypothesis that level of development reduces overall inequality is not supported in the analysis (Rubinson, 1976: 649).

Conclusion. In conclusion, it is important to go back to the original and fundamental objections to my analysis. Both Bach and Irwin argued that the method by which I tested my hypotheses was invalid because of the kind of theory I was using to approach the question of inter-country differences in inequality. Their objections, I pointed out, raised questions about the philosophy of methodological inquiry. They both assert that the usual forms of empirical inquiry are invalid. I have tried to explain why I believe they are incorrect. If we were to adopt their prescription and require a different method for different theories, then we would never be able to test the validity of different theories, because there would be no way to untangle empirical results from

methodological technique. The real problems in research are not methodological but conceptual and theoretical.

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SOCIAL CONSTRUCTIONS AND URBAN ETHNICITY

(COMMENT ON YANCEY, ERICKSEN AND JULIANI: ASR JUNE, 1976)

In their article, Yancey et al. (1976) attack the assimilationist and pluralist perspectives in American sociological studies of ethnicity. The major contention of the article was that the concentration of “pluralists” and “assimilationists” on the explanatory power of cultural origins as determinates of ethnicity in urban America is ill-founded. It was suggested by the authors that the role of a constellation of urban, historical, ecological, industrial and growth factors was of greater moment than that of an imported culture in the formation, maintenance and development of American urban ethnic identities. Although I accept the main thrust of the authors’ criticisms and also agree that the variables discussed are important

ones, Yancey et al. seem at least partially guilty of not practicing what they preach.

Their claim that "the development and persistence of ethnicity is dependent upon structural conditions characterizing American cities and position of groups in American social structure" (p. 391) is incomplete. One may clearly benefit from an analysis of these factors in trying to understand variations in the salience of ethnicity for any particular group in numerous situations. But the implication of the article is that the definition of the ethnic unit itself is not problematical, nor is mention made of the possible effect of these very factors on variations in ethnic definition. Yet one of the most important sociological questions concerning the generation of urban ethnic identities revolves around the use, acceptance or rejection of particular ethnic labels in various contexts and the effect which various, even discrepant definitions of ethnic entities have on ethnic response and the generation of new identities.

To give a very brief example, New Zealand recently has been experiencing an influx of migrants from the Cook Islands, Tokelaus, Fiji, Tonga, Western Samoa and Niue as well as the urbanization of its Maori population. As they get caught up in the urban milieu, specific factors discussed by Yancey et al. affect ethnic response. Yet these people, as different from each other as Poles, Jews, Italians, Greeks, Irish and Germans, may be labeled "Polynesian" in certain situations and treated as an undifferentiated ethnic group. Although this is a source of some resentment on their part, Pacific Islanders and Maoris are forced to accommodate to or confront these *pakeha* (white) definitions of the situation. In the course of doing so, their identities undergo some interesting changes. Literature dealing with American cities briefly discusses similar situations (e.g., Glazer and Moynihan, 1963; Hannerz, 1974) as do articles concerned with other areas.¹ Although I will not review these or the complex ethnic kaleidoscope I researched in Papua New Guinea towns (Levine, 1976), the point to stress is that ethnic identities as emergent phenomena arise through social interaction. They are constructed, objectified, etc., by interacting individuals whose social constructions fundamentally affect the course which ethnic emergence takes. Various factors may condition that interaction but they

do not determine the form of any particular "ethnic culture."

Without this perspective, the authors' argument remains culturally deterministic in regard to the form which particular identities take. Using their ideas alone, one still must rely on "cultural" definitions of urban ethnic units,² taking them as they are found and assuming their present form existed "since time out of mind" (Horowitz, 1975:113). The authors do briefly indicate that they are aware that new ethnic identities form in urban America (e.g., the hillbilly community mentioned on p. 397); but although we may all agree that the city is the place to look for the generation of such local identities, the constellation of specific factors discussed in the article is inadequate for a complete understanding of ethnic formation processes. Intensive studies of ethnic formation must take social interaction, the negotiation of identities, etc., centrally into account. Although an interactionist perspective used alone would be incomplete, a combination of the authors' ideas with empirical studies of the meaning, use, implications, etc., of various ethnic labels would be most productive. In this way, one can avoid some of the pitfalls in which ethnic studies have often become trapped.

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¹ See, e.g., Horowitz (1975) for a number of brief examples. This paper, Charsley (1974) and Levine (1976) make various theoretical points and provide further citations to relevant literature.

² Charsley (1974) points out that even if apparently stable identities are transported from one area to another by migration, the reasons they don't change are subject to the same analysis of migration patterns and various conditions in the host society as "new" identities are.

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NEO-ETHNICITY AS DEFENSIVE POLITICAL PROTEST

(COMMENT ON YANCEY, ERICKSEN AND JULIANI, ASR JUNE, 1976)

There is an obvious problem with our understanding of ethnicity. We had thought the salience of ethnicity was generally diminishing, when within the last decade there appeared a resurgence of ethnic solidarity and identification. This raises two questions. What is to be made of this neo-ethnicity, and does this invalidate the assimilation thesis? Present thinking seems to be that this ethnic revival reflects a persistence of ethnicity as an important factor in American life, and that the melting pot thesis may have been premature (Glazer and Moynihan, 1970; 1975; Novak, 1971; Greeley, 1971).

Yancey et al. (1976) also adopt the anti-assimilation position, but with an interesting twist. They argue that ethnicity should not be considered a fixed cultural quotient that either simply persists, as in the pluralist version of America, or gradually diminishes, as with the assimilation thesis. Ethnicity, in effect, ebbs and flows, depending upon the ecological structure of the cities in which ethnic groups find themselves. If people are commonly grouped by occupation and residence and share common institutions and services, then ethnic solidarity should flourish and persist; if these factors are absent, ethnicity should diminish.

I want to argue with Yancey et al. on three interrelated points. (1) We cannot speak of ethnicity as if it were one unified phenomenon. The highly *politicized* ethnic solidarity of the past decade is quite different from the notion of ethnicity as customs, religious practices, language, neighborhood networks, and everything else one ordinarily means by "ethnic." Yancey et al. conceive ethnicity in this way, but the most distinctive aspect of the new ethnicity is that it is largely a matter of *defensive political protest*. An evaluation of whether ethnicity is persisting or diminishing must take into ac-

count its recent political character. (2) Political protest and defensive solidarity movements are not generated by the simple ecological structure of cities. The intervention of the national government in local politics and the growing political power of blacks are generating white ethnic protest as they resist the threats to their dominance of city politics. The causes of this ethnic solidarity are political, not ecological. (3) Taking the political character of ethnicity into account leads one to conclude that this outburst of ethnic solidarity should be seen as one of the last gasps of resistance to assimilation, rather than as an indicator of ethnic persistence. Ethnic solidarity reflects neither the persistence thesis, nor the ebb and flow thesis of Yancey et al., but reinforces the original assimilation proposition.

Ethnicity as Defensive Political Protest

Whether we say "blue collar" or "lower middle-class" or "homeowner" in New York City, or whether we say "Italian" or "Irish" is not unimportant, and yet we know we are talking about roughly the same people. So the mass media discourse about the "white ethnic groups" or the "white working- and lower middle-class"—the people are the same, and the issues are the same: their feelings that they have been ignored, have received little from government in recent years, and have borne the brunt of the costs involved in the economic and political rise of the Negroes. (Glazer and Moynihan, 1970:xxvi)

Since about the time of Wallace's 1964 primary campaigns and the emergence of the "white backlash," we have experienced an almost unending succession of demonstrations, protests, complaints and collective violence by white ethnics against the intrusion of the national government into local politics to improve the status of blacks.

There have been protest marches, demonstrations and violence in resistance to court-ordered busing, as in South Boston; conflicts between white teacher associations and black administrators, as in the Ocean Hill-Brownsville fight over decentralization (Ravitch, 1974); struggles over who will control the police, as seen in the resistance to Police Review Boards (Abbott et al., 1969) and increased militancy by police officer associations (Bop, 1971); bitter mayoralty elections between white ethnic and black candidates (Levy and Kramer, 1972); a new militancy among voluntary associations which were traditionally of the self-help variety; and support for national politicians like Wallace and Agnew who opposed the extension of federal power into local politics and reaffirmed the sanctity of

family, neighborhood, church and authority in general (Lipset and Rabb, 1970).

The origins of this protest and conflict are not the simple ecology of the city. The Irish of South Boston do not express ethnic solidarity simply because they share common occupations, residential stability, or common institutions (the variables of Yancey et al.), but because they are reacting to an attack upon the local privilege of controlling schools. Their ethnic pride is a *defensive reaction* to an external threat. Similarly, many of the local heroes of the new ethnicity, such as Louise Day Hicks of Boston, Frank Rizzo of Philadelphia, Anthony Imperiale of Newark or Albert Shanker of the AFT, have gained their fame from engaging in defensive political activity and defending the slipping position of ethnic groups in the structure of local politics.

Nation-Building and Local Resistance

Defensive solidarity movements are a regular aspect of the political process, occurring throughout the history of nation-building in the West. The reaction of American cities during the past decade can be seen as another instance of the resistance of local polities to the expansion of state power. Local protest movements, in the form of food riots (Tilly, 1971), anti-tax and anti-conscription riots (Tilly et al., 1975), and the protest of the so-called Parisian "little people" (Rudé, 1959) to the intrusions of the new regime during the French Revolution, seem to occur at periods when the national government is expanding its authority at the expense of local sovereignty. Rudé's (1959:225) observations on the protest of the Parisian "little people" is applicable to the ethnic protest of today. They protested "not to renovate society or to remodel it after a new pattern, but to *reclaim traditional rights and to uphold standards* which they believed to be imperilled by the innovations of ministers, capitalists, speculators, agricultural 'improvers,' or city authorities" [emphasis mine]. The ethnic protest over schools, police, city hall, and shifting moral standards is a similar defensive protest to *reclaim traditional rights* which are being *imperilled* by the intrusions of the government and growth of black political power.

Since the early 1960s there have been unprecedented federal challenges to the local structure of power and authority. The object here was not the building of a national economy, levying taxes, or conscripting an army, as in the earlier expansion of state authority, but extending the rights of national citizenship to blacks. Between the Great Society programs

of the early 1960s, with their community action projects and threats to withhold federal funds, the civil rights legislation of the mid-1960s, the court-ordered busing for school integration in the late 1960s and the affirmative action policies of the 1970s (Glazer, 1976), city governments have been under almost constant pressure to share political power with blacks. These local polities, like their European brethren earlier, react with protest. Ethnicity is so merged with the substance and structure of city politics that federal intervention into local politics becomes not only a crisis in local authority but also a crisis in ethnicity itself.

The Institutionalization of Ethnicity in the Organization of City Politics

Along with the usual idea of ethnicity as a cultural heritage, it has persisted in an *organizational* form, in the reality of city politics. The ousting of native Yankee-Protestant control from large northeastern cities was one of the principal means for immigrant mobility (Hofstadter, 1955). "Public employment was a major channel for the Italian, the Irish, and the Jew, each of whom by successively taking over whole sectors of the public services, gave various municipal agencies their distinctive ethnic coloration. Now blacks are the newcomers" (Cloward and Piven, 1975:227). The political machine and patronage system provided a means for perpetuating ethnic control over cities, a control that only now is being challenged by the new "immigrants," the blacks. In America, city politics have been ethnic politics (Banfield and Wilson, 1963) in that they are both the institutionalization of group advantage (controlling city hall, schools, etc.) and a symbol of the presence and persistence of ethnicity in America. Ethnic protest emerges as a means of ritually reaffirming the collective identity of that social arrangement, which is now being attacked, and as a more literal means of resisting this loss of control.

Neo-Ethnicity: The Last Quiver of Assimilation?

We return to the original question: What of the melting pot thesis? The control ethnics have exercised over American cities can be seen as part of the *persistence* of ethnicity, and the subversion of that control by the government and the rising political strength of blacks must be seen as part of its *demise*. Assimilation is more than the shedding of habits, customs and social identities. It is also the crumbling of the organizational manifestations of ethnic per-

sistence: the control of city politics taken earlier from the native Yankee-Protestants. While assimilation is by no means complete, this outburst of defensive ethnicity means ethnics are losing control of one of the social structures in which ethnic consciousness and power was institutionalized. The new ethnicity reflects weakness, not strength, and the demise of ethnic control over the political structures of their communities, not its persistence. In this sense, the original assimilation hypothesis is ironically supported by this appearance of ethnic solidarity.

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REPLY TO LEVINE AND BERGESEN

Although Levine and Bergesen disagree regarding assimilation as a model of ethnicity, their principal criticisms of our paper are similar: for both of them, we focused too heavily on economic, ecological, and institutional explanations of ethnicity. For Levine, we omitted social interaction between individuals whose "social constructions" are said to have an important impact on the formation of ethnic identities. For Bergesen, we omitted political factors, particularly the social-psychological reaction of white ethnics to their loss of political influence. We accept their characterization of our paper as largely ecological. We were interested in identifying the structural relationships between the changing political economy of American cities and the formation and maintenance of ascriptive groups.

Levine's central hypothesis, that "ethnic identities as emergent phenomena arise through social interaction," appears reasonable to us. There is a long tradition of social psychology which has demonstrated the importance of social interaction for self-identification. We see no reason why these hypotheses do not apply to ethnic identities. We strongly suspect that structural conditions set the parameters within which the interaction takes place and within which the "social constructions" are generated. We suggest that the structural conditions are central to the formation of ascriptive groups, while social interaction between emerging groups—as well as interaction within any single group—may be central to the formation of personal identities.

Levine concludes his comment with the assertion that without the interactionist perspective, one "must rely on 'cultural' definitions of urban ethnic units, taking them as they are found, and assuming that their present form

existed 'since time out of mind.' " While he notes that we discussed the formation of the white Southern ethnic group in Chicago, he apparently failed to understand the major point of our paper. We concluded that rather than being a survival from other times and places, ethnicity may be best understood as a product of the American social structure. We came to this conclusion without the interactionist perspective.

In contrast to Levine's essay, which raises provocative research questions concerning the relationship between the political economy, social interaction, and the emergence of groups and personal identities, Bergesen's comment is based on a series of ahistorical assumptions concerning ethnicity, urbanization, urban politics, and opportunity structures. Apparently related to the ahistorical nature of his remarks is the misdirection of inquiry from the urban macro-structure to the defensive reactions of urbanites.

In commenting on a paper which emphasized the variable nature of ethnicity and which identified three forms of ethnic groups in contemporary cities, Bergesen writes: "We cannot speak of ethnicity as if it were one unified phenomenon. The highly *politicized* ethnic solidarity of the past decade is quite different from the notions of ethnicity as customs, religious practices, language, neighborhood networks and everything else one ordinarily means by being 'ethnic.' Yancey conceives of ethnicity in this way. . . ." We were relatively precise in our definition of ethnic groups. None of the elements of our definition are included in Bergesen's characterization of it.

Bergesen's critique of our paper is replete with ahistorical models of urban social structure. He suggests that recent federal efforts aimed at improving the level of social justice in cities has resulted in the unprecedented loss of local autonomy; he chooses to ignore the fact that the activities which have built cities—commerce, industry, and government—inevitably tie local areas to distant hinterlands. He assumes that local politics have been independent of external controls and relationships until the 1960s; he chooses to ignore such persuasive authors as Pirenne (1925), Duncan and Lieberman (1970), Vidich and Bensman (1958) and Warren (1963). He assumes that urban political structures have been static, remaining unchanged except for their ethnic coloration—Yankees, white ethnics and now blacks; he ignores the dynamic aspect of urban politics (Merton, 1957). He assumes that the position of blacks today is analogous to that of immigrants seventy years ago; he ignores the

differences in the structure of cities, in urban opportunities and the overriding and persistent fact of racial discrimination. Northeastern cities at the turn of the century were the centers of commerce and industry; some are now facing fiscal disaster.

Bergesen may be correct in his assertion that there has been an increase in the level of political and group consciousness among the urban white working classes. He may be correct in his assertion that this is related to the changing structure of urban politics. We are not convinced that it is possible to separate changes in urban politics from changes in urban ecological structures; neither are we as sanguine as he regarding recent federal intervention into local politics and the increased political power of blacks as *the cause* of solidarity among the white working class.

The most disturbing aspect of Bergesen's note is his assertion that the recent complaints of the urban working class are political in nature and not "simple ecology." Such an analysis avoids concern with the ecological constraints imposed upon the urban working class. It turns a social issue that is generated from structural conditions into an analysis of individual complaints. By contrast, the "simple" ecological perspective enables one to pursue the question of how structural location within complex societies encourages (or discourages) specific forms of organization and political action. In the case of the urban working class, it considers the fact that many ethnic communities were formed around expanding concentrations of industrial employment. That was over fifty years ago. Today these same communities are characterized by relatively old (and increasingly less competitive) industries or by vacated industrial shells. We don't claim to know *the cause* of the complaints of the Irish (and non-Irish) in South Boston, but we suggest that if they did not have common occupations, residential stability and common institutions, if they did not face similar economic structures, if they were not entrapped in urban neighborhoods experiencing industrial decline, then they would be less likely to behave as communities. It appears to us that the "new ethnicity" is a substantively different phenomenon from the "old ethnicity." To equate the two or to describe the new ethnicity as a reactionary movement is to ignore the economic structures from which it has emerged.

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IS COMMUNITY ALIVE AND WELL
IN THE INNER CITY?

(COMMENT ON HUNTER, ASR
OCTOBER, 1975)

Hunter (1975) has presented data which he claims questions the widely held view that ecological processes, notably urbanization and "decline" of inner-city neighborhoods, entail a loss of community. Three dimensions of community are investigated:

1. community as a functional spatial unit meeting sustenance needs,
2. community as a unit of patterned social interaction,
3. community as a cultural-symbolic unit of collective identity.

Replicating a survey conducted in 1949 in an inner-city neighborhood, he finds, in 1974, that while use of local facilities (shopping, churches, movies, doctors, banking and employment) has declined, the level of informal neighboring (chatting, exchanging favors, visiting, etc.) has remained about the same, and the "sense of community" (identification with place as a distinctive area, measured by an attitude scale) has increased. He (p. 549) interprets this as an indication "that the hypothesized consequences of an ecological and functional increase in scale have not resulted in a social and cultural-symbolic loss of community in this area."

A question can be raised as to whether the indicators used adequately measure the dimensions of community selected for analysis. Definitions of community, and each of these dimensions, focus upon integrative mechanisms within differentiated wholes. The holistic char-

acter of the urban neighborhood is suspect, by definition. A community should be capable of meeting the full range of fundamental human needs (Arensberg, 1961; Konig, 1968; MacIver, 1937). By this requirement, the neighborhood studied by Hunter was not a community in 1974, nor was it when studied by Donald Foley in 1949. Only 17.0% of the respondents worked in the neighborhood in 1949 and only 11.4% in 1974. Most of the major institutional functions, such as employment, government and education, were organized on a city-wide basis. At both times, the relatively complete local society was the larger metropolitan field. That urban people interact casually with their neighbors and have an identity with their area of residence in the city is not surprising. The empirical literature has not suggested otherwise. But to take such as evidence of a unit of patterned social interaction or of a cultural-symbolic unit of collective identity is to exaggerate the meaning of the measures. The finding that neighboring and place identification were weakly related to use of local facilities in 1949 and in 1974 shows that these are neighborhood rather than community phenomena. With functional integration removed to the larger field, these measures simply show persistence of association among neighbors in common activities of household and family life.

Hunter also presents observations based on his field work to argue that movement into the area in recent years has been motivated in part by a search for community and that among residents there has been a conscious effort to create community. Many people moved into the area, according to Hunter, because they desired such characteristics of the area as cheap housing, an urban environment, a racial mix and proximity to the University of Rochester. Such reasons, we submit, have no necessary connection to community; they merely reflect values which make the area attractive to some people. Some people, as Hunter notes, were searching explicitly for community, but other motives appear to have been dominant. The attempt to create community, according to Hunter, is shown in the creation of a citizen's association, which was set up to deal with air and noise pollution from a nearby airport and to prevent "block busting." One of its main functions has been to attempt to attract white homeowners so as to maintain racial balance. Members tend to be older, more highly educated homeowners who have recently settled in the area. In brief, this appears to be a special interest organization intended to protect real estate investments. The fact that it sponsors occasional political rallies, fairs and celebra-

tions does not make it a community association. Special interest groups such as this, which exist in many cities, are unlikely to provide the multi-interest linkages and coordination necessary for community. Nor can a racial distribution be accepted as a measure of integration.

Community in the full sense that we should define it is, of course, rare in a metropolitan society; and Hunter's finding that inner-city residents still engage in friendly interaction and have a sense of identity with their neighborhood is evidence of persistence of a quasi-communal aspect of modern life. Perhaps that is all that is to be left of community in America—friendliness and awareness of living near one another.

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REPLY TO LULOFF AND WILKINSON

I appreciate the comments of Luloff and Wilkinson as an opportunity to clarify a number of misconceptions—not only about my research, but about the concept of community itself. I think the major difficulty the authors have with my research is that they have attempted to define away what I consider to be an important empirical problem for urban sociology.

The authors begin by giving a "normative" definition of how community should be defined: "A community should be capable of meeting the full range of fundamental human needs" and, more explicitly about my research, they say "the relatively complete local society at both times was the larger metropolitan field." This is a holistic conception of community which, restated in the terms of my research, would say that all three dimensions of community (ecological, social and cultural) should be coterminous at the same scale.

However, precisely what I did was to treat each of these dimensions separately to test empirically whether or not, in fact, they were coterminous and to assess differences in the changes in these dimensions over time. The data show that the functional ecological community increasingly was operating at the expanded scale of the metropolitan field. The authors accept this finding but, with respect to the findings for the social and cultural dimensions, they begin to question the research's operational adequacy. "That urban people interact casually with their neighbors and identify with their area of residence," they say, is "not surprising." They add that these findings are an "exaggeration" of the meaning of "patterned social interaction" and "collective identity." First, I believe the findings were surprising in the persistence of community in the light of hypotheses to the contrary. Second, if these are not "patterned social interaction" and "collective identity," what are they? Where the authors err is in inferring that these are the *only* units of patterned social interaction and collective identity which these urban residents maintain. Nowhere do I imply this. It appears that the authors accept the findings of the ecological functional dimension of community because these fit their preconceptions of how community should be defined, but they do not accept the findings for the social and cultural dimensions because these findings do not fit their preconceptions.

Instead of accepting these findings, the authors attempt to define them away by maintaining that the local area I studied was a neighborhood, not a community. Elsewhere (Hunter, 1974; Hunter and Suttles, 1972), I have attempted to develop a less simplistic view of urban communities as a single-scaled phenomenon. A more inductive approach shows that urban residents live in a number of different scales of local communities ranging from the social block to the neighborhood, the local community and the expanded community of the urban region. Furthermore, different functions, activities and sentiments of residents are seen to vary systematically from level to level. In the modern metropolis, people simply do not live in a single community; but, rather, function in what I call a symbolic system of hierarchical communities. An individual's local communities will vary, not only throughout the life-span but even in the course of meeting his day-to-day life needs. I believe this multiple conception of community is closer to Robert MacIver's than is the single-scale, holistic conception the authors attribute to him. As MacIver (1970:30) says, "It will be seen that a community may be part of a wider

community, and that all community is a question of degree. . . . The one extreme is the whole world of men, one great but vague and incoherent common life. The other extreme is the small intense community within which the life of an ordinary individual is lived, a tiny nucleus of common life with a sometimes larger, sometimes smaller, and always varying fringe."

The authors also maintain that the community association in this area "*appears to be a special interest organization intended to protect real estate investments,*" and that "*special interest groups, such as this, are unlikely to provide multi-interest linkages and coordination necessary for community*" (italics added). However, as I point out in the article, this is but one of the association's important interests. The authors appear to dismiss the other activities as insignificant and irrelevant and, again by a selective reading and redefinition, this association becomes a special interest group. Nowhere do I maintain that the association is the community as the authors would imply; but, even by the authors' own criteria, it is a community association. It does provide multi-interest linkages and coordination within the community and, furthermore, it is a powerful political voice in representing the community across a diversity of interests with agents external to the community.

The most bald example of the authors' refusal to accept the findings that elements of community exist at the local scale is the opening line to their last paragraph where again they assert, by definition, that the problem does not exist: "Community in the full sense that we *should* define it is, *of course*, rare in a metropolitan society . . ." (italics added).

In concluding, the authors begrudgingly admit that perhaps I have documented some "quasi-communal" aspect of modern urban life. I think that "quasi-communal" has less to offer as a theoretical construct that may be linked to research than do the three dimensions of community defined for study in my article.

In their last sentence, the authors highlight what I think are also two prevalent misconceptions in the study of urban communities. They say: "Perhaps that is all that is to be left of community." This implies first, that contem-

porary communal forms are simply "residues" and second, that these residues are relatively insignificant. I would suggest that the significance or insignificance of these communal forms and their intersection with other spheres of modern life should be empirically assessed and not simply hypothesized into oblivion (Kornblum, 1974). Also, I would suggest that community is not simply a "residue" but, rather, that it is an emergent, socially-constructed reality (Suttles, 1972). I think it would be better for us to launch a series of empirical studies of the variety of communal forms and the processes of community formation and change rather than to debate the definition of community and then bemoan its predefined demise.

Finally, I would like to suggest that the authors consider more seriously the dynamics of community which were a major concern of my article. Their critique is couched in a static conception of community, while most propositions within urban sociology imply an inter-linked set of historical transformations of local communities and urban life. I believe the data and resources are available and all that is required is a clear and concerted effort by researchers to explore these transformations.

Albert Hunter
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ITEMS (Continued)

Clark and H. Thierry) of *Some European Contributions to Organizational Change and Development* (in preparation).

■ **LEONARD I. PEARLIN** (Marital Status, Life-Strains and Depression) is a researcher for the National Institute of Mental Health. He is engaged in a broad program of longitudinal research into social strain, coping behavior and psychological distress. **JOYCE S. JOHNSON** is a program analyst who was at the University of Maryland at the time of this work. Ms. Johnson is currently working on the construction and evaluation of health and safety standards in occupational environments.

■ **MICHAEL K. MOCH** (Organizational Innovations) is Assistant Professor in the Department of Business Administration, College of Commerce and Business Administration, University of Illinois, Urbana. He is currently investigating the impact of position in various networks (e.g., status, influence, interdependence, friendship) on individuals' attitudes at work. This is part of a larger effort to reconceptualize organizational structure to better understand how structure constrains and channels beliefs, values and behavior. **EDWARD V. MORSE** is Associate Professor of Sociology at Tulane University and Adjunct Associate Professor in the Tulane School of Public Health and Tropical Medicine. His current projects include the study of program adoption of health care techniques in Latin America, the application of career concepts to the sociology of adulthood and the study of career paths of academic scholars.

■ **LYNNE G. ZUCKER** (Institutionalization and Cultural Persistence) is Assistant Professor of Sociology at the University of California at Los Angeles. She is currently conducting a laboratory experiment which explores the relationship between labeling theory and the ethnomethodological theory of typification. She also is designing research to explore the impact of formal organizations on other social institutions and relationships in non-organizational settings. The basic theoretical approach is developed specifically to apply to social policy and evaluation problems, such as occupational mobility in manpower training programs. She is coauthor (with Howard E. Freeman and Wyatt Jones) of the forthcoming *Social Problems: Causes and Controls* (3rd edition).

■ **ILENE NAGEL BERNSTEIN** (Societal Reaction to Deviants) is Assistant Professor at Indiana University where she has a joint appointment in the Department of Sociology and Indiana University Law School. She is continuing her research on the antecedents of criminal justice processing decisions for defendants in metropolitan state courts and working (with John Hagan) on a study of the social organization of criminal justice processing in federal courts across the U.S. Her most recent book (with Howard E. Freeman) is *Academic and Entrepreneurial Research: The Consequences of Diversity in Federal Evaluation Studies* (Sage, 1975). **WILLIAM R. KELLY** is a Ph.D. candidate in the Department of Sociology, Indiana University. His present research includes a cross-national analysis of fertility differ-

entials, and study of theoretical and empirical relations between violence and social change. **PATRICIA A. DOYLE** is a Ph.D. candidate in the Department of Sociology, Indiana University. Her current research addresses questions of theoretical and empirical relations between work behavior and sex role orientations.

■ **MARK ABRAHAMSON** (Patterns of Urban Dominance) is Professor and Head of the Department of Sociology at the University of Connecticut, Storrs. He is presently studying patterns of stratification in professional sports. His most recent book is *Functionalism* (Prentice-Hall, in press). **MICHAEL A. DUBICK** is Assistant Professor of Sociology at the University of Minnesota. He is examining the organization of the news media and how it is affected by the social organization of the city in which they are located.

■ **ROSS M. STOLZENBERG** (Fertility Expectations and Employment Plans) is Assistant Professor of Sociology at the University of Illinois, Urbana. His current research includes the study of structural features of occupations, labor markets and industries and their consequences for the socioeconomic achievement of individuals. **LINDA J. WAITE** is Assistant Professor of Sociology at the University of Illinois, Urbana. Her current research includes an analysis of the consequences, for a woman, of an early first birth. This study, which focuses on the impact of teen-age childbearing on a woman's educational attainment, labor market behavior and rewards, as well as marital stability and later childbearing, is being done at the Urban Institute with Kristin A. Moore. In addition, she is studying the development of career commitment and its consequences among young women.

■ **DAVID F. SLY** (Ecological Approach to Migration) is Associate Professor at the Center for the Study of Population, Institute for Social Research, Florida State University, Tallahassee. His current studies include how rural development programs affect patterns of urbanization, how variations in the ecological structure of metropolitan areas, cities, and rings affect patterns of growth (with Robert Weller), how population redistribution affects the personal energy consumption sector (with Wilbur Zelinsky), how population loss to metropolitan areas is related to the theory of ecological expansion, and the relations between food production, food availability and urbanization. His most recent book (with Sidney Goldstein) is *Patterns of Urbanization: Comparative Country Studies* (Dolhain, Belgium: Ordina Editions, 1976). **JEFF TAYMAN** is a Ph.D. candidate in the Department of Sociology, Florida State University, Tallahassee. His current research concerns the effects of the growth and expansion of metropolitan areas on commuting patterns and the effects of family health care programs on fertility.

■ **JACQUES DELACROIX** (Export of Raw Materials and Economic Growth) is Assistant Professor of Sociology at Indiana University, Bloomington. This paper is part of an ongoing project of quantitative assessment of diverse aspects of the sociology of economic development.

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THE MALTREATED CHILD: The Maltreatment Syndrome in Children — A Medical, Legal and Social Guide (3rd Ed.) by Vincent J. Fontana, *New York Foundling Hospital, New York City*, and Douglas J. Besharov, *United States National Center of Child Abuse and Neglect, Washington, D. C.* Foreword by Mother Loretto Bernard. The Third Edition of this vital text presents the problems of child maltreatment; their nature, causes and extent; and the means to deal with them. Among the text's outstanding features are a statistical outline of child abuse, a discussion of social and medical manifestations and preventive measures, and a review of methods of differential diagnosis. '77, 176 pp., 11 il., \$12.50

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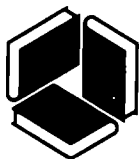
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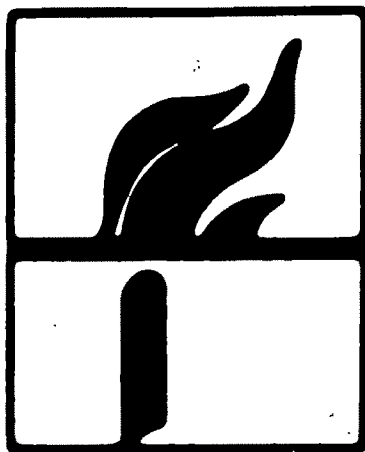
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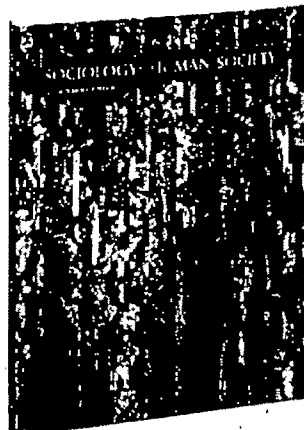
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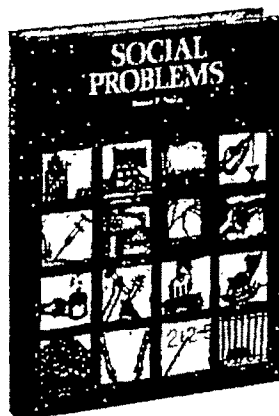
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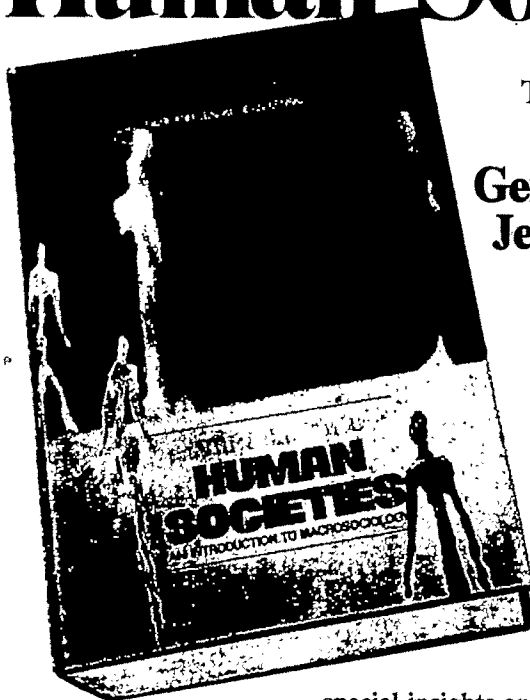
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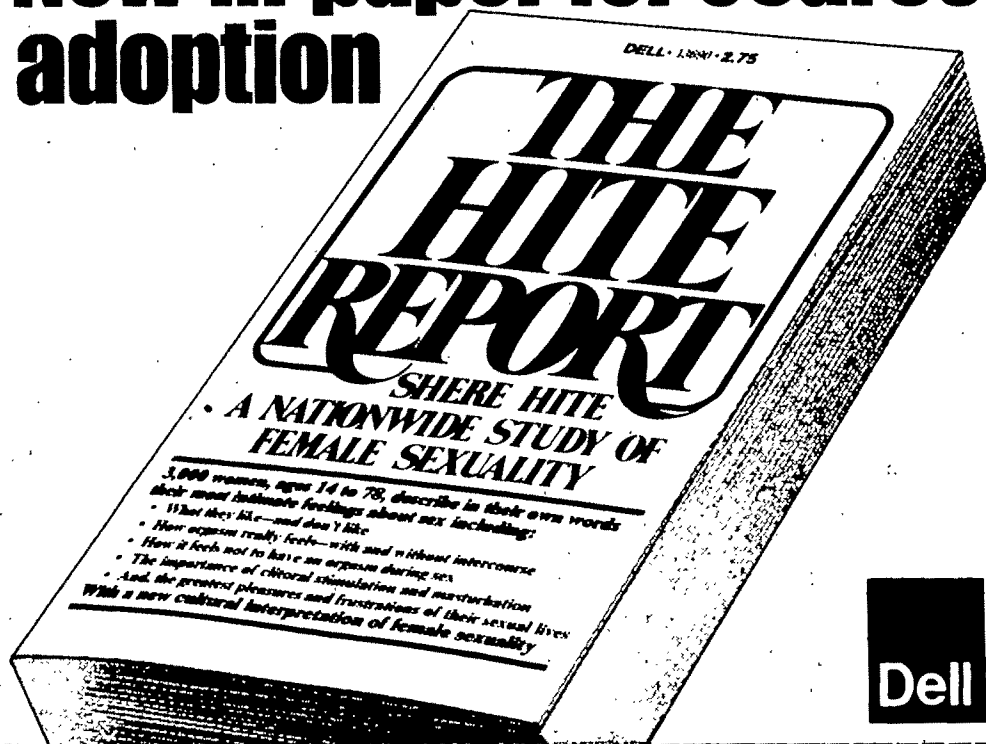
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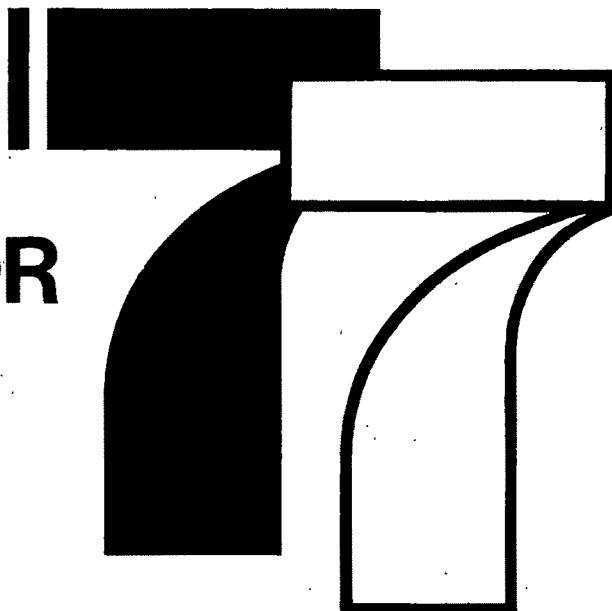
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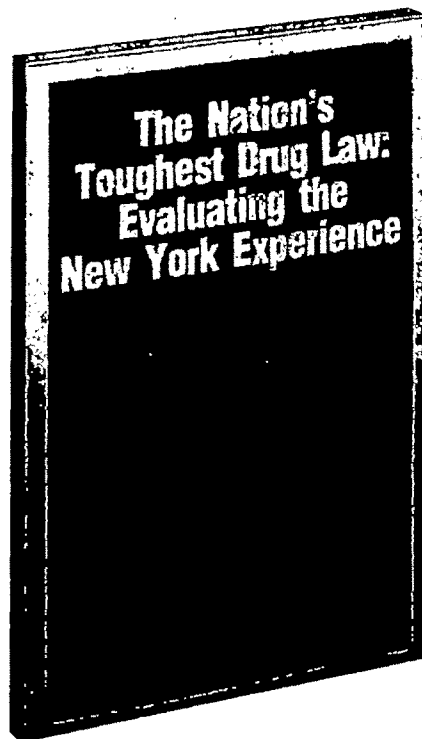
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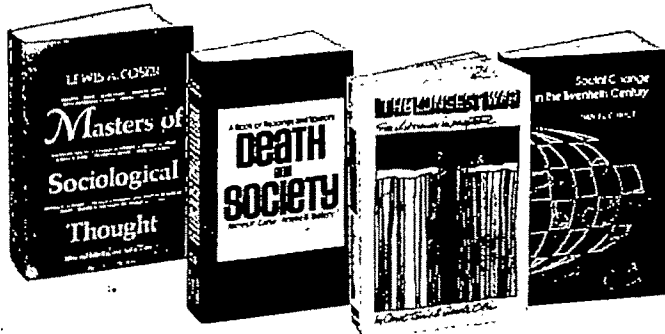
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Beginning January, 1978 a processing fee of \$10 will be required for each paper submitted; such fees to be waived for student members of ASA. This reflects a new policy of the ASA Council and the Committee on Publications affecting all ASA journals. It is a reluctant response to the rapidly accelerating costs of manuscript reviewing. A check or money order, made payable to the American Sociological Association, should accompany each submission. This fee must be paid in order for the review process to begin.

ITEMS

December, 1977

This is my last issue. Rita Simon will become editor of the ASR with the February issue. I owe thanks to four groups of people who were crucial to the quality of the ASR in the last three years: first, to the deputy editors of the ASR, Barry Edmonston, Mike Hannan and Barbara Rosenblum. I am not sure how far back the institution of the deputy editor goes, but as the volume of manuscripts increases so

does the importance of the deputy editor to the quality of the ASR. It is the deputy editors who assign most of the manuscripts submitted to referees. Hence, they are as important as the editor to the final content of the *Review*. I am not sure that the authors and readers of the ASR have quite grasped how crucial the deputy editors are; but I have, and I am

(Continued on page 987)

PRESIDENTIAL ADDRESS: COUNTERCULTURES AND SOCIAL CHANGE*

J. MILTON YINGER

Oberlin College

American Sociological Review 1977, Vol. 42 (December):833-853

It was a rather simple issue which I decided to examine on this occasion: to design as parsimonious a theoretical statement as possible regarding a set of problems with which I have been concerned for many years. The set, as it turns out, has several parts:

Religious sectarianism and such related concepts as charisma, heresy, antinomianism, gnosticism and revitalization.

Anomie and alienation—the causes and effects of normative voids and individuals' experiences thereof.

Freudian and other depth psychologies, with their conceptions of a characterological underworld, filled with impulses and tendencies in sharp contradiction with the conscious life of the ego and the normative order of the superego.

Socialization and social control—particularly the forces that interfere with socialization and increase the intensity of social control.

Deviation, its sources and outcomes; the inverted values of some criminals and delinquents.

Youth groups and age groups generally; generation conflicts and their causes.

Gemeinschaft und Gesellschaft; the mass society; the return to community in communes; utopias.

Collective behavior and its relationship to customary behavior; the influence of mass media.

Political sociology; protest movements; revolutions; modernization and counter-modernization.

Social stratification, discrimination, prejudice; and the forms of protest against them made by dominated groups.

Rites of rebellion and rituals of reversal in tribal societies; saturnalia, feasts of fools, and "Abbeys of Misrule" in feudal and modern societies.

The sociology of knowledge, of literature, art, and music. And, of course, in such a melange, social change.

Indeed, it was a rather simple issue—simple enough for a good question on a doctoral exam: "Read the literature dealing with the following thirteen topics and then write a paper showing how that literature converges on a common set of problems." I've been giving myself that exam for thirty years and have written rather extensive answers to some segments of it. But none of the answers grapples with this complex of issues as a whole. What one needs is a unifying theme, a concept that will focus attention on the analytic problems common to the segments.

To serve that purpose, I am working with the concept of counterculture, which I will define here—postponing a fuller definition briefly—as a set of norms and values of a group that sharply contradict the dominant norms and values of the society of which that group is a part. My aim here is to examine a few of these topics to discover what they may contribute, when studied by use of the concept of counterculture, to our understanding of society. This is certainly a paradoxical way to approach problems of sociological theory. The central questions have always been: How can we account for social order? And how is change from one system

*I am deeply grateful to the National Endowment for the Humanities for a Fellowship for Independent Study and Research, to Oberlin College for a sabbatical leave, and to Clare Hall, Cambridge University for appointment as Visiting Fellow.

of order to another brought about? I am proposing that we approach these questions in a different way, that we look for explanations of disorder in the hope that we can increase our understanding of the basic problems of sociology by seeing them in a new light. This light will also cast its shadows, of course, and leave dark places; so it is best used in conjunction with, or in alternation with, other perspectives. It may be well, therefore, to glance at the basic questions in the more usual light before seeing them counterculturally.

Social theorists have developed, of course, three partially competing theories of social order:

It is a product of a mutually shared normative system—a blueprint for action that has been internalized by a set of persons in interaction.

It is a product of reciprocity and exchange, of perceived mutual advantages.

It is a consequence of the power of some to command the behavior of others—the reciprocal of power being the fear of sanctions, of loss or pain or death.

Statements such as these are best seen as analytic. A given interaction, a specific social situation is likely to be a product of all three factors, although one may predominate. Norms may be exploited for power purposes; exchange arrangements may get "frozen" into culture; power long exercised may take on the trappings of authority—a cultural concept.

Quarrels among theorists over the "true" or "basic" source of order seem pointless. The task is to measure the range of empirical mixtures of norms, power, and reciprocity and then to explore the conditions under which these mixtures occur. The mixtures undergo continual change, hence the study of social order must at the same time be a study of social change. Any one of the three sources of order—or more commonly some combination—may be involved in the process of change: Normative agreement may break down; power balances, or more precisely stable power imbalances, may shift; individuals and groups may gain or lose in what they have to offer in exchange. Under these conditions, ex-

pected satisfactions are unfulfilled and newly envisaged satisfactions are kept tantalizingly out of reach.

This is where countercultures come in. Some individuals and groups feel particularly strongly that the social order is unable to bring them the accustomed or the hoped-for satisfactions. Depending on their social location and on their personal tendencies, they attack, strongly or weakly, violently or symbolically, the frustrating social order—that is, the normative-power-reciprocity system. The nature of the attack varies widely, with some believing that they have been caught in very bad bargains, others that they are being exploited by unjust and unwise leaders or rulers, and still others emphasizing that they are surrounded by a shoddy system of norms and values. All three elements are found in most protest movements, even though they can be distinguished analytically. Giving the terms sharper and more limited meanings than they ordinarily carry, we can say that reform movements are efforts to change the social bargains—the exchange rates; rebellions are attempts to change the rulers and the bases of power; and countercultural movements are attempts drastically to reorganize the normative bases of order. Revolutions, which are rare and usually require several decades, include all three.

Our concern here is with normative systems in sharp opposition to the prevailing culture, and with the groups and individuals who are proponents and carriers of the oppositional culture.¹ Attention to social organization and social structure must be complemented, we have come to agree, by attention to the fact that conflict is

¹ It has become nearly standard to use the term "counterculture" to refer both to the norms and values and to the groups with which they are identified, after the fashion of most anthropological uses of "culture." This is not my preference. I think it blurs distinctions that are often essential to clarity. Yet I do not want to carry the burden of another neologism—countergroup or countersociety—to serve as a parallel concept. Therefore, I will use various circumlocutions to make the distinction between culture and society, between normative systems and the groups that carry them; and on occasion will refer to both by "counterculture" when I think the context prevents misunderstanding.

endemic, presenting a need for synthesis. Similarly, the emphasis on normative integration, on culture as a governing blueprint, must be modified by *continuous* attention to countercultures, while working toward synthesis. I am suggesting not simply a philosophical dialectic, based on the belief that every theoretical argument is self-generating, since every thesis contains the germs of its own contradiction, but a cultural dialectic: every normative system contains the seeds of its own contradiction. This is not propounded as a truth, of course, but as a point of departure, a fruitful way to begin the study of societies.

In each of the thirteen topics mentioned at the start, the cultural-countercultural dialectic has appeared; only by dealing with it will we be able to resolve pressing problems of analysis and interpretation. Ideological as well as evidential factors have tended to make it difficult to attain a perspective that continuously and simultaneously examines structure and change, culture and counterculture. That we must strive to do.

In the current emphasis on power and exchange, contemporary sociology is largely unaware of the deeply nonrational forces at work in all societies, forces that are built around symbol, ritual, and myth. Most of us are ready to recognize the unconscious and nonrational aspects of individual life, but pay too little attention to the counterpart on the societal level—the shared myths and rituals by which we collectively strive to avert crises or deal with them if they come. We can think of culture on its most abstract and mythical level as a paradigm that selects, interprets, and powerfully affects our impressions and feelings and desires. When culture begins to leave many questions unanswered and many needs unfilled, when individuals suspect their own emotions and experience only a blurred identity, the cultural system may be pushed aside (Tiryakian, 1974:1–15). Periods of cultural crisis (a potential at any time), of anomie, are not simply periods of loss of faith, but of struggle toward some new way to deal with the threat or reality of crisis and chaos.

THE DEFINITION OF COUNTERCULTURE

In the seventeen years since I proposed the term, several explicit and dozens of implicit definitions of counterculture have been offered.² We can begin to sketch out the parameters of the concept by examining some of these definitions.

There are fundamentally two ways of defining what a counterculture is, the first on an ideological level and the second on a behavioral level. On the ideological level, a counterculture is a set of beliefs and values which radically reject the dominant culture of a society and prescribe a sectarian alternative. On the behavioral level, a counterculture is a group of people who, because they accept such beliefs and values, behave in such radically nonconformist ways that they tend to drop out of the society. (Westhues, 1972:9–10)

The statement that countercultural groups tend to drop out of society is problematic, as Westhues recognizes. Some do; others stay engaged, hoping to change society and its values; others look inward, searching for their souls, but not leaving society.

Some definitions proceed mainly by illustration. Contrast minor variations on a cultural theme, Fred Davis suggests, with sharp variations.

The gang boy configuration . . . fits nicely Yinger's notion of a contraculture; its very meaning and existential quality inhere in its members' patterned deviation from the dominant American cultural pattern. . . . hippies, too, are an instance *par excellence* of a contraculture whose *raison d'être* . . . lies in its members' almost studied inversion of certain key middle class American values and practices. (F. Davis, 1971:4)

Davis then spells out these contradictions of value and practice in some detail:

immediacy contra past preoccupation and future concern; the *natural* . . . contra the

² I preferred and used the Latin prefix, hence contraculture. But the voice of the people has spoken, and the usual spelling, by about three to one, is now counterculture. To my ear, contraculture is more mellifluous. I also wanted to avoid suggesting a close parallel with counter-revolution and counter-reformation, with their rather specific connotations of returning to an earlier situation. Yet, like Mark Twain, I have no sympathy for those ignorant people who know only one way to spell a word.

artificial . . . ; the *colorful* and *baroque* contra the classical, contained, and symmetrical; the *direct* contra the mediated, interposed, or intervening . . . ; the *spontaneous* contra the structured; the *primitive* contra the sophisticated; the *mystical* contra the scientific; the *egalitarian* contra the hierarchical; the *polymorphous* and *androgynous* contra the singular; the *diffuse* contra the categorical; the *communal* contra the private. (F. Davis, 1971:14-5)

There is a tendency to stereotype both the dominant and the hippie standards in Davis' description, as in many discussions of countercultures, in order to draw the sharpest possible contrast; but he does capture the ideological tone of at least one variant of contemporary countercultures.

Some definitions proceed from an opposite perspective:

Today's pop counterculture, especially among the young, is an awesome mix of maximum mindlessness, minimum historical awareness, and a pathetic yearning for (to quote Chico Marx) strawberry shortcut. To hell with established religions, with science, with philosophy, with economics and politics, with the liberal arts—with anything that demands time and effort. Dig the rock beat, kink up your sex life, meditate, tack a photo of Squeaky Fromme on the wall. (Gardner, 1975:46)

Amid the abundance of such descriptions and definitions, pro and con, it is difficult to reestablish the concept of counterculture as a useful part of a scientific vocabulary. Too often we illustrate the truth of Walter Kaufman's appraisal that as it becomes increasingly difficult to keep up with events in a field of study, many people feel a need for "bargain words that cost little or no study and can be used in a great variety of contexts with an air of expertise" (quoted in Schacht, 1971:xlix).

Any analysis of countercultures faces this problem. Some use it as a word of opprobrium, an indication of incivility, depravity, heresy, or sedition. For others, counterculture means hope and salvation, a unique and perhaps final opportunity to get humankind off the road to destruction. Some devotees with whom I have talked are offended when I suggest that countercultures have occurred in many times and places. Their response is a "religious" ob-

jection to any doubt cast on the uniqueness of their experience. They are implicitly supported by the host of writers who, in recent years, have discussed "the" counterculture. Others are aware of the frequency with which countercultures have appeared, but not wanting to reduce the enthusiasm of current participants, play down the historical parallels and emphasize the uniqueness of contemporary oppositional movements.

Certainly every counterculture has unique elements that for some purposes are appropriately the focus of attention. However, I shall be climbing up the abstraction ladder—often, it may appear to you to dizzying heights—to see the similarities. The term counterculture is appropriately used "whenever the normative system of a group contains, as a primary element, a theme of conflict with the values of the total society, where personality variables are directly involved in the development and maintenance of the group's values, and wherever its norms can be understood only by reference to the relationship of the group to a surrounding dominant culture" (Yinger, 1960:629).

This definition leaves several questions unanswered: Should we speak of countercultures when second level or subterranean values of a society are raised by some segment of that society to a primary place? The apocalyptic visions, populism, and evangelical fervor of American student radicals, Matza (1961) notes, are part of the dominant tradition. When these "counterthemes" are carried to an extreme, however, they are "publicly denounced." Delinquent youth can also draw on a subterranean tradition of the dominant society. The search for a thrill, the use of "pull," and aggression are scarcely limited to delinquents; they are secondary values of the dominant society (Matza and Sykes, 1961). At some point, an exaggerated emphasis on a value becomes, a counter-value by virtue of the exaggeration. We are dealing with a variable, and only careful study can tell us at what point an exaggerated value becomes countercultural, as indicated by the consequences of its use.

How is deviant behavior distinguished

from countercultural behavior? The latter is only one form of deviation, nonconformist and not aberrant, in Merton's terms (Merton and Nisbet, 1966:ch. 15). It is also supported by the norms and values of a group. When counterculture is used to refer to deviation, whether or not it is nonconformist and group supported, we are prevented from distinguishing behaviors which, although sometimes similar on the surface, are different in causes and consequences.

Is not the whole concept of counterculture riddled with class and ethnic bias in pluralistic societies? There is a growing tendency to view urban societies as so diverse culturally that each segment is, at the most, countercultural to the other segments. Empirical study tends to support the view, however, that most youths as well as adults hold to the values of the larger society. From their study of Danish and American high schools, Kandel and Lesser (1972:168) conclude that "far from developing a contra-culture in opposition to that of adult society, the adolescents we have studied express the values of adult society." A recent report on Britain's Sixteen-Year-Olds (National Children's Bureau, 1976) found that most of a sample of 16,000 were thoroughly traditional. Studies by Yankelovich (1974) and Wattenberg (1974) show wider deviations, especially among college youth; but the total impression is one of quite close identification with the values of society.

The degree of cultural diversity is not an either-or question, however; and countercultural theory must deal with the range of social situations, from those where value coherence and agreement are high to those where the common core is small (see Williams, 1970:ch. 10). *To the degree that* the total society has a shared culture, there can be reciprocal countercultures. And each subsociety can also have countercultures. Punk rock is not just a blue-collar deviation from upper-class music: it also drastically contradicts the musical and other values of the working class from which it springs.

How counter must a statement or action be (assuming it to be expressive of some norm or value of a group) before we shall consider it countercultural? In the most

limiting sense, we might say it has to be specifically a *reversal* of the established value. Note the use of such terms as "polarity," "reversal," "inversion" in the definitions I have cited. Berger and Berger (1971:20) speak of "diametric opposition." Indeed, these ideas are central to the original concept of counterculture (Yinger, 1960).

We have no way of saying with certainty, however, that sharply oppositional statements and actions are 180 degrees different from prevailing values. We have only the beginnings of a sociometry of dominant values (as in the work of Rokeach, 1973; Yankelovich, 1974; Williams, 1970; McCready and Greeley, 1976), and of measurement and scaling of countercultures (Musgrove, 1974; Wuthnow, 1976b; Wieder and Zimmerman, 1974; Spates, 1976; Spates and Levin, 1972; Wattenberg, 1974; Starr, 1974).

VARIETIES OF COUNTERCULTURES

One might organize material for a study of countercultures in various ways. The contrasting values and norms could be examined institutionally, with the orthodox or dominant patterns of economics, politics, education, family, and religion set against those of oppositional groups (see Wuthnow, 1976b: ch. 1; Gerlach and Hine, 1973: ch. 15). Or we could build on an analysis of human behavior that has proved to be useful in several other fields. Years ago Karen Horney (1937) noted that neurotics tend to struggle with their anxieties in three different ways: by attack, by withdrawal, or by a search for shelter and protection. Charles S. Johnson (1943) skillfully described the ways in which minority-group members dealt with discrimination by aggression, avoidance, or acceptance. Turner and Kilian (1957) develop a highly analogous interpretation of power-oriented, value-oriented, and participation-oriented social movements. There is a further close parallel in Weber's (1963) description of prophetic, ascetic, and mystical sects.

I shall not develop this valuable theoretical convergence, more than to say that countercultural groups quite clearly fall

into the same threefold pattern, not the twofold activist-withdrawal categories ordinarily used. The closest comparison is with types of sects. The radical activist counterculturalist is the prophet who "preaches, creates, or demands *new* obligations," to use Weber's words. In typological terms, the communitarian utopian is the ascetic who withdraws into a separated community where the new values can be lived out with minimum hindrance from an evil society. Neither of these descriptions fits the mystics searching for the truth and for themselves. Realization of their values requires, in their view, that they turn inward. They do not so much attack society as disregard it, insofar as they can, and float above it in search of enlightenment. "The enemy is within each of us," says Charles Reich (1970:356-7).

Although every countercultural group tends to be a mixture, the strains and the splits they experience often result from sharply contrasting views of the best way to realize their oppositional values. When at the 1969 Woodstock festival, for example, Abbie Hoffman tried to generate political action by saying that the festival was meaningless until a radical "rotting in jail" was freed, a member of The Who rock band, in the words of *Rolling Stone*, "clubbed Hoffman off the stage with his guitar" (Denisoff and Levine, 1970), a wonderfully symbolic way to express the conflict. From the perspective of prophetic counterculturalists, the tendency of many participants in the "New Left" to define political problems in terms of personal issues, often in the language of alienation, seems a terrible deflection from the basic goal of achieving a society based on new values (Lasch, 1969; Clecak, 1973). In theory, if not always in their own ideologically guided behavior, they share Weber's (1946:126) view that "he who seeks the salvation of the soul, of his own and others, should not seek it along the avenue of politics."

A group may seek to mix the mystical-experiential and the ascetic-communal countercultural modes, until it is clear that the search for wisdom and the ultimate trip doesn't get the corn planted or the groceries bought, whereupon the group

breaks up, or some members leave for a setting more in keeping with their inclinations. In recent years, persons who had sought salvation through drugs and unrestrained sex have sometimes turned to ascetic and highly restrained religious groups—the Divine Light Mission or Hare Krishna or Meher Baba (Kelley, 1973; Judah, 1974; Zaretsky and Leone, 1974; Glock and Bellah, 1976; Robbins and Anthony, 1972).

Other than this brief reference, I will not follow the institutional or what can be called the sectarian way of organizing the discussion of oppositional cultural movements. We can develop more meaningful comparisons and more useful general principles by examining the following questions.

Are there drastic shifts in the criteria and the methods by which a group claims to *know* what is true, good and beautiful?

Are there drastic shifts in the standards of what are *held to be* true, good and beautiful?

In the traditional categories of philosophy, we can define countercultures in terms of their epistemologies, their ethics, and their aesthetics. The events we study will almost always combine various elements; but we can understand the blends better if we keep in mind the elements out of which they are built.

(1) Truth, today's counterculturalists declare, is not attained by arid research but by mystical insight. It is found in populist, homespun wisdom, in anti-universities, in direct experience with the cosmos, in meditation, in chants, in drugs, in sensory deprivation, in sensitivity to the messages of the intuitive right hemisphere of the brain—all this set over against science, technology, the knowledge of the expert, and cold rationality.

Attacks on the intellectual establishment are scarcely new, of course. The Taborites denounced the masters of Prague University in the fifteenth century. In England, during the period preceeding the Civil War, sectarians called for discussion after sermons (a practice adopted for a brief period in Boston). Itinerant interrupters, professionally skilled hecklers, moved among the churches, which were indissolubly linked with the universities,

and despite legal difficulties, denounced the self-righteousness of the pastors and their greed in taking tithes (Hill, 1975:105-6). Truth, they declared, was not the monopoly of the clergy, but could be given to anyone through an inner light. It would require little shifting of terms to use this set of activities and claims to describe a free university in London or Berkeley in the 1960s.

Many persons share the distress that underlies these views, a distress that led H. G. Wells to say at the end of World War II, that "mind is at the end of its tether." Today we see even more clearly that some of our deepest problems come from application of scientific findings. The countercultural epistemology appears in response to this predicament. Pursue a sense of mystery and fantasy, embrace the occult, meditate, unlock the truth that is in you (Brown, 1966; Roszak, 1973; A. Smith, 1976).

Belief in the occult and the pursuit of understanding through mystery are, of course, persistent elements in human history. But their strength alternates, with waves of mysticism occurring when a society has experienced, as Tiryakian has put it, "a loss of confidence in established symbols and cognitive models of reality. . . . Occult practices are appealing, among other reasons, because they are seemingly dramatic opposites of empirical practices of science and of the depersonalization of the industrial order" (Tiryakian, 1972: 510, 494; see also Gellner, 1974; Wuthnow, 1976a).

The way paved for the countercultural epistemology of our time by the long tradition of unmasking in European and American thought (along with other sources). Do not trust appearances or presumed objective truth, for there are deeper realities, say Hume, Marx, Nietzsche, Freud, Pareto, and many others. Truth requires that we bring these deeper realities to light (See Mannheim, 1936; Merton, 1968; Ellenberger, 1970). If this unmasking laid the groundwork for the sociology and psychology of knowledge in the thought of some persons, it supported the search for truth through mysticism and the occult among others for whom science itself has been unmasked.

(My statement itself, of course, is an hypothesis in the sociology of knowledge.)

(2) Countercultural ethics contradict the values of the dominant society as sharply as countercultural epistemology. Proponents of one, however, are often not proponents of the other. Those who oppose the established ways to truth may be quite conservative in their definitions of the good life; while those who condemn the morality of the establishment may be quite comfortable with its epistemology. The search for a "new head" has much more often been associated with quietism, introversion, and withdrawal from an evil world than with efforts to change it. These tendencies are not characteristic of those I call ethical counterculturalists. They challenge the world with new visions of the good life.

To the seventeenth-century English Ranters, the prevailing moral law was not binding on the true believers. "I know nothing unclean to me," Clarkson wrote, "and therefore what Act soever I do, is acted by that Majesty in me" (quoted in Cohn, 1970:312). Do your own thing is a somewhat less elegant way of putting it. Many Quakers and Ranters went naked through the streets and into churches—no segregated nudist beach for them (Cohn, 1970; Hill, 1975). Abiezer Coppe and his more effervescent followers were a free speech movement, using obscenity as a weapon; they affirmed the rightness of sex before marriage and attacked the monogamous family; they supported the use of drugs to heighten spiritual vision (though it was alcohol and the new drug tobacco); they dismissed the prevailing doctrines of the church and its structure of authority; they rejected private property in favor of communism.

The Ranter ethic, as preached by Coppe and Clarkson, involved a real subversion of existing society and its values. . . . all that matters is here and now. . . . Nothing is evil that does not harm our fellow men. . . . "Swearing i'th [giveth] light, gloriously," and "wanton kisses" may help to liberate us from the repressive ethic which our masters are trying to impose on us. (Hill, 1975:339)

To the contemporary counterculturalists, what feels good is good. This sometimes collides with another principle:

if it is good to the establishment, it is bad for us. Since the establishment takes a somewhat ambiguous middle position with regard, for example, to aggression and sex, countercultural groups have to push away from the center, toward pacifism and celibacy or toward violence and sexual exuberance, meanwhile claiming that the establishment is not in the center, but actually occupies the opposite pole from the one being held up as good. Counterculturalists do not escape the common human tendency to make their enemies, as well as their leaders, into what they need them to be—need them in an effort to justify their actions and handle their ambivalences.

(3) A culture is as fully defined by its aesthetic standards as by its epistemology and its ethics. In many ways the opposition of artists has been negative, in the sense that they have held societies and their cultures up to fundamental criticism more than they have affirmed the values of a counterculture. Yet some oppositional art by its very creation defines an aesthetic counterculture, by using forms and sounds and ideas that formerly were taboo. As culture rather than as criticism, the contemporary counterculture scarcely runs the gamut. Its range is more nearly from yoga to yogurt to zen. The appeal to chaos and dissonance and the overwhelming of the senses correspond with many experiences of our time; but when that point has been made, we may begin to demand of our artists that they help us find—help us experience—some new sense of order and consonance.

I have said, help us experience, because it is not art as argument, but art as experience that more strongly carries the countercultural (or the cultural) impact. "The world doesn't fear a new idea," D. H. Lawrence wrote. "It can pigeonhole any idea. But it can't pigeonhole a new experience" (quoted by Trilling, 1966:xvii). In a society whose major values are intact, the artistic experience, though filled with turmoil and agony, becomes, in the last analysis, an affirmation of those values. In a society torn by doubt, much of the art—often including some of the best—will take us into a different world. The

pilgrim in Dante's *Divine Comedy* moved from low to high, into a kind of rebirth. Samuel Beckett has his heroes, if that is the word, still *Waiting for Godot* as the curtain falls. The protagonist of Kafka's fable is no nearer *The Castle* at the end of his journey than at the beginning.

As experience, many novels and plays in the "theatre of the absurd," using that term in a broad sense, are countercultural comments on the world (see Esslin, 1973, for a more restricted usage). Even this absurdity that we dramatize, they say, makes sense by comparison with our sorry state. Let a brief comment on Jean Giraudoux's *The Madwoman of Chaillot* stand for a variety of plays and novels. To understand the world they affirm, we shall have to turn our usual perceptions upside down. With a wonderfully comic touch, Giraudoux holds the rich, the power hungry, the presumably successful up to ridicule. The industrialist, the general, the broker, the southern fundamentalist minister, and the commissar are seen as truly mad. They are about to destroy Paris and the world as they seek to get the oil they believe is under the city, while the "madwoman" with her eccentric friends recognize the absurdity in the powerful and try to stop them.

I shall only point to some of the other forms of aesthetic counterculture—those drastic efforts to redefine the beautiful, to break away from old standards that seem not only to inhibit the imagination but to confirm the whole culture within which they are embedded. Some of these innovations, by their artistic merit and the timeliness of their appearance, change the culture and become part of new aesthetic traditions, whether classical or popular. Other innovations fade rapidly, particularly if they are to an important degree a vehicle for carrying other kinds of protest than the aesthetic one.

Most great musicians push back the boundaries of the received tradition and are sometimes regarded as countercultural by defenders of the prevailing classicism. Though counterpoint has long been regarded simply as one aspect of harmony, when it first appeared in the late medieval period, with its polyphonic styles, its in-

dependent rhythms and distinctive sets of sounds, it was an affront to those who knew how music "should" sound. Even some of Bach's contrapuntal forms—turning a melody upside down or running it backward—seemed harsh to some of his contemporaries. But it was inevitable—if I can offer a sweeping and quite unsupported hypothesis in the sociology of music—that the growing complexity of society should be expressed in the growing complexity of music.

To leap into our own century, Schoenberg and Stravinsky and many others turned against the romantic tradition, as many of their literary contemporaries did, to explore the farthest possibilities of dissonance—one is tempted to say in a world filled with crashing sounds and disharmonies.

Seen against the history of music, it comes as no surprise that popular as well as classical forms can drastically challenge prevailing standards. The contemporary counterculture in music, to be sure, seems to bring a much more powerful dissent, both in the sharpness of its reversal of standards and the breadth of its influence. The enormous power of the electronic media to give voice to new sounds, the economic resources to produce them and listen to them, the depth of the disenchantment and its expression in so many other ways, all support the musical protest of our time. For some, of course, rock music is entertainment, fad, and declaration of independence; or it is a source of great profit (Eisen, 1969:xiii). For others, however, it is the chief ritual of a new life; it is community; it is religion. The whole culture complex—the music, lyrics, volume, artifacts, audience, set, and setting—facilitates, as Harmon (1972:81) puts it, "an unprecedented questioning of basic cultural values and institutions." The "acid rock" groups have given coverage and confirmation to the use of drugs; the lyrics define a new morality ("All the things they said were wrong are what I want to be"); the festivals bring together a community that supports the new values; the music itself declares that the old harmonic and well-modulated sounds of the past are the sounds of a repressive society.

CANDIDATES FOR COUNTERCULTURES

Countercultures can best be identified, I have suggested, by studying movements based on epistemological, ethical and aesthetic standards sharply at odds with prevailing norms. Why are some people drawn to the new standards while others are repelled by them? A theory of countercultures must connect oppositional normative movements with individual personalities. Freud saw us all as good candidates for behavior that contravenes the norms because, as he put it, "what we call our civilization is largely responsible for our misery" (Freud, 1962:33) by blocking strongly motivated activities. Freud stated a principle that today we call hedonic relativism, a term similar to relative deprivation: "We are so made," he wrote, "that we can derive intense enjoyment only from a contrast and very little from a state of things. Thus our possibilities of happiness are already restricted by our constitution. Unhappiness is much less difficult to experience" (Freud, 1962:23-4). Although he does not locate the cause in the individual psyche, Durkheim had earlier made a similar point. One can only be disillusioned, he remarked, if one moves toward a point that "recedes in the same measure that one advances. . . . This is why historical periods like ours, which have known the malady of infinite aspiration, are necessarily touched with pessimism" (Durkheim, 1973:40).

We all know, from experience and experiment, how readily we accommodate to new standards of enjoyment, so that what seemed pleasant yesterday is now unacceptably bland. "Where pleasure is concerned, humans are insatiable animals, shifting their criterion level upward when the level of pleasurable input increases, so that once again experience is scored as one-third pleasure, one-third pain, and one-third blah" (Campbell, 1975:1121). We do not make equally quick adaptation to any downward pull of experience. There is nothing more painful, as Veblen noted, than a retreat from a standard of living.

Granted this principle, a society that enormously raises the aspirations but only

modestly raises the pleasures of its members, or some of its members, is furnishing an essential ingredient of protest. Even if there are gains in some objective sense, many will feel cheated by society and disenchanted with its values. These are perhaps the conditions under which a shift occurs, in Ralph Turner's (1976) terms, from institutional definitions of "the real self" toward impulse definitions. What he calls impulse selves also require social validation and support from others. What start out as impulse selves may be collectively validated as the partially institutionalized selves of a counterculture.

The criteria by which Turner defines the impulse self are similar to those used by Keniston and others to describe alienation (Keniston, 1965; Bloy, 1969). Living for today, rejection of "success," emphasis on "being" rather than on "doing," self-expression and the lowering of inhibitions are tendencies that Keniston (1965:81) sees as point-by-point denials of the established values and replacement by their opposites.

In addition to impulse self and alienated self, other terms can be used to help us examine the individual counterpart of countercultures. Adler (1972) discusses the antinomian personality, and Erik Erikson, using perhaps the most useful psychological analogue for counterculture, develops the term "negative identity." In an often desperate choice between being a failure, a nobody, an "invisible man," and being a shockingly visible anti-hero, a person to reckon with, some choose the latter, expressing their frustration "in a scornful and snobbish hostility toward the roles offered as proper and desirable in one's family or immediate community." It is not simply that standard roles are abrogated. "They choose instead a *negative identity*, i.e., an identity perversely based on all those identifications and roles which, at critical stages of development, had been presented to them as most undesirable or dangerous and yet also as most real" (Erikson, 1968:172-4).

Erikson tends to assume that a negative identity is pathological. It may be wiser to reserve one's judgment on that issue, asking in each instance what a person is being

negative against, what balance of creativity and inanity is found in the available positive identities, and what the consequences are for the individual and society in the various choices. Such self-feelings may be part of the character not only of the neurotic, but also of the prophet or the highly creative person who finds the preferred identities too closely bound to an unacceptable society. It is not only that individuals may be alienated, societies may be alienating. One psychiatrist, "when asked what he thought the best therapy was for students who had been severely alienated by the Vietnam war, replied, 'Stop the damn war!'" (Bloy, 1969:651).

Those driven to define themselves in terms that quite reverse the standards of their time and place are often torn by ambivalence (on ambivalence, see Merton and Barber, 1963; Room, 1976). Redl and Wineman note that although some delinquents so identify with criminal activity that they have few problems with guilt, "others are not quite that advanced. They still have mighty chunks of their value-identified superego intact. . . . In that case, the 'delinquent ego' has the additional task of 'duping its own superego,' so that delinquent impulsivity can be enjoyed tax-free from feelings of guilt." (Redl and Wineman, 1951:144).

Perhaps the counterpart of this is the need a participant in the dominant society—one who is also ambivalent about dominant values—has for duping his own id. The "long-haired, ne'er-do-well pot smokers" are the hidden and repressed parts of his own life—the dreams and fantasies of freedom from routine, inhibition, and the demands of work. By making advocates of such freedoms seem abhorrent, one can more easily resist temptation.

This interpretation ought not to be pushed too far in either direction, however. Genuine value conflicts exist; and the opponents are those who hold contrary values, not the hidden selves.

Because of their ambivalence, counterculturalists need opposition. In recent years, they have needed a "police state" and have helped to intensify already-existing tendencies in that direction

(Douglas, 1970:179). Harsh opposition helps to justify their feelings and actions and helps to push them away from the center, win attention, sympathy and converts, and draw sharper, value-protecting boundaries.

Although persons with feelings of negative identity are symptoms of and sources of countercultures, it is necessary to distinguish the individual from the group level. Not all those with negative identities support countercultural values or groups; not all who support countercultures have negative identities. Under some conditions, however, persons struggling with problems of identity and leaning toward a negative formulation are drawn into a social movement that validates their self-definitions and thus aids in the repression of the doubt and guilt that characterize ambivalent feelings. Or, in the absence of a countercultural group that gives validity to their negative identities, individuals may make tentative and ambiguous gestures to each other suggesting counter values, but leaving room for retreat or modification if the exploratory gestures don't lead toward consensus on the oppositional values (Albert Cohen, 1955:59-65; Klapp, 1969).

Even when the influence of social factors is taken into account, we should note that the concept of identity is not without problems. It has become loaded with implications and connotations related to the uncertainties of our time, as Bennett Berger (1971:90) says: "who would be so fatuous as to announce that he has found his identity." It is not even clear, in Berger's judgment, that finding it is a good idea. "A person who does not know who he is might *just be anybody*, and hence is fit for the unanticipatable opportunities and eventualities which rapidly changing industrial societies provide" (Berger, 1971:97). Having long thought that a dose of alienation and a dash of anomie were not only inevitable but essential in the modern world (Yinger, 1965), I share Berger's view to some degree. We are a long ways from knowing, however, what the long-run consequences of identity diffusion and of various levels of alienation and anomie are. Whatever the case, what requires study is the interaction between

given individual tendencies and countercultures, treating them alternately as the independent and dependent variables (see Kaplan, 1975; 1976).

RITUALS OF OPPOSITION AS COUNTERCULTURES

If countercultures are a continuing part of human experience, rooted in characterological and social constants, one should find them, in one form or another, in all societies. Their strength can be expected to vary widely, along with their specific or precipitating causes, but they should be found everywhere, in incipient or highly developed form.

This argument is put to its most severe test in its application to small, tribal societies, relatively isolated from contact with other societies. Many authors have described the role reversals and the rituals of rebellion and opposition that are not only permitted but sometimes required of persons occupying certain positions. Related to them are the saturnalia, the feasts of fools, the charivaris that come down to us from, at least, the days of classic Greece and Rome.

From James G. Fraser's *The Golden Bough*, to Gennep's *Rites of Passage*, to the work of Max Gluckman, Victor Turner, Jack Goody, Edward Norbeck, Peter Rigby, T. O. Beidelman, and many others, we have had a steady flow of ethnographic description and a variety of interpretations of rituals of opposition and of role reversals. Many American Indian tribes had burlesque ceremonies during which clowns "parodied serious rituals, introduced obscenity into sacred places . . . , and showed open disrespect for the gods themselves" (Wallace, 1966:136). A striking feature of the organization of the Zulu "is the way in which they openly express social tensions: women have to assert license and dominance as against their formal subordination to men, princes have to behave as if they covet the throne, and subjects openly state their resentment of authority" (Gluckman, 1963:112).

Such activities can be matched in the medieval and contemporary worlds. By the late fifteenth century, All Fools Days were banned by the cathedrals, but they

were picked up by laymen, by families, craft guilds or "sociétés joyeuses" who planned ribald festivals, Abbeys of Misrule. Lyon had a Judge of Misrule and a Bench of Bad Advice; Rouen's Abbot had serving him the Prince of Improvidence, the Cardinal of Bad Measure, Bishop Flatpurse, Duke Kickass, and the Grand Patriarch of Syphilitics (N. Davis, 1971:41-4).

Needless to say we have Halloween, football weekends, Mardi Gras, New Year's Eve, and twenty-four-hour rock concerts, all tolerated and to some degree protected by the agents of the official culture. Less obviously, but perhaps more powerfully, some teaching, particularly in the humanities and the social sciences, can be seen as a ritual of opposition attacking the established order and describing, or implying, an alternative cultural world.³

To some degree, to analyze a society is to weaken the mythic hold of its culture. I do not argue against this—indeed, at this moment I am engaged in such analysis—because the alternative, a kind of rigid ignorance, is more risky. Yet we need continually to remind ourselves that "a little learning is a dangerous thing." In a brilliant essay "On the Teaching of Modern Literature," Lionel Trilling (1966:3-30) observed that most of the best of modern literature is subversive, filled with "strange and terrible . . . ambivalence toward the life of civilization." From Blake and Wordsworth to Nietzsche, Conrad, Lawrence, Gide, Yeats, and Joyce (make out your own list) there runs a "bitter line of hostility" toward the dominant culture. By the time students have read Nietzsche's *The Birth of Tragedy*, Conrad's *Heart of Darkness*, and Dostoevsky's *Notes from Underground*, they seemed ready, as Trilling (1966:25) put it, to engage in "socialization of the antisocial, or the acculturation of the anti-cultural, or the legitimization of the subversive."

³ Particular circumstances, make a given discipline "a fulcrum for the rejection of established social arrangements" (Ladd and Lipset, 1975:73). During earlier periods, the physical sciences were also a source of radical challenge.

Such study—and the exams on the material—are a kind of ritual of rebellion, allowing strongly countercultural feelings and ideas to be expressed, but within a culturally permitted and ritualized frame of reference—the classroom—where participants see themselves and are seen as in a liminal phase, to use Gennep's (1960) term, surrounded by cultural ambiguities as they move from one social position to another.

How can we account for these sanctioned, even sponsored, deviations from the established cultures, these culturally circumscribed countercultures? They are most commonly seen as cultural inventions that serve, whatever their origins, as lightning rods. Gluckman (1954) described rituals of rebellion that blatantly subverted the usual moral and sexual norms as cathartic release mechanisms that lowered anger and resentment. It seems paradoxical, Wallace notes, that some people should be permitted, even required, to do the "wrong" thing, the culturally forbidden. "The paradox, however, is only a seeming one, for the ultimate goal is still the same: the maintenance of order and stability in society" (Wallace, 1966:135). Gluckman (1963) developed the distinction, formulated earlier by Aristotle (or, if you prefer, by Weber) between rebellion and revolution, and argued that only rebellion against rulers, not revolution against the system, could develop in tribal societies because they could envisage no alternatives. Such rebellions, he argued, were culturally contained and ritualized even as they permitted drastic counter-normative behavior.

These explanations seem to me to be, at best, incomplete. Even in the most isolated societies, persons who feel abused can imagine alternative norms and values in the form of contrast conceptions to those that prevail. Rites of opposition express a contradiction in society. They do not resolve that contradiction, but give voice to strongly ambivalent feelings. They often contain sharp criticisms of the social order, allowing some persons to express values that stand as potential reversals of the dominant values. This keeps such values alive while not compelling the participants to see themselves wholly in

terms of them. Such rituals also allow the orthodox, the "straights" to see and hear and sense the force behind alternative ways, perhaps breaking their cultural isolation and rigidity. As Victor Turner puts it, describing the contemporary "rock" experience: "The structure-dissolving quality of liminality is clearly present . . ." (V. Turner, 1974:263). He then goes on to note that the "rock" experience is ancient, arguing against those who suppose that contemporary rock music, with its accompanying group experiences and multiple stimuli, is unique. "Anthropologists the world over have participated in tribal 'scenes' not dissimilar to the rock 'scene' . . . 'synaesthesia,' the union of visual, auditory, tactile, spatial, visceral, and other modes of perception . . ., is found in tribal ritual and in the services of many modern religious movements. Arthur Rimbaud, one of the folk heroes of the counterculture, would have approved of this as '*un dérèglement ordonné de tous les sens*,' 'a systematic derangement of the senses' " (V. Turner, 1974:264). As a writer for the *Oracle* put it: "imagine *tasting* G-minor."

Of course we lack the kind of controlled study that would allow us to speak with confidence about the countercultural aspects of rituals of rebellion or more contemporary forms of sanctioned deviation. What we do have is the experience of seeing such rituals being developed before our eyes, in the form, for example, of rock festivals licensed by city officials and aided by the police—even as many of the norms and values of the dominant society are disregarded. By careful study over time we may be able to determine to what degree such rites promote cultural integration (or rigidity) by serving as safety valves and the degree to which they promote cultural change and conflict, by keeping alternatives alive.

SOCIAL CHANGE AND COUNTERCULTURES

Countercultures are variously regarded as engines of social change, symbols of change, or mere faddist epiphenomena. There are several ways to examine this disagreement.

Countercultures as Mutations

One way to study the connection between social change and oppositional movements is to glance at genetic mutations, which stand as powerful analogies to countercultures. If we take them as suggestive hints about similar processes that occur on the cultural level, study of these analogies can help us to describe, if not to explain countercultures.

Biological systems are self-reproducing, but they are not closed systems. In addition to natural genetic variations, they experience drastic discontinuities. Most mutations are maladaptive, and individuals whose genes are thus modified are less likely to survive or reproduce than more standard members of the species so long as the environment remains relatively stable. If there are significant changes in the environment, however, deviant individuals—those carrying the mutant gene—may have a survival advantage. Recessive genes can be thought of as a kind of gene bank where maladaptive mutations are sometimes stored, a survival resource to be drawn upon if drastic changes of environment make them adaptive (Gerlach and Hine, 1973:224–5).

Some cultural deviations can be seen as the mutations of a society's normative system. Since it is difficult to believe, however, that the individual-societal-environmental system is as well-balanced as the organism-environment system—the result of a process of evolutionary selectivity—we ought not to assume that most cultural deviation, like most mutation, is maladaptive. Nor should we assume the opposite, after the fashion of those who believe that any kind of change is preferable to stability. Some cultural deviations, like most mutations, are lethal.

Most social scientists today believe that rigidity is a greater threat than are cultural mutations. As Donald Campbell (1975:1121) has argued, in an important paper, social scientists and psychologists may be overeager "to discover and believe antitraditional, antirepressive theories"; they may be especially receptive to "the prophetic message of liberation."

The old ways, of course, may contain social adaptations that have become destructive under new sets of circumstances. Pressure against the traditional cultures, however, comes not only from their failures to adapt to new circumstances, but also from the hedonic individualism and self-centeredness that are the product of biological selection. The continuing task, Campbell argues, is to arrive at a minimax solution or a stable compromise between the needs and requirements of the biological and the social systems (see also Meddin, 1976).

Social norms the world over seek to limit selfishness, greed, and dishonesty, even though it can be argued that biological evolution favors individuals who practice them. "Look out for your own interests" may be as important as "thou shall not covet," Campbell observes, but spontaneous compliance with the former generally makes normative reinforcement unnecessary.

This is a powerful argument, but I would emphasize somewhat more than Campbell does the rigidity built into social systems, partly as a result, ironically, of the social virtues that make social life possible. He notes that in Moses' day, as in ours, honoring one's parents could have been carried to dysfunctional lengths, "but such excesses were so little of a social problem that 'Thou shalt show independence from thy parents' was usually omitted from the limited list of reiterated commandments" (Campbell, 1975:1118); usually omitted, perhaps, but not always. Several centuries after Moses, a charismatic prophet with countercultural tendencies declared: "For I have come to set a man against his father, and daughter against her mother, and a daughter-in-law against her mother-in-law; and a man's foes shall be those of his own household (*Matthew* 10:35, 36).

This quotation prompts me to suggest that major countercultural mutations often—I am tempted to say in the majority of cases—appear as religious movements. "The sect," says Werner Stark (1967:129) "is typically a contraculture." A set of values that changes life drastically, that threatens not simply opposition but persecution, that is entangled with deeply am-

bivalent feelings about one's self and others, demands a powerful revelation to a charismatic leader or a deep-going mystical experience or the support of a select community of believers if they are to be experienced as a viable new way of life. Charisma can be counterposed, not only to the sense of an excessive rationalization of life, but also to the sense that an established moral order must be broken (Weber, 1963; Roth, 1975). (Under some circumstances, of course, the charismatic leader seeks to protect and repair an endangered moral order [see Wilson, 1975].)

The religious element in oppositional movements is well expressed in Wallace's concept of revitalization—a term that overlaps the meaning of counterculture in many ways. "It can be argued," Wallace (1956:268) writes, "that all organized religions are relics of old revitalization movements, surviving in routinized form in stabilized cultures, and that religious phenomena *per se* originated . . . in the revitalization process—i.e., in visions of a new way of life by individuals under extreme stress."

To say that many cultural mutations are religious is not to say that they are good—or that they are bad. Most persons would probably agree that certain prophetic movements with which they identify have been major forces in transforming an unjust or otherwise inadequate social order. Not many are inspired with awe, however, by hearing from the Church of Satan that greed, pride, envy, anger, gluttony, lust, and sloth are cardinal virtues, not the seven deadly sins (Moody, 1974; Alfred, 1976). In noting that many countercultures are religious, I want simply to emphasize that those involved connect them with the fundamental problems of existence.

Countercultures as the Result of Social Change

Major countercultural movements are, in the first instance, indicators of a society experiencing severe stress. The common tendency today to associate countercultures only with the middle and upper classes fails to see that they are often a

two-pronged attack on the established culture, expressing different kinds of stress. On one side we get the countercultures of the privileged who say, in effect: here are values we can respect, that give us a sense of meaning. On the other side are the countercultures of the deprived who say: here are values we can attain, that give us a sense of control. To cite a diverse list of the latter, without explication, there are countercultural elements in "the culture of poverty"; among the Black Muslims, particularly in their earlier period; and in the values of delinquent gangs.

Thinking of countercultures as the dependent variable, we see many kinds of social change that upset the moving equilibrium of a social order, the structural-cultural-characterological balance of a given time, and make the appearance of countercultural movements more likely (none of these is a sufficient cause; some may be necessary; all are interdependent):

(1) Drastic reorganization of the way people make their livings. These ways are closely bound up with shared values and norms, with the power distribution, and with accustomed reciprocities. Rapid economic growth and related political changes, whether in industrialized or developing societies are closely connected with occupational changes and magnify their destabilizing impact.

(2) Changes in the size, location, age distribution, and sex ratio of a population. Insofar as a low average age is a factor, the most highly industrialized societies can expect a reduction of countercultural inventiveness, for better or worse. The average age in the United States is now 29—up from 25 a decade ago—and may be nearly 35 by the end of the century if present trends continue. The graying of America may prove to be a more accurate prediction than the "greening of America."

(3) Rapid importation of new ideas, techniques, goods, and values from alien societies or from earlier periods. By itself, this explains little, since one needs to know why the new ideas or goods seemed attractive, rather than repugnant, why cultural tariff barriers didn't keep them out. It is clear, nevertheless, that countercultural movements often use

foreign norms and values for their contrast conceptions; and the production and use of goods from another culture can, under some conditions, have a powerful dissolving effect on the established ways of doing things.

(4) Sharp increase in life's possibilities, hopes, dreams, and actualities, followed by a plateau, actual loss, or serious threat of loss. This has long been recognized as a factor in revolutions. Countercultural movements do not typically occur when desired values are becoming more difficult to attain, despite the ideology of the protesters, but when they have become more accessible over a period of time, although at a rate slower than the increase in demand. The result is a stronger sense of relative deprivation.

(5) Lower participation in intimate and supporting social circles—families, neighborhoods, work groups (Hendin, 1975; Berger and Berger, 1971). Many counterculturalists ranging from the utterly deprived to the "poor little rich kids," share in common a broken bond between parents and children that predisposes the latter to many forms of negativism. Some turn the negative impulses inward, which in our time has meant a sharp increase in rates of anxiety and suicide, or they may find support for turning the hostility outward. Feuer (1969) speaks of the deauthorization of the older generation as the major cause of the coalescing of individual oedipal problems into a shared revolt. He is one-sided in his interpretations and imprecise in his discussion of the sources of such deauthorization, though it seems reasonable to note its interdependence with the other sources of countercultures we have noted. The loss of intimacy between generations does not seem to have been overcome by the "love" generation, whose children are usually neglected in the name of permissiveness, much as they themselves were neglected (Yablonsky, 1968:302–15; Rothchild and Wolf, 1976).

(6) An increase in the number of antinomian persons, or those with negative identities, or in the strength of tendencies in those directions.

(7) A loss of meaning in the deepest symbols and rituals of society; or, if you

will, a religious crisis (Wuthnow, 1976b). The other factors we have mentioned doubtless contribute to the loss of meaning and several are, in turn, strengthened by it; but we are far from knowing why a major reduction in the legitimacy of the values of a society—rather than endemic but scattered skepticism—occurs. In a recent study of over a thousand university students in six countries (Yinger, 1977), I found that sixty percent believed that problems of meaning were the most fundamental issues facing humanity. (About half of these also mentioned problems of injustice, suffering, or both.) The present crisis of meaning is now generations old, and many feel, with Matthew Arnold (1907:321) that they are

Wandering between two worlds, one dead,
The other powerless to be born. . . .

Many of those caught in such a situation grasp for faith; they invent what they can (glorious religions are seldom the outcome) and they borrow meaning systems that seem uncorrupted by the society around them.

We need a theme? then let that be our theme:
that we, poor grovellers between faith and
doubt,
the sun and north star lost, and compass out,
the heart's weak engine all but stopped, the
time
timeless in this chaos of our wills—
that we must ask a theme, something to
think,
something to say, between dawn and dark,
something to hold to, something to love.
(Aiken, 1936:2)

We are moved by the poet more than by prosaic facts, but I think we need to guard against pluralistic ignorance—a shared belief in pseudo facts. It may be that a moderate reduction in the sense of shared fundamental values is so threatening that we are drawn to those sensitive voices warning of imminent danger.

(8) Such social and personality factors as we have mentioned create the context in which countercultures are likely to occur, but at least one other element is needed: communication among those with predispositions and living in situations that can precipitate countercultural values. Potential members need to identify one another; leaders need audiences;

powerful signals to and from the dominant society are required to help give protesters a sense of identity and to set boundaries. It is not by accident that the Diggers, Ranters, and Seekers, not to mention the more moderate Levellers, spread rapidly across England at a time when small printing presses were appearing in large numbers, when literacy was growing, and when the Puritans and the Royalists, struggling with one another, were for a brief period relatively tolerant of radical notions, each hoping to win support. It is scarcely necessary to note how much the TV camera, the mass circulation magazines and newspapers, the movies, as well as the enormously variegated underground press have spread the word in the contemporary Western world. More generally, the recurring conditions for countercultures are reenforced by direct lines of communication to the past and by the rediscovery and reemphasis of past leaders and movements (see Hill, 1975:ch. 18).

Countercultures as the Source of Social Change

When we turn the question around to ask how and to what degree countercultures are the cause of social change, we come to some of the most critical problems of sociological theory. With respect to recent American countercultures, a basically functionalist interpretation is now commonly given. Chief Justice Warren Burger of the United States Supreme Court recently declared "that turbulent American youth, whose disorderly acts he once 'resented,' actually had pointed the way to higher spiritual values" (*Cleveland Plain Dealer*, May 29, 1976:5-A). Philip Slater writes that a basic characteristic of successful social systems is the presence of devices that keep alive alternatives antithetical to the dominant emphases.

These latent alternatives usually persist in some encapsulated and imprisoned form ("break glass in case of fire"), such as myths, festivals, or specialized roles. . . . Such latent alternatives are priceless treasures and must be carefully guarded against loss. For a new cultural pattern does not emerge out of nothing—the seed must already be there. (Slater, 1971:110-1)

Well, some of them are priceless treasures and others are lethal bombs; but we may not be able to have one without the other. Slater's argument is close to Durkheim's interpretation of crime, which Durkheim declared; is necessary to the evolution of morality and law. "In order that the originality of the idealist whose dreams transcend his century may find expression, it is necessary that the originality of the criminal, who is below the level of his time, shall also be possible. One does not occur without the other" (Durkheim, 1938:70-1). And, of course, Durkheim goes on to say that crime not only implies that new ways are open; it may even be an anticipation of new collective sentiments. Socrates was a criminal, but he helped pave the way for a new morality.

I am uncomfortable with such unqualified functionalist views. Countercultures may stimulate the growth of highly resistant antibodies in society that make wise and necessary changes less likely. Opposition to the bizarre may deflect attention from basic needs; it may furnish those most resistant to change with superficially strong moral arguments, not to mention allies.

Antithetical groups do not escape each other's influence. We take on the face of the adversary, as the French proverb puts it, whether we wish to or not. Or, more technically, culture and counterculture are bound together in linked evolution. Under conditions of lowered legitimacy and loss of faith, efforts on the part of the dominant society to repress new norms and values can lead to deviation-amplification, not to a reaffirmation of the established norms and values. Efforts to coopt the deviants can, by legitimating their more moderate practices, have the same effect. Thus we need to complement Durkheim's view of the way deviation strengthens a group by an evolutionary view of the way it modifies a group. "If you're not part of the solution, you're part of the problem" is one of today's slogans. Gerlach and Hine (1973:260) offer us a similar notion: "if you're not part of a mutation, you are part of the environment which selects for or against it. No one can escape an evolutionary role." Deviant

ideas that become the operative values of a group tell us, at the least, something about the stresses faced by the members of that group. On another level, they can tell us something about the larger system, indicating points of inadequacy. On still another level, they can prove to be new values required to meet a new situation.

The changes in values and norms prodded by countercultures cannot proceed far without concomitant changes in social structure and character. Social change, as I use that term, is the process of movement from one relatively stable structural-cultural-characterological system toward another. This does not imply "that societies move from situations of relative balance, through periods of disruption, into new situations of balance. New forces may enter a system at such a rate that the realignment process cannot proceed rapidly enough" (Yinger, 1970:477). A situation where structural, cultural, and characterological elements are continuously out of phase with each other may now be endemic. It should not be assumed to be either exceptional or pathological. Nor should we assume that change must begin with one part of the system, with structural change in the economy, for example, or with the revelations of a charismatic leader, or with a shared cultural vision of what the new world should be. Whichever part of the social change process occurs first, its impact will be strongly influenced by the extent and direction of changes in the other parts.

Most revolutions show that structural change is more likely to be carried through than cultural change. In the early stages, revolutions usually contain a counterculture, carried along by a utopian conception of a new world. But those whose first concern is the transfer of power may believe this objective is threatened by demands for a new culture (Abner Cohen, 1974:ch. 3). After the transfer of power—often carried out in the name of new values—the new rulers don't seem very different from the old. From czar to commissar is not so far. Radicals fight the revolution; conservatives write the constitution. The family is attacked, to break the linkage of generations, but usually re-

turns to a pattern quite similar to its pre-revolutionary form.

The indirect effects of countercultures are probably more important than the direct and intended effects. The protest movements of mid-seventeenth-century England created a dissenting *tradition* that has outlasted some of the content of particular dissenting movements. This tradition has strongly influenced English and American history ever since. France and Germany, on the other hand, with much less strong traditions of dissent, have been more subject to revolutionary-counterrevolutionary cycles (see Baltzell, 1972:217-8).

Insofar as some upper-middle-class Wasps and Jews persist in their opposition to and withdrawal from technological society, Berger and Berger (1971) observe, they are less likely to modify that society drastically than to create new room at the top for the sons and daughters of manual workers—"The blueing of America." "If Yale should be hopelessly greened," the Bergers write, "Wall Street will get used to recruits from Fordham or Wichita State. Italians will have no difficulty running the RAND Corporation, Baptists, the space program" (Berger and Berger, 1971:23).

The effects of a given form of counterculture depend not only on its own characteristics, but on the situation. Suppose that it becomes essential for the United States to shift into a lower consumption, lower resource-using style of life. In the last few years we have discovered, once again, that even a small drop in accustomed levels of living exposes the society to enormous strains. All the political compromises and hard bargains through which a given distribution of resources has been attained are exposed. Without unusual support, lower consumption *values* would come about only from the effects of the slow and painful erosion of necessity. Now into this scene may come, not as a purposive invention but as a cultural mutant, a movement that reduces certain pleasure demands in the name of a powerful ideology that satisfies other needs. The reduction in what was formerly thought to be a high standard of living is not defeat; it is, in fact, the good life. We find "relig-

ious" support for the shift rather than sullen and strife-ridden retreat. Communes, ascetically inclined sects, hippies may help to devalue the pleasure currency, helping those involved and the many others pushed toward the new values by their appraisals of the situation, to balance their psychic economies. Without such shifts, we would have to think in terms of a continuously escalating pleasure-demand standard. The new values, of course, have a cost: painfully won disciplines, essential to other values, may be weakened; additional value shifts are tied into the change by the counterculturalists.

CONCLUSION

Someone has said that every country gets the socialist party it deserves. It is equally true that every society gets the countercultures it deserves, for they do not simply contradict, they also express the situation from which they emerge—pushing away from it, deploring its contradictions, caricaturing its weaknesses, and drawing on its neglected and underground traditions. If we shudder at the illegal drug problem, we ought more carefully to study our rates of alcoholism and lung cancer and the results of the use of other legal mind-altering drugs. If we shudder at the Manson family or the Symbionese Liberation Army, we ought to do more than contemplate the violence we do to some of our children in the ghetto or to the Vietnamese. If we shudder at the Church of Satan, we should at the same time note the aberrant violations of basic values by "respectable" people, violations that such nonconformist cults have transposed into virtues.

Lest I seem to be confirming only the negative image of countercultures, let me add: If we applaud the emphasis on gentleness and love, on conservation and sharing, on self-reliance and self-discovery that characterizes many oppositional movements today, we should recognize that these too—although expressed in the context of drastic value reversals—borrow from the dominant culture even as they oppose it.

The most important lesson from the

study of countercultures is not what it tells us about our times—or any specific time—but what it tells us about the human condition. In some ways, such a study underlines the points made by conservatives: the social fabric is delicate, it is based on long experience, it is built on constant factors in human life. Therefore, don't touch. This is often the wrong conclusion, however. Because the social fabric is delicate, we need continually to weave in new threads. A cultural-countercultural confrontation, a consequence of changing conditions and inflexible structures, is a costly way to proceed. We need to learn how to respond to early warning signals rather than waiting for over-compensating attacks powered by ambivalence and anger.

In my judgment, we are in the midst of a major civilizational transformation. The critical issue that humankind faces today is: How to create a rolling adjustment to the incredibly rapid and drastic changes taking place on the planet. We're faced with the problem of rebuilding the station, relaying and changing the gauge of the tracks, and accommodating vastly more passengers, while still keeping the trains running. Some say: don't change, civilization can be breached too easily; or, in the language of the train analogy, patch up the station a bit, but don't tamper with the basic structure. Others say: stop the trains; the building isn't worth saving; it's about to collapse; we need a clean field on which to build anew. That is the position taken by countercultures. If we think of them as art forms, we may find that, like other forms of art ranging from the sublime to the ugly, they highlight, dramatize and anticipate drastic problems. Whether as "voices crying in the wilderness" or as symptoms of major disorders—unintended warnings and illustrations of what may lie ahead—countercultures require the most intensive study by those whose aim and task it is to study societies and to see them whole.

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"SITUATION" VERSUS "FRAME": THE "INTERACTIONIST" AND THE "STRUCTURALIST" ANALYSES OF EVERYDAY LIFE *

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"Situation" and "frame," the elementary units of analysis of two versions of micro-sociology, are compared in order to elucidate the currently existing, but (as such) barely recognized, interactionist and structuralist approaches that they represent and to demonstrate that they contain widely divergent understandings of everyday life. After certain of the notions that underlie each of them are made clear, the contrasting positions of these two approaches are reviewed with respect to common issues, including the nature of the self, the place of meanings and subjectivity in analysis, and what are appropriate research methods. These considerations provide the basis for a general interpretation of Goffman's work, something the sociological literature has lacked. Though Goffman is most often treated as a symbolic interactionist, this paper argues that a better understanding of his work results from reading it as a version of contemporary structuralism. The fundamental ways in which his structuralism is distinct from the cultural (as well as the interactionist) approach also are given.

Micro-sociology, the attempt at a disciplined study of everyday life, has required that its theorists conceive of the "natural" unit of analysis for such a study, that is, the organized and bounded social entity most immediate to the individual's experience, within which his/her mundane affairs with others occur. This paper examines and compares two of the most significant of these conceptions—the "situation," as it is applied within symbolic interactionism, and the "frame," as conceived (most notably) by Erving Goffman—in order to demonstrate that they contain widely divergent understandings of the nature of everyday life. It is an exercise that must be seen as a fragment of what is necessarily a lengthier, more complex project that attempts to develop a truer and more comprehensive understanding of the body of Goffman's work, in part by proposing to view it as an American variant of contemporary structuralism and, consequently, by abandon-

ing the common misunderstanding that makes of Goffman a symbolic interactionist.

The initiate into American sociology might well come to know Goffman as a symbolic interactionist. That Goffman occupies a place within this school, sometimes as a major spokesperson, has been a frequent casual assertion in informal sociological talk and the (presumably) firmer conviction of many of those who make the classification of sociologists and sociological works their instrumental concern (e.g., Dreitzel, 1970:viii; Zeitlin, 1973:189ff; Mullins, 1973:76ff; Warshay, 1975:29ff; Eisenstadt and Curelaru, 1976:85,199,277; Cuzzort and King, 1976:240fn).¹ This paper asserts that when Goffman's work is forced into the symbolic interactionist paradigm and made to reflect, and speak to, its special concerns, much of the author's actual meaning and significance is lost, the result being some measure of distortion.² Such misreading

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¹ Many others make the assertion though it is not essential to their task (e.g., Denzin, 1972; Cole, 1975; Stokes and Hewitt, 1976).

² There are good sociological reasons to be found for this forced reading of Goffman's work within the organization and recent history of American sociology which, for lack of space, cannot be broached

conceals the fundamental and systematic differences between the theoretical character of Goffman's work and that of symbolic interactionism properly understood. These theoretical differences—regarding, for instance, the origin and nature of the self, the place of meanings and actors' subjective orientations in analysis, and what are appropriate research methods—surround issues of central importance, that is, those issues with respect to which symbolic interaction as a perspective has been defined by its leading figures (see Rose, 1962; Blumer, 1969). As will be seen, Goffman's approach stands opposed to the central tenets and most basic assumptions of symbolic interactionism.

Goffman's work from the beginning, has demonstrated not random departures from symbolic interactionism or mere idiosyncracies, but the application of an alternative paradigm.³ As opposed to the interactionist framework, Goffman proposes, albeit somewhat implicitly, his own version of a structuralist one.⁴ A structuralist reading of Goffman's work frees up for correct theoretical understanding most of the elements that have been seen in terms of author's style (or taken as superfluous) as well as those that previously have been misinterpreted or have simply gone unnoticed.

here. There are, it must be said, those commentators who refrain from applying the interactionist framework, at least in full, in their analyses of Goffman, and these are the better pieces (e.g., Gouldner, 1970:378–90; Glaser and Strauss, 1972; Manning, 1976). Yet these, too, fail to reach a general understanding of Goffman because they apply no alternative framework for interpretation and, therefore, remain impressionistic. Most often, some oddities are noticed about Goffman's work but they remain puzzles (e.g., McCall and Simmons, 1966:144–5). The resulting discussion of Goffman's work can be a confusing one (e.g., Collins and Makowsky, 1972:202–14).

³ It is not my intention to deny that important changes have taken place in Goffman's approach that some (e.g., Manning, 1976) have noted. This paper focuses on the central issues with regard to which Goffman has always been at odds with symbolic interaction. It might be said, however, that it is in *Frame Analysis* (1974) that Goffman most consistently takes the position ascribed to him here.

⁴ Certain commentators have begun to see this, notably Jack Douglas (personal communication) and Jameson (1976) whose excellent article suffers only from too great an association of Goffman with ethnomethodology.

In this paper, the constructs of situation and frame represent two opposing paradigms that are engaged currently in the study of everyday life, but each must be seen as the derivative of a classical sociological tradition. "Situation" is a password that opens up an interactionist (or social action) approach, the basic principles of which are Weberian, while "frame" poses an apparatus for a kind of structuralism for which Goffman is heavily indebted to Durkheim.⁵ The use within micro-sociology of these fundamental constructs activates many of the elements operative in each of these broader classical traditions, and multiple points of comparison continually suggest themselves. In this paper, most of these must be left undeveloped.

It should be clear that the identification and comparison of distinct theoretical approaches, such as the two in question here, is a project that requires some systematic method of its own. This study follows the suggestion of Bierstedt (1960:8) "to push [each] particular interpretation of social phenomena just as far as it is reasonable to go in our effort to shed illumination on it." What are presented, then, are idealized descriptions of each approach, which "candidly employ exaggeration" of their purity and exclusivity of orientation as an "heuristic device" (Bierstedt, 1960:8). This is seen as only a preliminary step in the overall effort to unify sociological knowledge, but crucial in order to avoid the common "facile synthesis" in which a reconciliation of perspectives is achieved at the cost of letting important differences between them go unrecognized. The point of our slightly

⁵ Unfortunately, in this paper, the links with these traditions cannot be made fully clear. This is an important task because micro-sociology has been thought of as a homogeneous field based on its presumed empirical domain, everyday life, and rejected or defended as a whole on the basis of the importance or triviality attributed to this interest. This atheoretical tendency should be counteracted by locating different micro-sociological theories within the greater theoretical paradigms to which they belong, that is, by showing to what theories of society they contribute. On this issue, this paper utilizes the recent work of Pope et al. (1975), which emphasizes the divergences, theoretically and methodologically, between Durkheim and Weber.

idealized codification of approaches, then, is to establish the actual nature of what are taken to be valid perspectives on the topic at hand. Only after such an effort is made can we hope for the working out of differences to produce a true synthesis, one that does not sacrifice the indispensable contribution intrinsic to either of the original partial approaches.

The Uniqueness of "Situations"

Symbolic interactionists have considered it their theoretical calling to describe the rich texture of everyday social life, a quality highly valued by them and, allegedly, coldly ignored in the functionalist portrayal of social reality, against which they define their movement (e.g., Scott, 1970). To remain true to human experience, they have insisted on study at close range, as participants, and, from this vantage point, the situations that make up everyday life have been seen as idiosyncratic.

Objective situations are unique. In the words of W. I. Thomas, "social situations never spontaneously repeat themselves, every situation is more or less new, for every one includes new human activities differently combined." (Stebbins, 1967:154)

The reward for research activity would seem to be an endless wonderment.

In fact, given this initial assumption about the uniqueness of situations, the abundant research energies of symbolic interactionism have cast it, as a body of work, in the direction of ever-more detailed description, that is, toward a vigorous empiricism. What concepts it has managed to generate have always been in danger of being rejected from within the paradigm by practitioners whose tactic to employ more precision (to appreciate more fully the rich texture, the "indeterminancy," of social life) undercuts attempts at generalization (e.g., Turner, 1962). The assertion that each situation is unique has been devastating of theory. Symptomatically, Blumer (1969:147) proposed that what he called the "definitive concept," that refers "precisely to what is common to a class of objects," give way in sociology to the less powerful "sensitizing concept" to prevent unwarranted

generalization. As he explained, "working with and through the distinctive or unique nature of the empirical instance, instead of casting this unique nature aside calls, seemingly by necessity, for a sensitizing concept" (Blumer, 1969:149).⁶

In contrast with the above-noted tendencies within interactionism, Goffman's work should be seen, in its essence and direction, as a formal sociology.⁷ This means that it is an effort toward the abstraction from everyday life of the very definite and limited number of forms or modes in which this life occurs. Goffman's goal, following a typical structuralist program-statement, is "the enumeration of forms and their organization, the exposition of signifying structures and their mode of functioning" (Lane, 1970:37); that is, the discovery of classes of objects, precisely the project Blumer rejects.⁸ There are not so many forms in which everyday life occurs that one will frequently be unrecognizable, its rules unfamiliar, to individuals who find themselves contained in it. Each of the forms of everyday activity studied by Goffman has a name: one is at the "movies," or the "dinner table." Note, however, that situations are nameless, but describable as, for instance, "the time Joe spilled his beans."

⁶ Compare Weber:

For the knowledge of historical phenomena in their concreteness, the most general laws because they are the most devoid of content, are the least valuable. The more comprehensive the validity or scope of a term, the more it leads away from the richness of reality for in order to include the common elements of the largest number of phenomena, it must necessarily be as abstract as possible and hence devoid of content. In the cultural sciences, the knowledge of the universal or general is never valuable in itself. (Weber, 1949:30)

⁷ Denzin (1972:88ff.) cites Goffman as offering a "formal sociology," yet his description of what constitutes formalism is, at least in the case of Goffman, misleading. Furthermore, his contention that interactionism itself tends toward formalism is, in the view of this paper, incorrect.

⁸ Thus, while Goffman's (1974:10) "aim is to try to isolate some of the basic frameworks of understanding in our society," Blumer (1969:148) would insist that "we do not cleave aside what gives each instance its peculiar character and restrict ourselves to what it has in common with the other instances in the class," thus precluding any such attempt.

While interactionists attempt to deal with the unfolding of actual everyday events, it is Goffman's intent to "see behind" this constant activity to what, in modern parlance, would be referred to as the "structures" that invisibly govern it. These structures Goffman now refers to as "frames," (and earlier as "occasions," and sometimes misleadingly as "situations"). Whereas a situation is described by its particular contents, especially by those elements that are unique to it and have been composed within its own existence, a frame is described by the stable rules of its operation, whatever the circumstances of any particular enactment. In other words, frames are not to be thought of as empirical in the way that situations are.⁹

In a structuralist analysis (here Goffman's), everyday life is seen to be made up of more or less well-delineated "worlds," realms of special meaning within which a particular language of reality is binding.¹⁰ The world is a mode of experience fleshed out by adherence to the rules of a frame or occasion.

Each class of . . . occasions possesses a distinctive ethos, a spirit, and emotional structure, that must be properly created, sustained, and laid to rest. (Goffman, 1963:19)

⁹ Godelier (1973:336) provides a clear exposition of this matter from the structuralist point of view:

"structures" should not be confused with visible "social relations" but constitute a *level of reality* invisible but present behind the visible social relations. The logic of the latter, and the laws of social practice more generally, depend on the functioning of these hidden structures and the discovery of these last should allow us to "account for all the facts observed."

Because of his use of innumerable empirical examples, Goffman is often thought to be interested in a sociology of appearances (e.g., Gouldner, 1970). But, though they possibly are excessive, I believe these examples are intended always to point up the relation between structure and action. In this regard, note Goffman's (1961a:35, 85) crucial distinctions between, for example, "game" and "play," or "role" and "role performance."

¹⁰ Alfred Schutz's analysis shares this method of dividing the subject matter into modes of experience that he calls "provinces of meaning," but Goffman (1974:3ff.) criticizes his handling of this job. The micro-sociologies of Schutz and ethnomethodology are not within the scope of this paper.

For Goffman, it is the world, not the situation-event, that is characterized by an analytically relevant uniqueness of cognitive and emotional style. Being in a particular world provides experience that is qualitatively different from that offered by any other world; merely different enactments of the same world are analytically indistinguishable. Goffman expresses this in an analysis of the worlds that the playing of games makes possible.

A matrix of possible events and a cast of roles through whose enactment the events occur constitute together a field for fateful dramatic action, a plane of being, an engine of meaning, a world in itself, different from all other worlds, except the ones generated when the same game is played at other times. (Goffman, 1961a:26-7)

If the structuralist analysis identifies well-defined worlds wherein action takes place, for the interactionist the substance of everyday life is much more homogeneous. The rules the interactionist sees operating in human interaction are universal, that is, are features of all interaction, not just that of any particular realm of activity. Furthermore, these rules are not discovered in research activity, but are starting principles, applicable to the study of any setting. In other words, they are not accurately referred to as rules at all but, rather, as laws. Basically, these are the Meadian principles that humans are symbolizing beings who, in interaction, make attributions and hold out expectations of others, who in turn amalgamate these to form a self capable of formulating meaningful reciprocal lines of action. This process, symbolic interaction, underlies all realms of human activity. What differs between settings and serves to generate interest within the interactionist paradigm is the varied character of social life that may be found and the different roles that are "played" by individuals, whose apparent naivete with regard to the facts of their cultural origin seems, from within the paradigm, to be notable.

Symbolic interactionism has looked to curious manifestations of its laws and, consequently, has been criticized for its picture of social reality that overemphasizes the "exotic" (Meltzer et al., 1975:98). Now we can see that this ten-

dency is *determined* for those working within the interactionist paradigm, who seek, quite naturally, to provide illustrations that will elicit high interest. The more exotic the behaviors of subjects appear to be, the greater the sense of irony generated by the seemingly blind identifications of these actors with their social roles. Barthes' examination of the "myth of exoticism" behind much popular ethnography lends support to this description of the similar habits of interactionist studies. In this sort of ethnography, he says, it is contended that "all things are alike, . . . they are only different in hue. . . . The cultural facts are never related to a particular historical order" (could we say particular world?). Rather, each example provides the means for emoting "the theme of an eternal condition" (Barthes, 1957:94-5). So it is that what varies for interactionists and makes things interesting, is not the set of rules that guides collective action in any setting, but the changing *contents* of human roles and situations.

In their descriptions of various subcultures that are built upon the pure laws of interaction, interactionists glide over the boundaries marking off the worlds that Goffman feels as stumbling blocks. He would consider the Meadian laws of symbolic interaction as something like truisms, which leave us as theorists in a seamless world whose apparent consistency defies real analysis. His work suggests that the apparent continuity of everyday life masks a real discontinuity, the coexistence of forms of daily life showing dramatic differences in principle. Their boundaries mark not just changes in taste or style, in how naked beings are clothed, but potentially significant differences in the character and quality of everyday life.

Thus, Goffman (1974:127) attacks the interactionists: "The first issue is not interaction but frame." Too easily, he says, "the term 'interaction' equally applies to everything one might want to distinguish," but this simple application "conceals the analysis that would be required to make sense out of this interaction" in the first place (Goffman, 1974:127). One cannot claim to have reached a scientific understanding of a strip of interaction,

says Goffman, unless one has worked out an understanding of the rules of the frame in which this interaction takes place. This latter understanding has been unconscious for the interactionists, who, like ordinary participants, have brought unexamined prior knowledge of background rules to the settings they have studied. Consequently, they have described variations of behavior within particular frames without acknowledging the set of rules and structured social relations that make the existence of such forms of activity possible.¹¹ To take Goffman's own example of this point, interactionists have gone to the theatre and studied such a phenomenon as performer-audience interaction without having come to recognize the system of conventions that brings into existence a "stage" (a new "plane of being") and a cast of "characters" who can behave without the ordinary consequences for their self-images.

Those who have studied the process of "defining the situation," then, mostly have ignored the micro-structures within which this informal, and (in a structuralist view) largely inconsequential, activity of defining takes place. But Goffman's study of everyday life has made these frames its focus. It asks about the central working principles that animate any particular mode of activity. "Frame analysis" is the attempt to become cognizant of the rules for cognition and communication that are bound up with the production of any world. Goffman's contribution to structuralism is his application of this method, almost unannounced, not to works of art or literature or to myth, but to the various "media" of everyday life, which constitute for him its different "genre" (Hymes, 1975), that is, forms of mundane activity characterized by stable rules of operation and understanding.

The Precariousness of "Situations"

Realizing that within a culture there is a great deal of uniformity in the manner in which particular activities are collectively

¹¹ See Godelier (1972:xv-xvi) for a terse account of the logically similar Marxist-structuralist critique of classical economics.

carried out—that a wedding, for example, is, in our society, always very much the same—interactionists have felt it necessary to make reference in their work to a distinction that is possible between “pre-determined” and “spontaneous” definitions of the situation (Thomas, 1972:332) or, in the same vein, between “rigid” and “pliable” ones (Stone and Farberman, 1970:150). Favoring the former pole of this distinction, there are those “role theorists” who have emphasized the cultural as opposed to the situational origins of these definitions. It will be necessary later in the paper to show why this concession does not make their analyses structuralist, as some commentators have suggested (e.g., Stokes and Hewitt, 1976).

For now, we should see that this tendency of some role theorists to emphasize the stable and cultural aspects of definitions resists the main pull of symbolic interactionism.¹² The insistence, by interactionists, on an approach focusing on “process” (e.g., Stone and Farberman, 1970:150, *passim*) has led them to favor the second pole of the above-mentioned distinction. Thus, as Stokes and Hewitt (1976:840) say, “The focus and strength of interactionist theory is an emphasis on people’s capacity to create meaning and order in the process of interaction.” The elementary unit of social life within the paradigm, the situation, is not constituted of imposed formal rules, but informal ones, devised from within for the duration of its short life. Its character, it is often stated, is ongoingly “negotiated,” always freshly accomplished. Seen as the ephemeral construction of social actors who are co-present and engaged in interaction, the situation is conceived as a *precarious* state. At any moment, it exists as a delicate and alterable balance of human (and physical) factors, the very “reality” of which can depend, we are told, on simple belief, i.e., its definition as real by participants.¹³

To Goffman, it is unthinkable that social reality might undergo a construction or reconstruction at every encounter. As opposed to the once-only understandings associated with situations, frames represent the worlds that are commonly “available” to members of a culture, and routinely “realized” through adherence to their conventions (Goffman, 1961a:41,28).

We cannot say the worlds are created on the spot because, whether we refer to a game of cards or to teamwork during surgery, use is usually made of traditional equipment having a social history of its own in the wider society. . . . (Goffman, 1961a:27–8)

The structure of a frame, unlike that of a situation, is fixed and left essentially untouched by everyday events. Thus, when some commentators (Gouldner, 1970; Collins and Makowsky, 1972; Manning, 1976) insist that Goffman depicts a social reality that is precarious and fragile, we must specify that it is the individual’s security and *sense* of reality that, in Goffman’s world, may be so described, not the social structure. Any world is an objective reality, whatever the status of its participants’ belief in it (Goffman, 1974:4–7). Variable definitions of this reality “contribute very marginally to the events in progress” (Goffman, 1974:1).

It is significant in this regard that Goffman sees the rules of frame as analogous to the syntactical structures of language (Goffman, 1974:11–2, 236). In this he shares a property crucial to most versions of contemporary structuralism, “the belief that all manifestations of social reality . . . that are practised in any society constitute languages in the formal sense” (Lane, 1970:13–4). The characteristic of language most salient to the present discussion is its stable and given structure. Its rules are left virtually unchanged by speech-events. Similarly, argues Goffman, social actors do not fiddle endlessly with the way their everyday worlds are built, nor need they explicitly recapitulate

¹² J. Turner (1974:151–92) makes the distinction between symbolic interactionism and role theory clear. Also see Robin (1974).

¹³ Peter Berger’s frequent usage of the word “precarious” to describe social reality is precisely this one, which is no accident. His work (Berger and

Luckmann, 1966:19) is also an attempt at “a sociological analysis of the reality of everyday life.” In that analysis, Thomas’ “definition of the situation” becomes Berger and Luckmann’s “definition of reality.” This serves to cast the analysis at a societal (rather than situational) level, but major portions of the interactionist conceptual apparatus hold.

the rules for understanding each time they act.

Presumably, a "definition of the situation" is almost always to be found, but those who are in the situation ordinarily do not *create* this definition, even though their society can be said to do so; ordinarily, all they do is to assess correctly what the situation ought to be for them and then act accordingly. (Goffman, 1974:1-2)

By ignoring frame, interactionists have had to hold to the position that nothing is understood by social actors that has not been communicated outwardly, or dramatized (e.g., Perinbanayagam, 1975). It is supposed that shared understandings surrounding every matter contingent to the actors' present project must begin in interaction, i.e., have an accomplished character. For Goffman, on the contrary, the basis of any realm of activity lies in a preexisting system of rules. It is simply inconceivable, from this point of view, that every element on which a situation is grounded be established within its bounds and, in the case of many of its features, not feasible because the actors' understanding is unconscious. Furthermore, to assume that any one such element could be defined in isolation from the others overlooks the systemic nature of the relations between them.

That the interactionists can think of situations as built up piece-by-piece is possible because these entities are thought of as constituted of an innumerable number of elements, amalgamated during the ongoing process of interaction, having no definite arrangement, and among them no relations of dominance. Stone and Farberman (1970:151fn) thus can tell us that they have thought of more than fifty such elements that make up a typical situation.¹⁴ These elements, they say, are "assembled, arranged, manipulated, [and] controlled" (Stone and Farberman, 1970:151) by participants in defining a situation. Any real-life situation is unique, then, because of its once only configuration of elements. And, within interac-

tionism, there will be no reference to certain of these elements as being ultimately determinant within any situation. Rather, a theoretical parity is assumed between them.¹⁵

From Goffman's analyses of particular framed activities, we can derive certain principle characteristics of frames. A frame is not conceived as a loose, somewhat accidental amalgamation of elements put together over a short time-span. Rather, it is constituted of a set number of essential components, having a definite arrangement and stable relations. These components are not gathered from here and there, as are the elements of a situation, but are always found together as a system. The standard components cohere and are complete; that is, together they represent a potential world that answers all questions about what it is that shall be taken by participants as real, and how it is that they should be involved in this reality. Other less essential elements are present in any empirical instance and lend some of their character to the whole, yet these are of necessarily minor importance relative to the basic configuration of components that govern the action. In all this, frames are very close in conception to "structures" as discussed by Piaget (1970) and Althusser and Balibar (1970).

As has been suggested, it is inherent in the nature of the task that interactionists have set for themselves—constrained by the imperative to demonstrate the emergence of definitions during interaction—that the situation be considered extremely malleable. In fact, because its members are granted a "nearly complete flexibility in the form and direction of conduct," the situation seems in conception to be infinitely variable. "The image conveyed is of [the] virtually limitless creation of culture" (Stokes and Hewitt, 1976:840). Since a situation entails no central principle of organization, no set of fixed relations at its heart, its participants presumably may redefine its loosely bound elements one at a time, incrementally changing its overall char-

¹⁴ Stebbins (1967:156,158) also refers to the "great many factors" that have a role in the composition of a situation, and he lists some of them. See also Stebbins (1972).

¹⁵ Compare Weber's very similar understanding of a historical "situation" as described by Aron (1970:235-47).

acter. A frame, though, would seem not to be subject to the gradual changes brought about by continued redefinition. Although change processes are very much under-examined by Goffman, a frame's invariant status implies that change occurs at some point by rapid mutation. With a great enough reworking of frame, we have done more than redefined a situation; we have destroyed a world.

"Structuralist" versus "Cultural" Orientations

I have referred to the emphasis placed by some role theorists upon widely-held cultural definitions of the situation, that is, on norms and values held collectively prior to the time a situation convenes. This is a viewpoint often termed "structural," and, sometimes, quite inappropriately, as we will see, as "structuralist," as, for example, in the recent article by Stokes and Hewitt (1976). In this section, I will discuss the important ways in which Goffman's approach departs from this "structural" or "cultural" one, even as it departs simultaneously from the interactionist approach (within which, incidentally, Stokes and Hewitt place Goffman with respect to their dichotomous "structuralist-interactionist" classification scheme).

Cultural definitions come about, it seems, from the accounts of some theorists, in the same manner as do the more ephemeral or situational kind, that is, through the interaction of individual social units. Thus, Stebbins (1967:158) speaks of them as having "developed from the interrelationship of many similar personal definitions." Such cultural definitions form the necessary consensus upon which any working social reality is seen to depend (Stone and Farberman, 1970:152; Perinbanayagam, 1974).

Frame analysis is in opposition to these views in at least two ways. First, "framing conventions," the set of rules governing an activity, are not seen as having arisen through a conscious, interactive process. Second, and more important in what follows, frame analysis rejects the picture of culture as given by role theorists, specifically the quality of near-perfect consis-

tency attributed to it when seen in the above-stated fashion—that is, as a large situation.

Within the cultural approach, the situations that embody a culture at any moment all reflect that culture's core values, as well as the principles of operation of the whole, which can be described as pluralistic. Each situation is seen as a mediating fluid between culture and the individual. It serves to transmit culture in two directions. First, it provides a medium of socialization where cultural norms and values materialize, i.e., are made real for the individual (Thomas, 1972). Hence, in this model, culture is ultimately the explanation of conduct as, within interactionism, it is the sum of the influences of co-present others (Stokes and Hewitt, 1976:839). Second, the incalculable number of situations serve as so many collecting devices for the inputs that individual actors make to the cultural "definition of the situation." In either direction of this flow, the transmission is true, clear and unimpeded.

The notion of frame allows Goffman a much different picture of culture and its consistency. Everyday activities are not simple reflections of the broader culture's organization and values. The boundary, or "membrane," between a focused activity system and the wider society is a screen which "not only selects but also transforms and modifies what is passed through it" (Goffman, 1961a:33). As Manning (1976:16fn) has noted, "since Goffman . . . sees the situated encounter as being 'tied' to the larger social structure through what he calls transformation rules, he does not adopt the Parsonian view that each relationship is patterned by a set of pre-established normative orientations." Though each framed activity is embedded in the organization of the wider society and owes its existence to the functioning of the whole, a close view of it does not provide a description of the greater system. There may be a difference, Goffman tells us, between, say, the distribution of substantive rights and power in a society and that which we find in the structure of a certain kind of encounter (Goffman, 1961a:34fn). Social action is contained, then, within micro-structures that have an

integrity as systems in their own right. Society is a "framework of frames" (Goffman, 1974).

What guides conduct in this structuralist model is not a set of shady core values or the influence of others who are co-present, but the individual's place within the formal social organization of a concrete activity, or, to put it differently, one's place with respect to the social relations of production of a ritual world. In comparison with this, an examination of Stokes and Hewitt's (1976) attempt to resolve the "parallel weaknesses" of the cultural and interactionist approaches must conclude that it offers no real alternative to these. Stokes and Hewitt simply give recognition to both "culture" and "others" as factors that influence conduct, thus paying homage at once to both the cultural and interactionist approaches, which, respectively, point to one of these two realities as that with which actions need be "aligned." Like these other approaches, they leave wholly unrecognized the structured "activity system" (or frame) within which everyday action, in Goffman's approach, takes place. In this way, Stokes and Hewitt succumb to the persistent belief that culture (meaning) and action are "all there is."

For the role theorist and the interactionist, then, culture is composed of activity-situations all operating on the same basic principles and carrying the same messages, reflections of one another and of the whole. But in the structuralist conception, each realm of activity exhibits, as a system, rules and understandings that may be in contradiction with those of other realms or with the greater society.

Where the social content of a situated system faithfully expresses in miniature the structure of the broader social organization in which it is located, then little change in the traditional role analysis is necessary; . . . But where a discrepancy is found, we would be in a position to show proper respect for it. (Goffman, 1961a:99)

In the structuralist model, it is in such discrepancy or incompatibility between the structures composing a system, where the potential lies for social transformation (Godelier, 1972:ix).

We can now see that role theorists who orient themselves to structure or culture and symbolic interactionists who emphasize process merely study from opposite ends what both see as a pluralistic and consensual society. Goffman's structuralism, while under the influence of both these other schools of thought, actually fits neither model and must remain misunderstood by those who would force it into one or the other.

Subjectivity and Meaning

I will return to the matter of exploring the divergence of the structuralist and interactionist approaches to the study of everyday life, now examining more traditional issues than those treated thus far: the place of subjectivity and meaning in analysis, and the ontological status of the self.

To gather the real importance of the role of subjective orientations within interactionism, it is helpful to return to the early development of this perspective. The formula known as the "definition of the situation" was conceived by Thomas in the 1920s in order to facilitate an understanding of the behavior of the individual social actor (Meltzer et al., 1975:22). It referred to the meaning an individual gave to his/her immediate circumstances, the interpretation one made of them. This definition was meant as one very important factor in the determination of the individual's behavior. Different behavioral responses could be seen as the result of variations in subjective definitions of the situation.

For the next generation as well, the focus of interactionism, in fact that of much of American sociology, remained "the person's behavior in the group" (Gorden, 1952:58), or "the relationship between . . . definition and behavior in the situation" (Stebbins, 1967:149).¹⁶ And, because it was the actor's subjective assessment of the situation that was seen as the most salient determinant of be-

¹⁶ Cottrell (1950:711) could say, in his 1950 presidential address to the ASA that, "Indeed, sociology can be thought of as a discipline devoted to the analysis of social situations."

havior, it was this mental construction that became the object of intensive study. Despite Thomas and Thomas' (1970:154-5) early warning that analyses should be done "not without regard to the factual elements in the situation," the "objective situation," as Stebbins (1967) termed it, came to be seen, quite inevitably, as negligible, since it was not to this unmediated reality that social actors responded (Perinbanayagam, 1974:523). It is the "subjective situation," stated Stebbins (1967:151), this "construction of the person's world, that is of concern to sociologists [because] it will affect [the individual's] action orientations." For his part, Stebbins (1967:151) announces what is usually implicit in interactionist studies: "In this paper the term situation will refer to the subjective kind unless stated otherwise." The tradition of interactionism had ruled out the objective conditions surrounding social action as a possible realm of scientific interest.

Interactionists, then, consider it to have been unquestionably established by their forebears that the starting point for any social analysis is the meaning that actors give to their situation. Thus, Perinbanayagam (1974:523) seems to feel that he is expressing more than just an analytical predilection when he states that "one is condemned . . . to a world of meanings." What he actually *does* express is the nature of his confinement within the interactionist paradigm. Of course, as Perinbanayagam suggests, an approach cast steadfastly at the level of meaning can be mobilized to an analysis of other than the behavior of individual actors within social situations:

We may fruitfully treat all human communicated material as "texts" (spoken, written, and I may add, carved, sculpted, painted, hieroglyphed, symbolized, emblemized, dramatized, filmed) and apply the same methods of interpretation and understanding. (Perinbanayagam, 1974:538)

But, if taken seriously, such an approach would entirely eliminate Goffman's present program of study, as outlined in *Frame Analysis*. For him, each of the above-mentioned media of expression presents a "code" (Goffman, 1974:7) to be cracked, an unconscious system of

categories and rules governing the creation and communication of meaning. It is the task of analytically recovering these grammatical systems that Goffman has set for himself. For him, the meanings transmitted via each of the media cannot be thought of as commensurate, as the quotation implies, for, implicitly contained within each, lies its more decisive "message," a formal system of social organization.

For interactionism, the focus on meaning is linked, as we have seen, to its conventional assumption that "everyday behavior is a 'direct' indication of [the actor's] inner state, of the doer's being" (Davis, 1975:601). Goffman, in the tradition of structuralism, inverts this understanding by making the subjective aspect of action subsidiary, an "after-effect." Any outward indication "is itself the substance, not the shadow" (Goffman, 1974:463).¹⁷ The actual pattern of determination, then, according to Goffman, is just the reverse of how, in our society (and in interactionism), it is perceived (Goffman, 1974:462-3). It is the activity of producing a particular world that generates a characteristic set of "inner states" for its participants. The pattern of their occurrence—when and for whom they shall be felt, as well as how they are to be expressed—is a determined aspect of the operation of any established kind of activity-system. Individuals infer the subjectivity proper to their roles in such a system by applying current doctrines concerning how they are to be related to their acts. Conventions regarding the nature of these "linkages" between acts and actors are a feature of the rules of any frame (see Gonos, 1976:200-1).

It follows from his estimation of the place of subjective orientations that Goffman's methodology necessarily differs from that employed in the study of situations. Because the situation is, in large part, a subjective reality, the means of its investigation lie most naturally in the Weberian method of *Verstehen*, the em-

¹⁷ Levi-Strauss (1963:68-70) similarly inverts the relationship between inner-states and their outward manifestations that he finds postulated in certain functionalist theories.

pathetic grasp of the social actor's understanding of the situation and his/her typical intentions within it. Documentation of these inner states may be sought through questionnaires, intensive interviews, or in natural dialogue.

Though for interactionists it is the only means of entry into the situation, Goffman does not wish for purposes of research to "take the role" of a participant in a setting (Goffman, 1974:8-10). Because his interest is largely in the unintentional properties (or syntax) of systems of activity, he can, for the most part, disregard what interactionists take to be the most important source of data, the meanings attached by actors to their situations. The rules for cognition and conduct by means of which an activity is actually produced and understood, as these are conceived of within structuralism, cannot be discovered in the consciousness of its participants, through an intersubjectivity. Thus, Goffman maintains a deep conviction about the uselessness of attending to actors' subjectivity and holds a special distrust of verbal admissions of motives.

When an individual speaks—formally or informally—sometimes what he seems to be doing is voicing an opinion, expressing a wish, desire, or inclination, conveying his attitude, and the like. These attestations of the existence of what are taken to be inner states have a relevant feature; they can be as little established as disconfirmed. (Goffman, 1974:502-3)

It is clear why Goffman's methodology, though following in its prejudices those of Durkheim, should seem mysterious, or even licentious, to some commentators. It is because his interest resides in what, for interactionists, is that forbidden terrain, the "objective situation" or, as MacIver put it, in "the situation as it might appear to some omniscient and disinterested eye" (quoted by Stebbins, 1967:150).

The "Self"

The interactionist perspective posits individual selves as the elementary, even the primal social entities, that is, existing prior to social structure. Mead, according to Blumer (1969:62), "saw the human being as an organism having a self." The

interactionist version of the "myth of creation" proceeds from the existence of selves (having certain predispositions) to the establishment, through interaction, of a social order. Situations and society are conceived of as "individual-generated" (Meltzer et al., 1975:24).

The starting point of this process, usually left unspoken, is called by Waller the "undefined situation," apparently not subject to any external constraints. For example, from Waller's *Sociology of Teaching*:

We see many undefined situations confronting the individual in the school. The new teacher confronts a situation wholly undefined, and he has very little idea as to the details of the definition that would be desirable from his point of view. The experienced teacher who faces a new class faces an undefined situation, and it is part of his job to impose his definition of the situation on the class quickly, before any alternatives have had an opportunity to be considered. (Waller, 1970:166)

Such a definition, Waller says, will establish, for example, whether the teacher will be "friendly" or "dominating" (Waller, 1970:166). Now, it is certainly true that such latitudes in work-style are allowed the schoolteacher and may indeed be discussed under the rubric of the definition of the situation.¹⁸ But, in the structuralist view, they must be thought of as fine points of adjustment within a system whose most fundamental rules are well-established and essentially nonnegotiable in our society, that of the classroom. This frame consists of a set of parameters for the student-teacher relationship, a peculiar conception of the nature of knowledge, a constricting system of grading and credit

¹⁸ It would be mistaken to understand the idea of frame as parallel in purpose to that of situation and, thus, as simply a substitute for the latter. Frame analysis retains a place for the notion of situation, as any concrete circumstance, but gives priority to the study of frames, which exist at another "hidden" level. Thus, in Goffman's analysis, factors are labeled "situational" that do not have a "continued consequence" (Goffman, 1974:426) beyond their present circumstances, in terms of frame. "[S]heer volume of commotion and furor is not what signifies, but again, it is frame relevance that counts" (Goffman, 1974:427). There is no equivalent distinction within interactionism.

which stratifies the student body and introduces power relationships, a customary physical arrangement, etc. Waller's approach, interactionism, effaces all this in beginning with a postulated "undefined situation" on which, presumably, actors may imprint their own notions of the classroom. Necessarily, then, Waller's description of the process of defining that ensues is surreptitiously narrowed in scope to focus on the teacher's actual rule-making capacity (see Waller, 1970:171). Consequently, the system of formal relations that is constituted by the classroom must go unexamined, while Waller, in the manner of interactionist studies, begins his analysis with nothing but selves.

Ultimately, then, the character of any situation is the result of the unique meeting of particular individuals and their combined effects on one another. In this conception, the sheer number of individuals present becomes important, as do arrivals and departures (Stebbins, 1967:153-8). Other individuals, not social structure or even norms, are seen as the greatest influence on social actors.

The attitudes of others are, in fact, the most important limitations upon the behavior of any one person, and they are also the most important inducements to any particular action. The attitudes of others are dynamic elements in the undefined situation which presents itself to any individual, and as a result of those attitudes the individual may come to define the situation in one way and not in another. Therefore we may say that one person defines the situation for another. (Waller, 1970:163)

In the interactionist analysis, then, differences in experience seem often to depend not so much on the realm in which interaction occurs as on whom an individual is with. The process of defining the situation consists of "the fitting together of the lines of behavior of the separate participants," resulting in what Blumer called "joint action."

For Goffman, everyday social encounters do not pose the problem of coordination, because they are examples of the ritualized reproduction of cultural objects according to preestablished formulae. Here, the primary forces exerted

on social actors do not come from other individuals.

The concept of interaction is thus transformed: instead of referring to mutual influence that might be peripheral and trivial, it now refers to a highly structured form of mutual fatefulness. (Goffman, 1961a:35)

For Goffman, say Glaser and Strauss (1972:457), "interaction proceeds to its termination almost as inexorably as a Greek tragedy."

Such imagery reflects the fact that a structuralist analysis, such as Goffman's, assumes a different starting point than that of interactionism. To employ one current means of expressing this, we can say that it does not "start with" social actors, but with the socially given frame that exists prior to these individuals, who are called upon to give it life. The language of frame analysis suggests that, in a very real sense, individual selves do not exist except for a tenuous social reality that certain frames bestow upon them. Self, here, is not an entity, as the interactionist perspective has it, but a dramatic effect. When and in what manner the self appears is an artifact of the operation of particular activity-systems, "not a result of some organic expressive carry-over from actor to actions" (Goffman, 1974:297). Goffman has, in different ways, always suggested that the appearance of self lies in a relationship, that it is conveyed through a discrepancy between structure and action (see esp. Goffman, 1961a:85-152; 1961b:320; 1974:293-300; Gonos, 1976:200-9). There is more than a hint that the issue of self only arises in our own historical epoch (Goffman, 1967:47-95). Far from grounding his analysis in the existence of individual selves, as is gathered by many commentators from a reading of the *Presentation of Self* (1959), Goffman has almost single-handedly among American micro-sociologists worked to undermine this possible grounding.¹⁹

The concepts "situation" and "frame" thus imply very different understandings of the place of the individual in analysis.

¹⁹ Note Balibar's similar determination to eliminate from social theory "as a foreign body" the idea of "men" (Althusser and Balibar, 1970:207).

Interactionists emphasize the world-building capacities of people in everyday situations and look to them as the source of social change, while the concept of frame leads Goffman to think of individuals as "supports" for the continued existence of social structures. They are "resources" or "materials" called upon to bring forms of activity to life: "human nature" simply "fits the frame" (Goffman, 1974:516).

Conclusion

Whereas interactionism objects to the "oversocialized conception of man" (Stone and Farberman, 1970:13) and emphasizes voluntarism, Goffman's individuals are aptly described as "sociological to the core" (Davis, 1975:601). What interactionists can be seen to glorify in sentimental manner as personal style, Goffman wishes to dignify as subject matter for a strict sociological analysis. Interactionism is charming because it retains a split picture for the analysis of everyday affairs and macro-social structures; thus, its easy coexistence with functionalism. Conversely, it is the understanding of daily activities as manifestations of social structure, a single picture so to speak, that enables Goffman to carry on his life-work, "to combat the touching tendency to keep a part of the world safe from sociology" (Goffman, 1961a:152)—a tendency, he suggests, that is very much alive within interactionism.

Goffman's work tells us that it is the reality of frame, not fashions of behavior, that the sociological study of everyday life must explicate, lest all modes appear to us as the same, as human interaction in different dress. It remains for us to analyze the place of the frames of everyday life within political and economic systems, the frames at another level of organization. This is the possibility that Goffman's work affords—the incorporation of micro-sociology into sociology proper.

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THE FRENCH REPUBLICAN CALENDAR: A CASE STUDY IN THE SOCIOLOGY OF TIME *

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This paper is a sociological analysis of the most radical attempt in modern history to challenge the Western standard temporal reference framework, namely the French Republican calendrical reform of 1793. This reform obliterated the existing system of units of time and time-reckoning and dating frameworks by introducing a new annual cycle, new months, a new weekly cycle, a new subdivision of the daily cycle, and a new chronological dating framework. The paper first analyzes the symbolic function of the Revolutionary Calendar, namely, the representation of three main themes of the French Enlightenment: secularism, naturalism and rationalism. It then brings into focus three major factors which account for its failure: totality of the obliteration of the traditional sociotemporal order, overemphasis on secularization, and advocacy of nationalistic particularism at the expense of a practical and cognitive disruption of temporal coordination on a global level.

MODERN WESTERN STANDARD TEMPORAL REFERENCE FRAMEWORK

One of the fundamentals of social life is a *standard temporal reference framework* which is shared by all members of the society. Since time is among the major parameters of the social world, any social organization presupposes a coordinative calibration of subjective temporal references in accordance with a standard yardstick. Subjective temporal formulations such as "a while," "soon," "then" or "later" ought to be definable in terms which are accessible to others besides their user (e.g., "twenty-six minutes," "next Monday," "on May 19, 1977" or "at 10:45 P.M.'). This impersonal, collective, or intersubjective modality of temporal reference has been brought into sociological focus by both Durkheim (1965:23), in his discussion of a "time common to the group," and Schutz (Berger and Luckmann, 1967:26-8; Schutz and Luckmann, 1973:45-58), in his discussion of "standard time." The evolu-

tion of a standard system of units of time and standard time-reckoning and dating frameworks is quite parallel to the evolution of such systems as language, religion and law and to the transformation of subjective spatial formulations such as "here" and "there" into a standard grid system. They are usually internalized during primary socialization, and Defoe's satirical portrayal of Robinson Crusoe's habit of keeping track of the days of the week (and his naming of the first person he encountered "Friday") far away from civilization is highly suggestive of their coercive power once internalized.

The modern Western standard temporal reference framework¹ is based on the relatively universal relevance and use of the Gregorian Calendar, the Christian Era, International Standard Time, and clock time. It consists basically of the following two elements:

(1) *A standard system of units of time*
The year consists of 12 months or 365 days. Each cycle of 7 days constitutes a week. Days are divided into 24 hours, each hour into 60 minutes, and each minute into 60 seconds. All seconds are isochronal (that is, durationally uniform), as

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¹ For some general sociological discussions of time and modern Western civilization, see Zerubavel (1976a; 1976b).

are minutes, hours, days and weeks. Of the twelve months of the year, seven are 31 days long, four are 30 days long, and one is 28 days (or 29 days in leap years) long. Every fourth year is a leap year and includes an additional day, the 366th.

(2) *Standard time-reckoning and dating frameworks* within which past, present and future events are standardly "localized" in time.² These frameworks are based on the following: (a) the standardization of time-reckoning by the clock and the institutionalization, in the 1880s, of *International Standard Time*; (b) a standard chronological dating framework, namely, the *Christian Era*, which takes the birth of Jesus Christ as a *standard temporal reference point* in relation to which events are "dated"; (c) a social convention, according to which days "begin" at midnight, months on the day called "—1st" and years on January 1.

Both our standard system of units of time and our standard time-reckoning and dating frameworks are social artifacts and, as such, by no means natural. The artificial origin of units of time such as the week, the hour, the minute or the second is pretty obvious. Yet even the year, the month and the day rarely correspond precisely to the periods of the revolution of the earth around the sun, the revolution of the moon around the earth and the rotation of the earth on its axis, respectively, but are, rather, "rounded" approximations of those. Moreover, the very selection of these natural cycles as the cornerstones of our system of units of time is in itself a *social convention*. Also, the fact that the temporal continuum does not naturally have any divisions along it suggests the conventional basis of our standard time-reckoning and dating frameworks as well. There is no "natural" reason for fixing the International Date Line and the boundaries between time zones where they are. Likewise, it is merely conventional that our era begins with the birth of

Jesus Christ, our year on January 1, our week on Sunday³ and our day at midnight.

The conventionality and relative validity of our standard temporal reference framework are among the basic arguments underlying Durkheim's (1965:22–8) critique of Kant's "apriorist" theory of knowledge and are also at the center of both Hubert and Mauss' (1909:213–9) and Sorokin's (Sorokin and Merton, 1937; Sorokin, 1943:171–97) characterizations of "social time." It also has been brought into focus time and again by comparative studies of non-Western calendrical, time-measurement and time-reckoning systems. However, an even better way of exposing the relative validity of the modern Western standard temporal reference framework would be to point out the changes made within it, since these imply that even within one and the same society it has no absolute validity and that it actually rests upon entirely arbitrary social conventions. Fortunately, history provides us with a fascinating case of a calendrical reform which spared almost none of the constituents of our standard temporal reference framework, replacing it with an entirely new one. Nevertheless, the fact that it did not survive longer than 13 years suggests that, despite its arbitrary and conventional basis, the standard temporal reference framework is deeply rooted in the culture, and its considerable organizational and cognitive centrality to social life may account for the resistance to any changes made in it. The following discussion is an analysis of the socio-historical significance of this reform and an attempt to account for its eventual failure.

² For some discussions of social frameworks of time-reckoning and dating, see Hubert and Mauss (1909:199–207), Halbwachs (1925), Sorokin and Merton (1937:623–26), Sorokin (1943:171–97), Roth (1963:1–13, 99–102) and Zerubavel (forthcoming: ch. 5).

³ Whereas the boundaries of the year, the month, the day or the hour are pretty clearly defined, there is some ambiguity with regard to the "beginning" of the week. According to the biblical and ecclesiastical tradition of Judaeo-Christianity, the week begins on Sunday, which is believed to have been the day of the creation of the world. However, though on most Western calendars the week is represented as a cycle which begins on Sunday, some ambiguity is introduced by the fact that the workweek begins on Monday. Thus, whereas Thursday is named "*Yom Hamishi*" (fifth day) in Hebrew and "*Quinta Feira*" (fifth market) in Portuguese (note also that the German name for Wednesday is "*Mittwoch*," which literally means "mid-week"), it is named "*Czwartek*" (forth) in Polish (see also Colson, 1926).

THE REFORM AND ITS SOCIAL MEANING

The reform which will be discussed here is the one associated with the French Republican Calendar (or the "Revolutionary Calendar"), which was in official use in France, its colonies, "sister republics" and annexed territories between 1793 and 1805. Based on a proposed recommendation by the Committee of Public Instruction,⁴ it was put into effect by the National Convention on November 24, 1793, and introduced the following changes in the standard temporal reference framework:

- (1) The establishment of a *new chronological dating framework*. The traditional Christian Era was replaced by the Republican Era, which began on September 22, 1792, the day on which the French Republic was founded.
- (2) The establishment of a *new annual cycle*. The traditional January 1 was replaced by September 22 as New Year's Day. Not only did the year on which the Republic was founded become a standard reference point for the new chronological dating framework, the day on which it was founded became a standard reference point for the new annual cycle.
- (3) The *uniformization of the months*. Unlike the traditional Gregorian year, which consisted of 31-day, 30-day and 28- (or 29-) day months, the Republican year consisted of 12 isochronal 30-day months. The five complementary days ("Sansculottides") were grouped together at the end of the year, as in the ancient Egyptian calendar. A sixth intercalary day was added on leap years ("sextiles"), which still fell every four years, although on the third—rather than the fourth—year of every group of four years ("Franciade"); that is, on years III, VII and XI, rather than on years IV, VIII and XII.

- (4) The *abolition of the seven-day week and Sunday*. Each 30-day month was divided into three 10-day cycles called "*décades*." Sunday was replaced by "*Décadi*," which was celebrated only every ten days, as the official rest day.
- (5) The *decimal subdivision of the day*. Days were divided into 10 hours,⁵ hours into 100 "decimal minutes" and "decimal minutes" into 100 "decimal seconds."
- (6) The introduction of an entirely *new nomenclature*. Aside from introducing new concepts such as "*Fran-ciade*," "*Sansculottides*," "*décade*," "decimal minutes" and "decimal seconds," the reformers renamed each day and month within the new calendar. The days of the "*décade*" were named in accordance with their numerical order as follows: *Primidi*, *Duodi*, *Tridi*, *Quartidi*, *Quintidi*, *Sextidi*, *Septidi*, *Octidi*, *Nonidi*, *Décadi*. The five "*Sansculottides*" were named after Virtue, Genius, Labor, Opinion and Rewards. The Catholic saints' days were abolished, and the days of the year were renamed after trees, plants, seeds, roots, flowers, fruits, farming implements and domestic animals. The new months were named after seasonal aspects of nature in the following manner: *Vendémiaire* (vintage), *Brumaire* (mist), *Frimaire* (frost), *Nivôse* (snow), *Pluviôse* (rain), *Ventôse* (wind), *Germinal* (seeds), *Floréal* (blossom), *Prairial* (meadows), *Messidor* (harvest), *Thermidor* (heat) and *Fructidor* (fruits).

It is hard to overemphasize the extent to which the reformers obliterated the traditional standard system of units of time and the traditional standard time-reckoning and dating frameworks, since hardly any component of those was spared. The scope of the reform was almost total, since its architects attempted not only to gain social control by imposing a new

⁴ For a complete protocol of the workings of this committee, see Guillaume (1894:437-51, 579-91, 693-713, 872-92). For some historical accounts of the reform, see Andrews (1931), Godechot (1951:363-6) and Herrmann (1938).

⁵ One implication of this was that watchmakers had to put new dials on timepieces that designated hours from "1" to "5," rather than "1" to "12."

rhythm of collective life, but also to bring about a *total symbolic transformation* of the standard temporal reference framework, as became an age pronouncedly devoted to total regeneration. As the fixing of leap years on the third—rather than the fourth—year of the *Franciade* suggests, these reformers were guided, in part, by none other than the considerations of the symbolic implications of introducing changes just for the sake of change. The Revolutionary Calendar was introduced in an age which advocated the total obliteration of the old order in the name of progress and modernity: the beginning of the new Republican Era marked the *total discontinuity* between past and present. In order to appreciate fully the social significance of this reform, which also accounts for the fact that the Revolutionary Calendar was re-adopted by the Paris Commune in 1871 (Mason, 1930:241), it is crucial to consider its *symbolic functions* within the socio-historical context of Revolutionary France.

Secularization

Ever since antiquity, calendars always had been closely associated with the religious sphere of life and, thus, totally controlled by the priesthood. In the particular case of pre-Republican France, the calendar in use had been actually introduced by Pope Gregory XIII and, thus, symbolically associated with the Catholic Church. In order to appreciate the symbolic significance of the Gregorian Calendar, one should note that, while Italy, France, Spain, and Portugal already had adopted it in 1582, it was not until 1752 that it was introduced into England, whereas Greece and Russia adopted it only after World War I (Ronan, 1974:603). The tremendous de-Christianizing effects of any reform of this calendar were pretty obvious. When Sylvain Maréchal proposed, back in 1788, to dispose of the saints' days as well as to abolish the seven-day week and Sunday, the Bourbon government ordered the destruction and burning of his influential *Almanach des Honnêtes Gens*, which it considered as "impious, sacrilegious, blasphemous, and tending to destroy religion" (Andrews, 1931:516). It is hardly

surprising that Maréchal opened his *Almanach des Républicaines* five years later with the following warning: "The calendar of the French Republic . . . must not resemble in any respect the official annuals of the apostolic and Roman church" (Andrews, 1931:525).

By introducing the Revolutionary Calendar, which was later condemned by John Quincy Adams not only as "superficially frivolous" and "coarsely vulgar," but also as "irreligious" (Lovering, 1872:350), the French Republic stripped the Church of one of its chief agents for exercising social control and regulating social life. The major thrust of the calendrical reform was against religion, and the new calendar was to be a civil one, divested of any ecclesiastical associations. More specifically, it was supposed to function as a de-Christianizer.

The Church's control of the *temporal regulation of social life* was challenged first of all by the abolition of the seven-day week and the replacement of Sunday by *Décadi* as the official rest day. Aside from the immediate symbolic significance of doing away with one of the most sacred elements within the Christian tradition, the introduction of the ten-day rhythm of social life played a major role in disrupting church-attending practices, since it presented both practical and cognitive difficulties in keeping up with the traditional, sacred seven-day cycle. It is interesting to note that an almost identical anti-clerical measure was applied nearly a century and a half later by Stalin, when he introduced the continuous five-day week (Moore, 1963:122).⁶ However, the replacement of Sunday by *Décadi* as the official rest day also resulted in a considerable reduction of the total number of rest days. That the new sociotemporal order involved working for nine—rather than six—days straight before having a rest day certainly did not help to increase its popularity.

Another act of de-Christianization was the replacement of January 1 by September 22 as New Year's Day. The temporal

⁶ Consider also the Fascists' attempts to secularize the calendar by introducing a civil, nonreligious rhythm of social life (Schneider, 1928:226).

continuum does not naturally have any divisions along it, and even though the annual cycle originated in a natural phenomenon, the decision as to where to fix the cut-off point that marks its "beginning" is a purely arbitrary one based entirely on social convention. It was Durkheim (1965:347) who maintained that it is the alteration between the sacred and the profane which leads man to "introduce into the continuity and homogeneity of duration, certain distinctions and differentiations which it does not naturally have." One of the major characteristics of any calendar is that it interrupts the continuous flow of time⁷ by introducing some regularly recurrent "*critical dates*" (Hubert and Mauss, 1909:197-208), and the events which constitute these standard temporal reference points usually have a particular symbolic significance within the society. In the particular case of the Revolutionary Calendar, a non-religiously significant event, namely, the foundation of the French Republic, was selected as the standard temporal reference point on which the annual cycle was based and, as far as secularization was concerned, that act had a most profound social significance. It was probably not practical convenience alone, but also symbolic considerations, that led Napoleon to restore the Gregorian Calendar beginning January 1, 1806, a totally meaningless date within the Republican Calendar, yet symbolically so significant (New Year's Day) within the restored one.

The Christian Era was replaced by the civil Republican Era as the standard chronological dating framework for precisely the same reason. It was time that separated the old order from the new one, and the beginning of the new age with a "new time" was to symbolize the total discontinuity between past and present. Since the flow of history is continuous, the selection of those historical events which mark the "beginnings" of chronological eras is a purely arbitrary

one. Usually these events are charged with profound social connotations, as the First Olympiad was to the ancient Greeks, the Creation of the World to the Jews, the foundation of Rome to the Romans, the Hegira to the Mohammedans, the March on Rome to the Fascists, and so on. As a most meaningfully-charged cultural symbol, the year 1792 seemed to be a perfect choice for the new "point of departure" and the fact that, although the establishment of the new era was decided upon only in 1793, it began retroactively in 1792, attests to the particular symbolic significance of the latter. The substitution of the date of the foundation of the French Republic for that of the birth of Christ as the standard temporal reference point of the new era symbolized the de-Christianization of the standard dating framework. It implied that France had entered a new, secular age, in which the foundation of the Republic was to be regarded as far more historically significant than the beginnings of Christianity.

Naturalization

A purely physico-mathematical analysis of the Revolutionary Calendar, which would ignore the poet Fabre d'Églantine's nomenclatural innovations, would never provide us with an adequate basis for grasping its full social meaning. Within a sociological perspective, which recognizes the great extent to which names are charged with meaning (Cassirer, 1953:44-62), calendrical systems ought to be regarded as products of the interaction between mathematical divisions of time and symbolic frameworks of meaning. It is in this light that one should view the naming of the new months after seasonal phenomena,⁸ as well as the abolition of the Church's saints' days and the renaming of the days of the year after phenomena in Nature instead. The new calendar was to symbolize the centrality of natural phenomena to the life of the new society, thus expressing the belief of the French Enlightenment in the need for

⁷ For an empirical study of the way in which the continuous flow of time is interrupted through the introduction of *social cycles*, see my analysis of the rhythmic structure of organizational life (Zerubavel, forthcoming: ch 1).

⁸ Consider also, in this regard, the Nazis' attempt to substitute Teutonic archaisms for the names of the months (Grunberger, 1971:446).

man to be in harmony with Nature. In the case of the months, names were designed so as to suggest Nature in a way that even transcended their mere referential function. By means of a morphological device, namely a differential application of suffixes ("aire," "ôse," "al" and "dor"), both the rhythm and the tone of these names suggested that they were grouped in four distinct three-month blocks, each of which was associated with a particular season. Thus, by both semantic and morphological means, the new nomenclature established a seasonally-based differentiation between the months, symbolizing the unbreakable bond between the *social* calendrical system and the cycles of *Nature*.

One of the most remarkable accomplishments of the calendar reformers was exposing people to the naked truth, that their traditional calendar, whose absolute validity they had probably taken for granted, was a mere social artifact and by no means unalterable. However, the reformers must have realized the possible boomerang effects of that, since it was impossible to expose the conventionality and artificiality of the traditional calendar without exposing those of any other calendar, including the new one, at the same time. One could not help realizing, for example, that if the seven-day week did not derive from the heavens and was merely an arbitrarily imposed convention, so was the ten-day *décade*. The new calendar could not be viewed as absolutely valid, since its users witnessed its birth and knew that things used to be different in the past.

Nevertheless, upon introducing the Revolutionary Calendar, its architects attempted the nearly impossible, namely, convincing their contemporaries that calendars in general were merely conventional artifacts (to account for the disposability of the traditional calendar), yet that their new one was not! However, in order for the Revolutionary Calendar to be accepted as absolutely valid, it first had to be viewed as an inevitable development and by no means a mere arbitrary convention. The reformers' way of achieving that was by associating their calendar with Nature. In order to suggest that it reflected

the natural order and drew its authority from it, the new months were named after seasonal phenomena. Along the same lines, a symbolic association between the new social order and Nature was attempted through the use of the co-occurrence of the foundation of the French Republic and the autumnal equinox. Though it was probably a mere coincidence that this unique historical event took place on a day which happened to be so significant in Nature, the reformers nevertheless could legitimize their choice of September 22 as the principal temporal reference point in the new calendar on the basis of establishing a harmony between the social and the natural. The symbolic implications of the multiple significance of this date—on which the sun also entered *Libra*, which is symbolically associated with equality—did not escape the attention of the calendar reformers. When it proposed to synchronize the annual social cycle with a cycle which derived from the rhythmicity of Nature, the Committee of Public Instruction stated explicitly that "the equality of the days and nights was marked in the heavens at the same moment when civil and moral equality was proclaimed by the representatives of the French people as the sacred foundation of their new government" (Andrews, 1931:526-7).

Rationalization

In order to establish the legitimacy of the new system of units of time, the reformers also tried to make it more "rational" and "scientific" through the application of the *decimal system*. Whereas calendars traditionally had been designed by the priesthood, the Revolutionary Calendar was intended to be an unprecedented project rationally designed and adhering solely to the principles of Reason and Science, as advocated by the French Enlightenment. Accordingly, its chief architect, Charles-Gilbert Romme, was a former professor of physics and navigation, whose consultants included scientific authorities such as the mathematicians, Monge and Lagrange; the astronomers, Lalande and Pingré; the chemist, Fourcroy; and other

distinguished members of the Academy of Sciences. It was no mere coincidence that Monge and Lagrange were also members of the committee who had introduced, only a year before, a major reform of the system of weights and measures. In fact, the calendrical reform actually was viewed as an extension of the metric reform, and, accordingly, it adopted the *decimal principle* as the basis of the new system of units of time. The act of adopting the very basis of our counting system in order to systematize the relations between the new weeks, days, hours, minutes and seconds involved much practical convenience,⁹ yet it also had some important symbolic implications as well. Just consider the religious connotations of the number "7" within the Judaeo-Christian tradition, for example, and, on the other hand, the rationalism and scientism associated with the decimal principle, which is among the cornerstones of Western mathematics.

A similar attempt to associate the new calendar with Reason was the naming of the days of the *décade* according to their numerical order. This act symbolized the replacement of the qualitative conception of time—which was associated with the traditional, religious and nonrational—by a *quantitative* orientation towards it, as became an Age of Reason. The new units of time were regarded as certain "quantities" of duration, which were systematically related to one another in mathematical terms, namely, as multiples of one another. With the abolition of the saints' days, Sunday and the Church's religious holidays, the days of the week and the year lost much of their unique character and, as mathematically equal entities, they became far more interchangeable with one another.

The *systematization* of the relations among the various units of time was also furthered by the uniformization of the months and the replacement of the seven-day week by the ten-day *décade*. Aside from saving the users of the new calendar the trouble of memorizing the

number of days in the various months of the year, the reformers also accomplished an unprecedented concordance of monthly and "weekly" cycles by establishing a mathematically regular relation between the month and the *décade*. Within the Western standard temporal reference framework, the weekly and monthly cycles are based on incompatible rhythms, since the former runs independently of the latter. Whereas years consist of a number of *complete* months, so that the beginning of a year coincides with that of a month,¹⁰ weeks do not have to be "completed" in order for a new month to begin. The implication of this for everyday temporal reference is that "weekly" and "monthly" dates are independent of one another, so that a calendar is necessary in order to reckon on what days of the month Thanksgiving Day, Election Day or Labor Day fall, or on what day of the week Christmas falls, in any given year. In the French Republican Calendar, however, the fixed mathematical relation between the month and the *décade* (1:3) made the *synchronization* of these two cycles possible through fixing the beginning of the month on *Primidi*. The user of this calendar could convert dates which were formulated in terms of the day of the month into dates formulated in terms of the day of the *décade*, and vice versa, simply by referring to the last figure of the former or the mere name—which also reflected the numerical order—of the latter. The tremendous contribution of this simple, yet sophisticated "rational" arrangement to everyday temporal reference can hardly be overemphasized.

THE FAILURE OF THE REFORM

As it turned out, the French Republican Calendar did not prove to be a success and, on January 1, 1806, the Gregorian Calendar was restored by Napoleon. One main reason for the failure of the calendrical reform was probably its overemphasis on the totality of the obliteration of the

⁹ For a discussion of the convenience involved in applying the decimal principle to social organization, see Simmel (1950:171-4).

¹⁰ The same is true about the relations among months, days, hours, minutes and seconds. As Colson (1926:2) has noted, the seven-day week is the only "intruder" in this ordered system.

traditional system of units of time and time-reckoning and dating frameworks. Given the popularity of any standard temporal orientation, it is quite likely that the reformers would have been far more successful had they incorporated parts of the old sociotemporal order into the new one. By suggesting some symbolic continuity between the two, they would probably have contributed to further the *legitimacy* of the new system. As the recent case of the transformation of the meaning of the British "penny" (in terms of its mathematical relation to the £) while preserving its traditional name suggests, such an approach might have avoided hurting traditionalist sentiments, thus facilitating the acceptance of the new symbolic order. However, the metric reform of the system of weights and measures, which was introduced in France at about the same time, clearly achieved a tremendous success despite its absolute obliteration of the traditional order. While very few people today are even aware of the fact that the French Republican calendrical reform ever took place, the metric system is still in use at the present time, not only in France, but in most of the Western world. The failure of the calendrical reform is even more striking, therefore, when contrasted with the success of its sister reform, and it certainly cannot be fully accounted for only by the totality of the obliteration of the old order.

The calendar reformers' overemphasis on secularization was another major factor which probably hindered their potential success. When we contrast the failure of the Revolutionary Calendar with the tremendous success of the metric system, we must remember that the Revolutionary Calendar had to challenge the existing symbolic order associated with the sacred—such as disposing of Sunday and the Church's saints' days—whereas the metric system did not have that problem.¹¹ The fact that the restoration of the Gregorian Calendar by Napoleon was

actually a part of his general reconciliation with Rome clearly suggests that the failure of the calendrical reform must be viewed within the context of the overall failure of the French Revolution to de-Christianize France. The reformers must have underestimated the depth to which religious sentiments were still rooted among the French people, many of whom probably found it impossible to depart so abruptly from a traditionally sacred symbolic order which had been taken for granted for centuries.

Another major factor which proved to be detrimental to the calendrical reform's chances of survival was its pronounced *particularistic* spirit, which was manifested in the nationalistic symbolism of names such as "*Franciade*" and "*Sansculottides*" and dates such as "September 22" and "1792." The symbolic function of introducing a unique system of units of time and unique time-reckoning and dating frameworks is very similar to that of introducing a unique national anthem or flag.¹² However, whereas a multiplicity of national anthems or flags does not generate any confusion or inconvenience which might hinder international communication, a *multiplicity of temporal reference frameworks* certainly does. There was no way to synchronize the Revolutionary and Gregorian time-reckoning and dating frameworks, and any date within the former was totally meaningless to users of the latter in other countries. The days of the *décade* could not correspond to those of the week, since these two functionally analogous social cycles were based on totally incompatible rhythms. As for the numerical names of the days of the Revolutionary and Gregorian months, although these two cycles were based on very similar rhythms, their beginnings did not coincide, and all the Revolutionary months began sometime around the 20th day of the Gregorian month. Finally, the numerical expression of the Revolutionary year was also mean-

¹¹ It is interesting to note, however, that those societies in which the metric system has not yet gained a stronghold tend to cling to their non-decimal monetary, measure or weight systems as sacred symbols of their traditions.

¹² To take the case of the Jewish Calendar in modern Israel, for example, most of the debates over whether to keep it revolve around the nationalistic symbolism involved in a unique standard temporal reference framework.

ingless to anyone who did not consider the foundation of the French Republic the most significant event in history. Even the official *Moniteur* had to insert the Gregorian date in brackets after the Revolutionary date (Andrews, 1931:528, 531n), and contemporary historians must use concordance lists (Herrmann, 1938) in order to "translate" Revolutionary dates so as to locate them in conventional chronology.

That the calendrical reform entailed France's international isolation, whereas the metric reform did not, need not surprise anyone who appreciates the functional uniqueness of the clock and the calendar in Western civilization. The remarkable sophistication in the popular development of time-measurement and time-reckoning devices in the West has no parallel in the domain of measures and weights, and most children are introduced to the clock long before they learn how to use a ruler or a scale. Whereas spatial markers such as maps, train stop and street signs, and elevator floor indicators are needed only upon relocation in space, *temporal markers* such as watches and desk calendars are referred to much more frequently due to the inevitability of our constant mobility in time. Modern social life would have been impossible without *temporal coordination*, which presupposes a generally accepted standard temporal reference framework.

Thus, it may very well be that the French Republican calendrical reform was also less successful than the metric reform in gaining popular acceptance because the abrupt transformation of the standard spatial reference framework threatened modern social organization less than did that of the standard temporal reference framework. The reformers challenged the latter when it was already adopted by most of the Western world, in an age when international communication already presupposed temporal coordination on a global level. It was that factor, as well as the reformers' overemphasis on total obliteration of a traditional sociotemporal order symbolically associated with the sacred, that spelled doom for the French Republican Calendar. It should be noted that its eventual abolition actually marks

the failure of the most radical attempt in modern history to challenge the Western standard temporal reference framework, which is, until this day, among the cornerstones of modern social life.

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CHINESE CONSUMPTION OF FOREIGN COMMODITIES: A COMPARATIVE PERSPECTIVE*

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A recurrent theme in the literature on modernization is the importance of Westernization in non-Western societies—the purchase of Western commodities and the emulation of Western styles. This paper critically examines this theme in relation to a negative case, the lack of Westernization in nineteenth-century China. Three different theories of consumption that have been offered to explain the nonacceptance of Western products in China—an economic theory, a cultural theory and a sociological theory—are analyzed from comparative and historical perspectives. On the basis of this analysis, it is suggested that Westernization during the nineteenth century can often be viewed more accurately as an indicator of stratifactional continuity with the premodern era than as an indicator of modernizing changes.

With considerable justification, Karl Marx and many later writers believe that a decisive factor in the nineteenth-century expansion of Western civilization was the distribution of commodities produced by capitalist factories. Marx (1959:11) says, "Cheap commodity prices are the heavy artillery with which [the bourgeoisie] batters down all Chinese walls and forces the barbarians' intensely obstinate hatred of foreigners to capitulate. . . . It compels them to introduce what it calls civilization into their midst, i.e., to become bourgeoisie themselves. It creates a world after its own image." Nowadays, students of society seldom accept such simplicity in their explanations. Nonetheless, most recent theories recounting the economic factors of modernization prominently in-

clude, as a causal mechanism, the distribution and sale of Western products in non-Western societies. The more sophisticated theories no longer rely exclusively on the difference in price between factory-made products and handicraft goods. Instead, they emphasize such factors as the restructuring of market relationships (e.g., Smelser, 1963), the establishment of primary trading cities (e.g., Murphey, 1969) and the subordination of non-Western producers (e.g., Frank, 1967)—all of which are subsidiary to the creation of a world marketing system (Wallerstein, 1974; Eisenstadt, 1973). Despite such insightful and justified elaborations, *one* of the causal mainsprings by which this process of Western expansion *began* remains the non-Western consumption of Western goods.¹

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¹ This causal mechanism is illustrated in such studies as Wallerstein's (1974), where Eastern Europe (the periphery) buys manufactured commodities from and sells agricultural products to Western Europe (the core), and Gallagher and Robinson's (1953), where England builds an informal

Granted the ubiquity of this causal theme, surprisingly little is known about the factors influencing non-Western use of Western commodities.² This paper will explore some of these factors by focusing on what is regarded by many as a negative case—nineteenth-century China. It is well-known that the Chinese, during the height of Western commercial expansion (1860–1930), did not consume Western products readily or abundantly. It is equally well-known that the lack of Western influence in China goes far beyond the non-consumption of a few factory-made commodities. In fact, some suggest that, of all the non-Western agrarian societies in the nineteenth century, China was the least influenced by the impact of Western civilization (e.g., Jacobs, 1958; Nathan, 1972; Murphey, 1974). Concentrating on a very narrow aspect of this impact (i.e., the lack of interest in Western products) can (1) suggest some factors that influenced the acceptance of Western commodities in non-Western societies and (2) evaluate some approaches towards understanding larger, and obviously more complex, issues of Westernization during the nineteenth century.

Three sets of explanations will be outlined; each attempts to account for Chinese lack of interest in Western commodities: the “faulty marketing and merchandise,” the “culture” and the “status competition” explanations. The “faulty marketing and merchandise” explanations contend that Western products could not overcome the competition of the traditional economy; hence the Chinese, being rational consumers, wisely purchased Chinese products. The “culture” explanations assert that China possessed a pervasive, socially integrating culture, whose

symbols were known and approved by all; hence the Chinese, being highly ethnocentric and xenophobic, exclusively consumed tokens of their own culture. Nineteenth-century observers and modern scholars primarily use one of the two or both to explain Chinese non-consumption of Western goods. The “status competition” explanation—very rarely, and then only implicitly encountered in the sources—maintains that the Chinese conception of high social status was more influenced by local and rural than by national and cosmopolitan standards; hence the Chinese preferred to consume commodities having established meanings of social status rather than those having exotic symbolic content. Each of these explanations will be examined from a comparative perspective in order to judge their usefulness in explaining the Chinese case. In order to approximate roughly the range of conditions within Chinese society, I will limit my comparisons to non-Western agrarian societies.

Limited space will confine my explanation to the non-Westerners’ consumption of Western-made, primarily British, textiles. This choice is advantageous. Textiles rank as the most prominent export from nineteenth-century capitalist factories; textile production laid the initial industrial foundation for England and for other Western societies; and, most important, because the Chinese did not purchase much Western cotton, the Blackburn Chamber of Commerce (composed of Lancashire textile manufacturers) commissioned an investigation to discover the reasons. The report from this investigation gives the most detailed information available on the conditions of the textile trade in the interior of China.³

empire out of non-colonialized countries (e.g., Latin America) by maintaining commercial suzerainty over the territory, thus ensuring an overseas market for British goods.

² The lack of interest in this area can be explained, in part, by the sociologist’s general lack of interest in the process of consumption. As Smelser (1976:131–40) remarks, “consumer behavior” typically has been a topic of interest only for economists. There are, of course, some exceptions. Besides those sociologists mentioned below, Veblen (1953) and Fallers (1966) provide notable contributions to the sociology of consumption.

³ The Blackburn Commission traveled some 4,300 miles through the interior of south and central China between October, 1896 and June, 1897. They collected detailed information on all factors relating to how the Chinese met “their own wants in the matter of textiles” including material on marketing, distribution, indigenous handicraft production, taxation and “local customs and usage as they might affect our textile industry” (Neville and Bell’s Section, 1898: Preface). The report was published in two lengthy sections, one written by H. Neville and H. Bell and the other by F. S. A. Bourne, a member of the British Consular Service, under the title, *Report*

*The Consumption of Foreign
Commodities during the Ch'ing*

That the Chinese did not purchase large quantities of Western products in the nineteenth and early twentieth centuries is a well-known fact. The extent to which the Chinese did not buy Western goods is not so well-known and bears repeating briefly.⁴

One of the few quantitative measures of the consumption of Western products is the record of imports entering through specific trading centers. When used as an indicator of Chinese consumption, such statistics, even if completely reliable, represent the maximum figure possible. For China, however, these statistics are known to be greatly inflated for several reasons. First, because of extensive transshipment of goods between reporting stations, there is a significant but unmeasured amount of duplication in the figures. Second, Westerners and their staffs, who lived in the treaty ports, purchased a sizable portion of the imports. Third, as more treaty ports opened, the statistical coverage included previously unreported imports, thus exaggerating the rate of growth. Fourth, the value of imports was reported in Chinese taels, whose value declined between 1870 and 1930 by about two-thirds. Fifth, nearly a third of the total imports into China after 1920 went to Manchuria, a separate economic system developed and tightly controlled by the Japanese. Last, drug and foodstuffs from Southeast Asia—such as rice, birdnests and sharkfins—constituted between twenty to thirty percent of the total amount of imports.

Based upon these qualifications, Murphey (1974:48–9) estimates that, although recorded foreign imports increased between the years of 1870 and 1930 by a factor of thirty, the actual increase was “perhaps three or four times at most.” Even if the statistics are not corrected to take these factors into account, the total dollar amount of imports places China on

par with such countries as “Mexico, Chile, New Zealand, Brazil and the Union of South Africa, and far behind Argentina, Australia, Canada, Denmark, the Federation of Malaya, India, Indonesia, or of course any of the industrialized countries of the West, or Japan.” If these figures are computed in terms of per capita consumption, then Murphey’s assessment is not an exaggeration: “China’s importation of foreign commodities remained negligible, and even at its peak probably smaller than that of any country in the world, including Tibet.”

*The Faulty Merchandise and Marketing
Explanations*

Although composed of many separate explanations, the faulty merchandise and marketing explanations category has a central theme: Western products were simply unable to overcome the competition posed by indigenous products. An important corollary to this theme maintains that the imposition of Western imperialism in China was incomplete. Western powers did not colonize China. Although they divided China into vast “areas of influence,” Westerners lived in only a few coastal cities, such as Shanghai, and in China’s capital, Peking. They did not disrupt the existing organization of the Chinese economy and used traditional means to market their products.

In elaborating this theme, the faulty merchandise and marketing explanations cover two overlapping, but analytically distinct, areas: (A) Western products were more expensive and of poorer quality than comparable handicraft products. (B) The traditional commercial system hampered the distribution and marketing of Western products.

(A) Many analysts believe that the principal reason Western cottons sold poorly in China was that handicraft cottons were cheaper and better suited to the needs of the Chinese (Cooke, 1858:187; Neale, 1862:383; Nathan, 1972; Reynolds, 1974). The evidence presented by the Blackburn Commission, based largely on interviews and observations, seems to support this explanation. The Commission observed (Neville and Bell’s Section, 1898:256) that

of the Mission to China of the Blackburn Chamber of Commerce.

⁴ For these comments, I am closely following the discussion of Murphey (1974:47–9); also see Feuerwerker (1969).

the majority of Chinese, the peasantry, engaged in outdoor manual labor and, therefore, required durable, warm clothing. Because they were tightly woven, using a fine thread, Western cottons lacked the durability and warmth of the coarser native cloth. A smart consumer, they reasoned, would purchase native cottons. Many modern students of China follow the same line of reasoning, describing the Chinese peasant as a "rational economic man": "A great deal of the Chinese reluctance to buy foreign goods or to adopt foreign business methods or technology was *entirely rational and not culture-bound*: traditional Chinese goods and methods were equal or superior, especially in cost terms" (Murphey, 1974:30, emphasis added).

The quality of native cotton notwithstanding, the Blackburn Commission was especially concerned about its cheapness in comparison with the cheapest lines of Western fabrics. In seeking an explanation for this, the Commission made what they considered to be an important discovery: the relative cheapness of native cottons was the result of the importation of huge quantities of inexpensive cotton yarn from Indian cotton mills in Bombay (Neville and Bell's Section, 1898:210-9, 234-5). The availability of this yarn had caused a "revolution" in weaving (Neville and Bell's Section, 1898:271). Wherever they traveled, the Commission found cotton handicraft industries, which once had been concentrated in the cotton-growing regions of the Yangtze river basin, spreading throughout China. Much of the increase in production, they discovered, was not for home consumption but was for sale in the marketplace. In some areas, such as South Yunnan, the Commission found that most of the local population, long accustomed to buying cloth in the marketplace, did not know how to weave before Indian cotton yarn became available. But with the importation of this yarn, many attended classes in weaving in order to take advantage of the sudden opportunity to profit from handicraft production (Neville and Bell's Section, 1898:261-6). This increased the supply of handicraft cottons, causing their price to decline.

This explanation is appealing, for it would tend to support the contention that factory-made commodities were purchased because they were cheaper than handicraft goods, but when they were more expensive, they were not purchased. Hence in China, the exceptional society in which Western commodities were more expensive, they did not sell.

There are, however, some shortcomings to this explanation. It is questionable whether handicraft products, especially textiles, in other non-Western agrarian societies were more expensive than Western goods. Although information is limited, we do know that handicraft industries in many societies survived the nineteenth century and, indeed, some survive even today.⁵ The important point here is not the cheapness of the commodities but who consumed them. The above explanation is designed to explain why the poorest Chinese, the peasants, did not buy Western fabrics. But in other non-Western agrarian societies were peasants *the first, the only, or the most likely* consumers of Western fabrics? Platt (1973:82) maintains that during the nineteenth century, "For Latin America a large part of peasant consumption was met by home weaving, in cottons and especially in woollens. . . ." Morris (1968) makes the same observation for India. Based on this, it seems plausible that, in those societies where peasants lived more on the edge of subsistence than they did in China⁶ and where the marketing system was less developed than it was in China, peasant populations would be even less likely to consume Western fabrics than were Chinese peasants. Moreover, based

⁵ For example, considerable handicraft production continues today among the highland Indians of Central and South America (Orlove, 1974), throughout sub-Saharan Africa (Plumer, 1971), in Indonesia (Adams, 1969), in Thailand (Ingram, 1955:118-23) and in India (Gandhi, 1931; National Council of Applied Economic Research, New Delhi, 1959; Morse, 1908; Raychaudhuri, 1968).

⁶ Although the point is certainly subject to debate, recent research is showing that the Chinese per capita income was quite high by world standards, perhaps as high as that in any society in the world prior to the Industrial Revolution. See, for example, Murphey (1974:32-43), Myers (1970) and Perkins (1969).

on ethnographic data, one has every reason to think that a great many peasants, Chinese and otherwise, would not be disposed towards the use of Western fabrics. For instance, we know that in many places, peasant communities often used distinctive textile designs to differentiate their work from that of other peasant communities (e.g., Primov, 1974). Nineteenth-century Western manufacturers did not attempt to imitate *most* of those designs.

This explanation seems even less reasonable, then, if one hypothesizes that it was the richer, not the poorer members of non-Western societies who were the first, and continued to be the most likely consumers of Western piece goods. Indeed, this hypothesis would agree with most nineteenth-century observations about who wore Western-style clothes. Though impressionistic, the documentary evidence is fairly clear that in Latin America, Turkey (Ottoman Empire), Southeast Asia and Japan, it was members of elite groups (e.g., the mestizos, the aristocrats, and the colonial indigenous leaders) who first dressed in Western fashions (e.g., Halperin-Donghi, 1973:85-90; Yanagida, 1957:9-30). Thus, one can suggest that Western textiles were initially luxury products whose comparative cheapness, when compared with other luxury fabrics, placed them within reach of other aspiring groups of individuals (e.g., merchants) who quickly emulated the dress of the socially higher ranked.⁷

If these qualifying remarks are accurate, then the fact that Chinese peasants did not buy Western piece goods because they lacked durability and were too expensive is also a fact among peasants in

many other non-Western societies. Thus, it does not explain why China consumed fewer Western fabrics than other countries. In fact, it complicates the question considerably because one must now explain why the richer members of Chinese society, whose attire differed greatly from that of peasants, did not consume Western fabrics.

(B) A possible explanation for why the more wealthy did not consume significant quantities of Western piece goods is that the traditional marketing system was simply too vast for Western goods to reach potential customers. Although one often finds this explanation in nineteenth-century sources (e.g., Cooke, 1858:202-6) as well as in modern studies, it contains some shortcomings when evaluated from a comparative perspective. Murphey (1974) and Feuerwerker (1969), among others, argue that in China an important factor in the lack of foreign trade was the inability of the Westerners to create a "modern" marketing system. Western commodities trickled into a fully-developed, well-supplied traditional economic system that was totally controlled by Chinese merchants. Western merchants were unable to obtain an adequate base from which to promote their products, so much so, says Feuerwerker (1969:57), that "foreign trading firms [were] gradually transformed into Shanghai and Hong Kong commission agents serving the established Chinese commercial networks."

The reasoning behind making the correlation between China's exceptionally efficient traditional marketing system and the limited Chinese demand for imported goods is ambiguous, at best. During the nineteenth century in most complex agrarian societies, non-Western commercial firms gradually replaced Western commission agents in handling textile imports. What happened in China is in no way exceptional. Simply put, Western merchants typically did not venture beyond the major trading centers to establish wholesale or retail outlets. Native firms, which had formed the link between the Western commercial houses and retail outlets, began to deal directly with the manufacturing firms. This change occurred

⁷ From this perspective, the textile handicraft industries supposedly ruined by Western capitalism were the ones that produced the more expensive fabrics consumed by the upper classes. This hypothesis also accords with such information as we possess. For instance, in South Asia the large textile handicraft industries which were hurt by Western competition were those that produced fine fabrics for the export and local elite markets (Chandra, 1968:52-62; Raychaudhuri, 1968:93-4; Halperin-Donghi, 1975:54). Too often this handicraft industry mistakenly is equated with the handicraft industries producing for peasant consumption, some of which survive today.

throughout the Ottoman Empire, Latin America, Russia, Africa and Southeast Asia (Platt, 1973; Skinner, 1958; Wickberg, 1965). Far from limiting the demand, the takeover by non-Western merchants intensified the distribution of Western cottons. Whether these developments in other non-Western societies mark the beginning of a "modern marketing system" or not begs the question.

For China, however, it is abundantly clear from the investigations of the Blackburn Commission that Western cotton fabrics and Indian cotton yarns were found throughout China, even in minor markets in the interior. Every other import that the Chinese showed they would consume (e.g., kerosene, cigarettes, matches) was likewise distributed throughout China. In fact, it should be argued that this vast and well-ordered traditional marketing system was better able to place Western imports in front of potential consumers than the marketing systems in most other non-Western societies, whether a "modern marketing system" or not. For the Westerners, the problem was that the Chinese simply would not accept much in the way of imports.

Nonetheless, the traditional economic system did have a direct effect in lowering the amount of imports sold in China. The reason, however, was not that it limited consumer demands but, rather, that it increased the supply of products first imported from the West. Those imports the Chinese did accept were constantly subjected to competition from native products produced in imitation of the imported item. Indigenous production of cigarettes, opium, matches, kerosene, factory-made cotton yarn, cotton piece goods, and even particular economic services (e.g., steamship transportation) rapidly gained a sizable portion of the market in competition with the respective import (Murphey, 1974; Feuerwerker, 1969; Morse, 1908:343-50; Liu, 1962: 151-6). The productive capacity of the Chinese economy certainly limited the *amount* of foreign commodities imported, but not the *range* of imports the Chinese would accept in the first place.

In summary, the faulty merchandise and marketing explanations attempt to ac-

count for the non-consumption of Western goods in terms of a universalistic consumer decision model: Economic man, rationally satisfying his needs, decides between competing items by weighing quality against cost—factors which, in turn, depend upon elements of production and distribution. When using this approach, one, of course, synthesizes a great deal of information about the Chinese use of Western imports. The approach, however, does not explain why the Chinese did not consume a wider range of Western products. What it does provide is a partial explanation for why the Chinese did not consume more of those items they found acceptable.

Perhaps one of the reasons for the limited explanatory value of this approach is because the question to be answered is essentially a comparative question: Why did the Chinese consume fewer and a smaller range of commodities than did people in other non-Western societies? In order to fashion a valid answer when using the universalistic consumer decision model, one must demonstrate that the economic factors influencing consumer decisions for the Chinese were *substantially different* from those factors in other societies. The faulty merchandise and marketing explanations do not demonstrate these crucial differences.

The Culture Explanations

Whereas the former explanations are based upon universalistic models, the culture explanation is based on a particularistic one. Explanations constructed from this model only apply to a specific culture—in this case, China. Of the three categories of explanations suggested in this paper, this category is the one most often used to account for the Chinese lack of interest in Western goods and, of the three, it is the most ambiguous.

The main tenets are easily described. China possessed a pervasive, well-articulated culture composed of a homogeneous set of institutionalized values emanating from the "Great Tradition" of Confucianism. Those who maintained this tradition were the upper class, educated and examined in its literature.

This upper class, the literati, indoctrinated other societal members in its prescriptions and set an exemplary standard for other groups to emulate. Because of the widespread geographical distribution of the literati and because this class (the literati) was not hereditarily determined, Confucian culture formed an especially powerful value-integrating orientation. In this role, Confucianism supposedly provided the principles through which Chinese society functioned.

There are typically two ways that this and similar characterizations of Chinese culture are used to explain China's non-consumption of Western imports. First, the literati were the model by which other Chinese judged appropriate behavior. Since it was in the interest of this group to consume the symbolic representations of Confucian culture, other social groups emulated their pecuniary canons of taste. Thus, this standard of consumption necessarily excluded Chinese interest in Western commodities. In this regard, Wright (1962) says, "In China, where imports of consumers goods were predominant, there are no signs that these imports reflected an undermining taste for foreign luxuries or any desire to ape foreign ways. . . . The Chinese upper classes, being non-Western, serenely preferred the tokens of their own culture." The second and closely related variant of the culture explanation suggests that because of the believed superiority of Chinese culture, Chinese were extremely ethnocentric, disliking all foreigners and, by default, foreign-made commodities.

The Blackburn Commission gives considerable indirect evidence that would seem to support this explanation. They recommended that Lancashire manufacturers not try to produce cloth for poor Chinese. Instead, because Western cottons were cheaper than silks and the better grades of handicraft cottons, a more appropriate market should be upper-class Chinese. They noted, however, that wealthy Chinese thought Western cottons to be inferior when compared with these native fabrics; if purchased at all, they were used to supplement, not to replace the more expensive native textiles. Decisive in this regard was the high status

associated with wearing silks: "Every individual [wears] silken fabrics if the nature of his work and his means enables him to do so" (Neville and Bell's Section, 1898:216). Thus, even though silk might sell at prices slightly higher than Western fabrics, its "consumer appeal" was much greater. The same sort of evaluation as that placed on silk also was placed on the best grades of handicraft cottons, especially those made entirely of Chinese cotton (Neville and Bell's Section, 1898:255).

More important, the Commission recommended that the Lancashire cotton manufacturers give the Chinese precisely what they wanted. The Chinese, they maintained, patronized commodities because they met Chinese cultural standards, not simply because they were Chinese-made. Hence, the Commission strongly advised that Chinese designs be accurately reproduced.

ornament applied to woven fabrics is in all cases symbolical to the Chinese mind. . . . A descriptive classification is attached to all products of the weaver's art by the Chinese themselves, and unless our manufacturers of cotton brocades pay attention to such minute detail they cannot hope to maintain their interests in these markets. . . . It is no use attempting to give a Chinaman a three-toed dragon instead of one with five, or vice versa; and it certainly is not a wise proceeding to irreligiously tamper with the traditional figure "motifs" of applied Chinese art. (Neville and Bell's Section, 1898:288)

The Commission even found that the Chinese dyed or in some way refinished an extremely large portion of imported cloth sold in China (80 to 90% of the English-made bleached shirting), with most of the remaining imports being used "for *under clothing*, or as *linings* to the various garments" (Neville and Bell's Section, 1898:289, emphasis added). Somewhat disheartened, they remarked, "It is not that these goods lack anything in bleach or finish—they are as perfect as good machinery and highly-developed skill can make them, it is that though [bleached shirtings] approach the Chinaman's requirements most nearly of anything we send him, yet they are not exactly what he wants" (Neville and Bell's Section, 1898:294).

The culture explanations are likewise appealing. They seem to account for why wealthier Chinese would not consume commodities symbolizing a non-Confucian way of life. The acceptable imports were thus those items which lacked cultural content (e.g., matches and cigarettes) or could be transformed into commodities having significance within Chinese culture (e.g., imported cotton yarn made into Chinese-style fabrics) or could be concealed (e.g., imported cotton cloth used as lining material). Yet, as acceptable as these explanations seem to be, they contain some ambiguities when examined from a comparative perspective.

The question is why should Chinese culture, at this point in time, *cause* more ethnocentrically-oriented patterns of behavior than did cultures in other non-Western societies? Was Chinese culture stronger or better articulated than were cultures elsewhere? The culture explanations are difficult to use in answering such questions because "culture" tends to be viewed as an indivisible whole. Inseparable aspects of the whole, such as the principles of propriety and filial piety, are considered unique to the particular cultural civilization. Thus, when comparing two cultures, one contrasts traits of two unique, holistic configurations. This makes systematic comparisons of similarities and differences impossible. Instead, what results from such comparisons is a tautological statement that people of particular societies do what they do because it is their cultural way to do so. In the case of Chinese culture, this problem of analysis is heightened by the belief that principles of Confucianism have had a continuous effect upon Chinese behavior for over two thousand years. In this role, Confucian culture becomes a "first cause," the unmoved mover of all that has happened in China. As Popper (1964) shows, for the purposes of analysis, such conceptualizations are useless because they cannot be falsified.

These objections notwithstanding, the culture explanations can be questioned on the basis of *intracultural contrasts*.⁸ For

well over the first thousand years of the imperial period, the Chinese regularly and widely consumed far more imports than they did during the last two dynastic periods, the Ming (A.D. 1368–1644) and the Ch'ing (A.D. 1644–1912). Moreover, most students of China consider the Han (202 B.C.–A.D. 220) and particularly the T'ang (A.D. 618–907) and Sung (A.D. 960–1279) dynasties to be high points in China's political, social and cultural achievements. The Ming and Ch'ing periods, however, are often viewed as a long period of decline in all these respects.

Because of several excellent studies, we know more about the consumption of foreign commodities during the T'ang than during the Han and Sung dynasties. In the T'ang period, quite unlike in the Ch'ing, the Chinese consumed an extensive variety of imports whose very acceptance depended upon their exotic appeal, their "foreignness." According to Schafer's (1963:28) study:

The Chinese taste for the exotic permeated every social class and every part of daily life: Iranian, Indian, and Turkish figures and decorations appeared on every kind of household object. The vogue for foreign clothes, foreign food, and foreign music was especially prevalent in the eighth century, but no part of the T'ang era was free from it.

The staples of foreign trade during the T'ang included many different kinds of foreign aromatics, drugs, woods, foods, and jewelry and a host of spectacular esoterica ranging from foreign birds and wild animals to slaves, musicians and dancers. Among the imports were foreign textiles and, more important for the purposes here, foreign clothing styles: "Fashions in the two capitals [of China] tended to follow Turkish and East Iranian modes of dress." Popular among men were such items as Turkish riding clothes, boots and "barbarian" hats and among women, veils, fitted blouses, pleated skirts, "long stoles draped around the neck and even

suggesting that the political, social and economic organization of the two periods were essentially the same. I am suggesting that culture explanations need to be specified. Whatever their shortcomings in this respect, temporal comparisons are a way to hold culture constant, thus aiding specification.

⁸ In the following temporal comparison, I am not

hair styles and makeup of 'un-Chinese' character" (Schafer, 1963:24).

The T'ang dynasty may be considered atypical by some because of the extensive influence of non-Confucian doctrines then promulgated by the imperial rulers. But during the Sung dynasties, at the height of both neo-Confucianism and literati authority, foreign trade was every bit as extensive as it was during the T'ang. In fact, the records of Chau Ju-kua (Hirth and Rockhill, 1970) and the fact that Southern Sung obtained twenty percent of the total imperial revenues from foreign trade alone (Ma, 1971) suggest that foreign imports may have increased between the two periods.⁹

Considering this striking contrast in the use of imports between the first half of the imperial period and the last half, the analyst faces a dilemma. If one uses the culture explanations to explain the lack of import consumption during the Ch'ing, then why should these explanations not also apply to the T'ang and Sung eras? Does one account for the difference by suggesting that the culture changed, hence allowing for the validity of these explanations for the Ch'ing? Or does one hypothesize that it is not Confucian culture but something else which determines the prevailing patterns of consumption, thus invalidating the culture explanations? This is not an enviable choice. To say that the content (i.e., the principal tenets) of Confucian culture changed flies in the face of evidence to the contrary. But to suggest that Confucian values did not influence Chinese consumer behavior seems equally untenable. A possible solution to this dilemma is to hypothesize that a change occurred in the way Confucian culture was maintained and interpreted within context of Chinese society. In other words, the structure of Chinese society changed between the two eras and not necessarily the content of Chinese culture. This hypothesis is the substance of the status competition explanation.

⁹ A short description of clothing styles during the late Sung period given by Gernet (1962:127-32) indicates some foreign influence on the style of dress, but certainly not as much as that which prevailed during the T'ang dynasty.

The Status Competition Explanation

The final explanation is one that emphasizes social structural parameters of consumption rather than economic or cultural factors. Although seldom encountered in the literature on the lack of Westernization in China, this explanation furnishes a basis for differential comparisons, which, in turn, provide an understanding of Chinese consumer logic.

The model upon which this explanation rests is straightforward. Insofar as they are able, individuals rationally purchase what they socially desire. Social desirability is a product of an ongoing situational process in which individuals choose the content and style of their consumption predicated on (1) what is available in that context and (2) how they view themselves in relation to their contemporaries. Thus, how a person decides on the social desirability of specific ideal and material goods is *in part* a function of his membership or aspirations of membership in what Weber (1968:937) calls a "status group": "Status groups are stratified according to the principle of their consumption of goods as represented by special styles of life." From this perspective, consumption is directly related to a person's social status and his efforts to substantiate that status by consuming "a style of life" in agreement with or in opposition to significant others. Hence, the content and style of consumption can be seen as a component of status competition, that is, competition in terms of ideal and material goods with significant others in the substantiation of one's status claims.

The task of analysis is to understand the composition and structuring of status groups in a particular context and to relate these two factors to an individual's consumption of goods. In this regard, Alexis de Tocqueville's distinction between aristocratic and democratic societies is useful. According to Tocqueville, (1969:530-3), distinctive styles of consumption prevail in aristocratic societies.

The rich in aristocratic societies, having never experienced a lot different from their own, have no fear of changing it; they can hardly imagine anything different. The comforts of life are by no means the aim of their

existence; they are just a way of living. They take them as part of existence and enjoy them without thinking about them. The . . . human taste for comfort being thus satisfied without trouble or anxiety, their faculties turn elsewhere and become involved in some grander and more difficult undertaking that inspires and engrosses them.

However, in democratic societies very different styles appear.

When distinctions of rank are blurred and privileges abolished, when patrimonies are divided up and education . . . spread, the poor conceive an eager desire to acquire comfort; and the rich think of the danger of losing it. A lot of middling fortunes are established. Their owners have enough physical enjoyments to get a taste for them, but not enough to content them. They never win them without effort or indulge in them without anxiety. . . . There is no question of building vast palaces, of conquering or excelling nature, or sucking the world dry to satisfy one man's greed. It is more a question of adding a few acres to one's fields, planting an orchard, enlarging a house, making life ever easier and more comfortable, keeping irritations away, and satisfying one's slightest needs without trouble and almost without expense. These are petty aims, but the soul cleaves to them; it dwells on them every day and in great detail; *in the end they shut out the rest of the world.* (Emphasis added)

I have quoted Tocqueville at length because I think here is a clue in understanding the problem I have posed. Nineteenth-century China was obviously not a democratic society, though nineteenth-century Western observers frequently referred to it as democratic (e.g., Cooke, 1858; MacGowan, 1889). Nonetheless, in this period, Chinese society has many of the structural characteristics of those societies that Tocqueville described as democratic. China did not have an hereditary elite but, rather, one whose members obtained formal and informal power positions through achieved means ranging from education to personal connections (Ho, 1964). Moreover, this elite was not a national elite. It was a local elite, locally determined through informal means and locally oriented in its exercise of power (Ch'u, 1969: 168-92). The nexus of this elite was

the locale—the standard marketing communities (Skinner, 1964; 1976), the district (Ch'u, 1969) or sub-district units (Freedman, 1965). Although there is some scholarly disagreement over the unit of the locale, it is agreed that the locale had a considerable political and economic autonomy from the bureaucratic state. In fact, Skinner (1976) insightfully argues that the influence of the locale in determining the organizational parameters of Chinese society extended to people of all income levels and was a principal factor in shaping "mobility strategies" by which individuals could aspire to artisan, merchant and official positions. These organizations (e.g., lineage associations, secret societies, *hui-kuan*) existing at the local level typically, but not necessarily, crosscut class lines so that one could find both the wealthiest and the poorest members of the local communities in the same organization.

Within this local society, an individual measured his success in relation to the success of others in his locale. Such a determination was always ambiguous because there was no rigid hierarchy of formal positions denoting the attainment of success other than office holding, which, by regulation, could only occur outside one's local community. However, the assessment of one's relative success was very important because the ability to exert influence in local affairs was strongly associated with one's social standing (Ch'u, 1969:175-7). Hence, status competition was a complex and very serious undertaking that interlinked those recognized to be successful, those claiming success, those aspiring to success and those clearly recognized to be unsuccessful. One of the best ways to demonstrate success or a claim of success was to practice a style of life that those along the continuum from the successful to the unsuccessful could agree revealed high social status. It is in this regard that Confucian culture is important because it contained those symbols of success—the accouterments of living the life of a gentleman—upon which all could agree.

In this context, as the status competition grew more intense, the symbols of status became more fixed and the local

inhabitants of all classes became more knowledgeable about the meaning of these symbols. In fact, one might hypothesize that the intensity of this competition helped promote the fad for domestic esoterica which was so prominent in the late imperial period but was less pronounced in earlier periods.¹⁰ These esoteric items (e.g., snuff bottles, carved miniatures of all varieties, silk textile designs) were certainly luxury goods, but those with a "middling fortune" could afford many of the items and those with greater wealth could obtain the most exquisitely made. This love of luxury did not go unnoticed by the Blackburn Commission: "The Chinese everywhere emphatically has [a taste for luxury]. He may be trusted to buy luxuries to the full extent of his means" (Bourne's Section, 1898:115). The Commission, however, saw this as an indication that the Chinese ought to be receptive to Western goods. As this analysis suggests, commodities having a Western symbolic content did not belong to those items which revealed a person's success.¹¹ Thus, to consume Western products was to put into question one's social standing, including one's influence in local affairs.

This line of analysis gains support from cross-cultural comparisons. In those complex agrarian societies accepting large quantities of Western goods (excluding such settler colonies as South Africa and New Zealand), one finds very similar

structural configurations. In each society, considerable social distance separated vertically-ranked status groups. The membership in these groups tended to be ethnically, religiously or hereditarily defined. For instance, in Russia a large number of Western products found ready acceptance simply because they were made in Europe, particularly France. However, the Francophilia that seized Russia was restricted to the Russian nobility and their retainers. Western commodities had a symbolic value precisely because the class lines were so sharply drawn. Holding a vastly superior rank in comparison with peasants, the Russian elite had been consciously cajoled into this pattern of consumption by the centralizing monarchs, especially Catherine the Great. Like France's Louis XIV, Russian rulers attempted to augment their power over Russian nobility by increasing the prestige of their status while at the same time reducing their political authority. They were turned into a cohesive leisured class with wealth but not responsibility (Von Laue, 1961).

South American elites likewise consumed Western products in large quantities but for slightly different reasons. In the revolutionary upsurge in the early nineteenth century, South American elites freed themselves from Spain's political domination. However, this elite group which had been formed during Spanish rule, was ethnically categorized as *mestizo*; that is, the members spoke Spanish, belonged to the Catholic Church, and generally adhered to Spanish patterns of behavior as these patterns were implanted in the New World. Like the Russian elite, they were socially cohesive and distinct from the rest of the indigenous population who were categorized as Indians. Even though they might have owned haciendas in the hinterland, the elites were cosmopolitan and the Indians rural. Unlike the Russian elite, however, the South American social elites also were the political leaders who maintained invidious distinctions as one of the means of maintaining their hegemony. During the nineteenth century, the consumption of Western commodities, especially from England, symbolically signified elite status, so

¹⁰ Connoisseurs of Chinese art often describe late Ch'ing art as being primarily composed of heavily stylized copies of the art of earlier dynasties. In contrast, T'ang and Sung art is seen as more free flowing and original, though lacking the technical perfection achieved in the Ch'ing period.

¹¹ This point is well illustrated by Chen's observations in the 1930s of the style of life adopted by Chinese returning to China after working abroad, and presumably exposed to Western styles, for many years. According to Chen (1940:111), successful returnees made an "effective display of pride" by showing "evidences of taste and culture. . . . [Their] ideal of 'complete happiness' . . . is not in fact anything new the emigrants bring back with them from abroad, but embodied in the folkways of the countryside. What they do contribute is financial ability to gratify these tastes. . . ." Watson (1975) makes identical observations in his excellent study of a contemporary single-lineage village in the New Territories of Hong Kong.

much so that the word *importado* became synonymous with quality and *nacional* synonymous with shoddy.¹²

In India, Western commodities were used by specific *jati* that in some way served the British, such as the Brahmin administrators and the Parsi merchants. In Thailand, which was never formally colonized, the Thai aristocracy westernized in the mid-nineteenth century after Chulalongkorn centralized his administration in response to Western pressure. In the Philippines, and to a certain extent in Malaysia, Western textiles became an increasingly important item of consumption among the upper classes.

Of all the places in Asia, however, the craze for Western commodities was especially prevalent in Meiji Japan where "The very word for 'imported article' was uttered with a feeling approaching reverence, and [the use of] foreign things was a source of great pride" (Yanagida, 1957:288). According to one Japanese account in 1878, "Although imported goods appeared only ten years ago, one sees them in the house and on the person of every member of the urban upper classes. People want anything foreign, whether it is of value or not" (Yanagida, 1957:289). The consumption of Western textile products ranged from hats and caps, the former being worn by the upper classes and the latter by soldiers and merchants (Shibusawa, 1958:31), to gloves and all the trappings of formal Western attire. Although some of these products later

spread to lower-class groups, the upper classes—the former aristocracy—were the first and the most frequent consumers of Western cloth and fashions, so much so that one analyst suggests that the use of Western clothing "simply emphasized the difference between the rich and the poor" (Shibusawa, 1958:25).

These and other examples strongly suggest that the consumption of Western commodities often reflected internal cleavages present in non-Western societies before Western industrialization began. And in each case, the elites of the respective societies consumed in a manner not influenced by subordinate status groups. In short, status competition was class-specific; for the elite, it was a competition among gentlemen.

These remarks also apply to the T'ang and Sung dynasties. During these periods, Chinese society was much more hierarchically structured than it was during the Ch'ing. T'ang society is noted for its hereditary aristocratic families, which supplied the court with officials and enforced sumptuary laws for other groups in the society. As Elvin (1973) shows, the social distance between the peasantry and the elite at this point in time was so great that it is analytically justifiable to refer to a "serf-like" peasantry. The cosmopolitanism of the T'ang and Sung dynasties was in large part a reflection of social elitism, a response of an upper class without roots in the peasantry.

Although the status competition explanation allows for the use of differential comparisons, the testing of the explanation, nonetheless, presents some difficulties. For instance, it can be argued, on one hand, that since Russia and Latin America are part of the Western cultural complex, the receptivity of their elites to factory-made commodities is not surprising. This line of reasoning is an application of a culture explanation. On the other hand, the status competition explanation, suggests that Russian and Latin American elites, having weak ties to local society, were more open to exogenous influences from whatever source. In this respect, Westernization in Latin America would be analogous to the vogue for Chinese products in eighteenth-century Europe, when

¹² The best summary of these patterns is found in Halperin-Donghi (1973:82-94). In regard to the use of Western textiles in Latin America, he (1973:86) writes, "From Mexico to Buenos Aires women began enthusiastically to accept the dictates of fashion which not only took for granted the acceptance of a new style, but above all that of periodically changing styles, which increased the consumption of imported cloth. The process was not restricted exclusively to the upper classes. In a society which ever since colonial times had been divided into only two sectors according to criteria which were not exclusively economic, any woman who did not wish to be associated with the lowest stratum had to use her ingenuity to follow the changing moods of fashion." An important point he makes, which also applies to Japan (Shibusawa, 1958:21-7), is the rapidity with which the people of these societies accepted Western clothing. In Mexico, the changeover occurred between 1824 and 1827 and in Japan between 1872 and 1884.

every manor house had its chinaware and other oriental luxuries. The demonstration that other aristocratic societies outside the Western cultural complex also welcomed Western commodities strengthens the status competition explanation, but this is not a definitive test. More research is obviously needed to refine the comparative testing of these explanations—perhaps by looking for elites who retained their native dress (as did the tribal leaders of West Africa and the Persian Gulf) or by finding variation in Westernization within societies that generally consumed Western products (as occurred in portions of Southern Mexico where elites, having strong affiliations within Indian communities, continued to dress as Indians).

Despite the difficulties in comparative testing of alternative explanations for a particular occurrence, there are few other approaches as useful in isolating possible causal relationships and in weighing the contribution of each explanation. As this case study has shown, each alternative has some merit, even though none of them alone is able to account for the non-consumption of Western goods in China. But together they form a composite answer to this question. The faulty merchandise and marketing explanations allow insight into a dynamic traditional economy that was able to produce effectively many Western commodities the Chinese found acceptable. That the Chinese did not import more of those acceptable items is explained, in part, by these explanations. The status competition explanation suggests some parameters of the situational logic by which individuals evaluated a range of possible consumer choices in relation to their relative social standing within the community. That the Chinese did not find exogenous commodities acceptable symbols of status is best interpreted by means of this explanation. The culture explanations provide an understanding of those commodities that did reflect one's success or claims of success.

Conclusion

If one narrowly defines "Westernization" as the adoption of Western styles

and the purchase of Western commodities—thus analytically distinguishing this process from other processual aspects of Western expansion (e.g., political modernization, industrialization)—then some generalizations can be tentatively offered. The above discussion suggests that Westernization would be more likely to occur in those non-Western societies in which there are established elite status groups. If these elite status groups also effectively controlled political power within their respective societies, as they did in Latin America, to a certain extent in Japan and to a lesser extent in Russia, then one might further argue that Westernization can be regarded as an *indicator* of the maintenance of a pre-modern political and social structure. Thus, it is not unreasonable to alter Marx's prediction, quoted at the beginning of this paper, to say that the purchase of Western commodities reproduces the old world in new dress.

This conclusion is, of course, in agreement with some recent efforts to show that contemporary Japanese and Latin American societies exhibit considerable continuity with the pre-modern period. However, in contrast to some of these efforts, the important point to be made here is that Westernization should not be viewed as necessarily antagonistic to the preservation of the old order, with historical continuity being maintained in spite of it. Rather, in such highly stratified societies, Westernization might be better viewed as a new way to signify, as an attempt to reaffirm, a traditional social order.¹³

The relative lack of Westernization in China, given its very different system of stratification, reveals substantially the same outcome as that experienced in most other societies, namely, the choice not to break decisively with the past. Thus in theory, at least, China should not be seen as a negative case in terms of its rejection

¹³ This hypothesis implies a slight but significant alteration in the posited (e.g., Frank, 1967) direct relationship between Western capitalism and underdevelopment in the third world. The implication here is that the relationship is not direct but, rather, is mediated through political and stratificational systems, in which variance would produce different outcomes at different times and places.

of Western influence simply because the Chinese failed to patronize the products of Western factories. The spread of Western civilization, even in an economic sense, is more complex and subtle than this. And Westernization, though significant and interesting, is hardly the whole story. In fact, it could and perhaps should be argued that the Chinese responded to Western economic expansion in more aggressive and positive ways than did individuals in many other non-Western societies. For instance, the Chinese, more than any other group of non-Westerners, became the principal retailers of Western products and exporters of raw products to the West throughout all of Southeast Asia and portions of the Pacific and the Caribbean. To call this collective action of millions "unresponsive to Western influence" is certainly dubious.

Therefore, if these suggestions have some validity, then the expansion of Western civilization is by no means an unambiguous process. Its analysis requires more than that which an economic, political or sociological perspective can alone provide. But, in combining these perspectives, one must strive for an analytic framework that facilitates the use of differential comparisons, for only by making systematic contrasts can one begin to disentangle the complexity of civilizational encounters.

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NUMBERS GAMBLING AMONG BLACKS: A FINANCIAL INSTITUTION*

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Mainstream financial institutions have never been able to provide generally prevailing service levels in poor communities. In the resulting partial-service vacuum, blacks invented numbers gambling. Numbers-gambling banks became sources of capital and a major savings device of urban black communities. In conjunction with the usury industry, numbers banks framed an alternative institutional system for the savings-investment cycle in the slum. Numbers banking illustrates the conjoint contribution of institutional and cultural causes in analysis of poverty.

CULTURAL AND INSTITUTIONAL EXPLANATIONS OF POVERTY

The literature on poverty conventionally distinguishes institutional and cultural explanations (Light and Wong, 1975; Elesh, 1977). The distinction hangs upon whether an explanation identifies social institutions or the culturally-induced behavior of the poor as the cause of poverty. Although this taxonomy satisfactorily encompasses the structures of explanation, two intervening processes always precipitate poverty. These two are low income and wasteful and/or destructive consumption.

Low income is the most obvious cause of poverty, but the balance between income and consumption is always the technical issue. Even where disposable family incomes are above the poverty level, wasteful or destructive expenditures may deplete a family's reserve until income is no longer adequate. In this case, wasteful consumption—not low income—actually caused a family's poverty. Naturally, when incomes are inadequate, low income becomes the immediate cause of poverty but, even here, wasteful or destructive consumption can exacerbate it.

The intervening processes of low income and wasteful/destructive consumption are compatible in principle with either institutional or cultural explanations of poverty. For example, institutional explanations of income-induced poverty have stressed external barriers imposed by racial discrimination, structural unemployment, minimum wage laws, labor exploitation, the split labor market, and so forth. Cultural explanations of low income-induced poverty have stressed unfavorable work habits (absenteeism, soldiering, tardiness) which reduced employability, attributing these variously to the lack of a Protestant ethic, or the presence of employment and earning-inhibiting cultural residues (Banfield, 1974).

On the neglected consumption side, cultural explanations have emphasized the adverse consequences of wasteful/destructive consumption habits allegedly characteristic of the "disreputable poor" (Matza, 1966). For example, in the decades of temperance agitation preceding the Prohibition Era in the United States, much social research concerned insobriety among the working class and the putative connection of drinking and pauperism (Koren, 1899: 64-99). Many other consumption habits have been linked to poverty, among them: unsophisticated shopping, purchase of luxuries, big families, unhealthy diet, excessive use of installment purchase, failure to save and gambling (Glazer and Moynihan, 1970: 33; Foxall, 1974).

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One institutional response to this catalogue of wasteful consumption practices has been to question the scope of culturally-induced consumption differences between the poor and nonpoor. This issue produced a complex and contradictory literature which permits no easy encapsulation (Alexis, 1962; Nixon, 1963; Simon and Simon, 1968; Stafford et al., 1968; Sturdivant, 1969; Cicarelli, 1974). In some cases, undeniable income-linked differences have appeared—for example, lower rates of life insurance purchase by the poor. Even here, institutional writers have insisted that poverty produces situational pressures which require the poor to seek irregular and often more expensive alternatives to those prevailing elsewhere in the market (Light, 1972: 152–69; Ferman and Ferman, 1973; Wong, 1977). In a memorable phrase, Caplovitz (1963) observed that “the poor pay more” because the retail stores of the slum compel it. Similarly, successive studies of pawnbrokers and loan sharks have concluded that poor people turn to high-priced lenders because they are unable to obtain credit in cheaper institutions (Forman, 1906: 622; Seidl, 1968: 88–9). In all of these cases, the poor are making unwise, destructive or wasteful purchases, but circumstances rather than improvidence necessitates the waste.

The ideological implications of any simply cultural or simply institutional explanation of poverty are mutually repugnant (see Huber, 1974), and a spirit of dogmatism often has characterized social science discussion as a result. The source of this dogmatism is the insistence that cultural and institutional theories of poverty must exclude one another. This insistence is fallacious because cultural and institutional explanations can produce poverty jointly as well as singly. Recognizing this possibility of conjoint causality, Valentine (1968: 117) nonetheless condemned it as “eclectic,” and social scientists have been dismayingly willing to accept this unwarranted constraint.

The treatment of Lewis' (1968) “culture of poverty” thesis is a case in point. Lewis' detractors as well as his defenders accepted the premise that his thesis is a cultural theory (Leacock, 1971). Yet, the

premise is strictly untrue for, in Lewis' view, historic institutions gave rise to contemporary cultures of poverty. His total explanation actually included both institutional and cultural components so it was never a purely institutional nor a purely cultural explanation. The controversial Moynihan report was only a specific application of Lewis' formula to American blacks (Rainwater, 1967). Moynihan asserted that slavery, a bygone institution, had engendered a matriarchal cultural tradition among blacks. The female-headed family, a cultural survival of the bygone institution, had become the principal cause of poverty among American blacks today. Moynihan's thesis contained cultural and institutional components; it was never a purely cultural explanation.

True, a purely cultural phase succeeded a purely institutional phase in both the Lewis and Moynihan versions. (The sequence might, in principle, have been reversed.) On the other hand, no barrier prevents institutional-income and/or institutional-consumption causes from coexisting in time with cultural-income and/or cultural-consumption causes. Numbers gambling offers empirical confirmation of this logical possibility. Numbers gambling is a wasteful consumption practice of the poor. Existing literature attributes this wasteful consumption to cultural causes. This cultural-consumption orthodoxy is simplistic. Actually, numbers gambling combines institutional and cultural causes in the same time span. Therefore, this empirical case supports the conclusion that institutional and cultural causes of poverty may operate conjointly in a situation, and that empirical determination of preponderance is necessary in every case.

NUMBERS GAMBLING: A FINANCIAL INSTITUTION

In the late nineteenth century and until roughly 1940, policy wheels were the prevailing type of lottery among urban blacks and whites of the lower class. Although policy gambling persisted in Chicago and Detroit until 1940, numbers gambling by now has replaced policy in all major American cities. In numbers gambling, a

"gig" bettor stakes a small sum on three digits, 000 to 999. Instead of a wheel or drum, the house takes its winning number from published numbers such as bank clearing totals, volume on the New York Stock Exchange, or parimutuel totals. The published figures eliminate the possibility of a rigged outcome, a decisive technical advantage over the policy format (Light, 1974). Beale and Goldman (1974: 541; cf. Roebuck, 1967: 136; *Black Enterprise*, 1973: 12) estimate that numbers gamblers in the United States wagered 2.5 billion dollars in 1973, roughly ten percent of all illegal gambling revenue.

Discursive evidence claims that blacks were overrepresented among policy gamblers, but the implications of these statements are unclear (Crapsey, 1872: 104; Martin, 1868: 517; Du Bois, 1899: 265). There is no evidence that blacks were overrepresented relative to poor nonblacks; in addition, white ethnics commonly had their own lotteries which may have been functional equivalents of policy gambling (Ianni, 1972: 67; Carlson, 1940: 24). However, Caribbean blacks invented and popularized numbers in this country during the 1920s. As a result, numbers gambling was still 60 percent black in 1934 (Light, 1974: 55). Carlson (1940: 3-4; see also Caldwell, 1940:2) noted that although "white people of the lower economic classes" had begun to bet on numbers, this form of gambling remained "predominantly an urban negro [sic] activity." Currently, Marcum and Rowen (1974: 31) observe that numbers gambling is "concentrated among blacks and ethnic groups in the older cities, especially in the East and Midwest." The Fund for the City of New York (1972) found that blacks represented 30 percent of numbers bettors, but only 20 percent of the city's population. This progression indicates that numbers began as an exclusively black lottery, but diffused to nonblacks.

Studies of lottery gambling, even those most sympathetic to blacks, have always regarded the betting as superstitious nonsense justifying a prodigal waste of money (Riis, 1892: 155; Peterson, 1952: 194; Drake and Cayton, 1962: II, 491). The amount wasted is substantial. Drake and

Cayton (1962: II, 481; cf. Warner and Junker, 1941: 19) concluded that the policy syndicate on Chicago's South Side employed 5,000 persons and grossed at least 18 million dollars in 1938. This sum would represent \$64 for every black person in Chicago and \$256 for a family of four. The median income of all families in Chicago was \$1,463 in 1940, so numbers gambling of blacks accounted for about 17.5 percent of family income. More recently, the Fund for the City of New York (1972: 9) concluded that New Yorkers wagered 600 million dollars a year on numbers bets. Blacks represented 30 percent of New York's numbers bettors, so the city's black population presumably laid 180 million dollars on numbers in that year. This sum represents \$87 for every black person in New York City. Since the median family income of blacks in New York was \$7,309 in 1969, nearly five percent of black family income went for numbers gambling. This estimate is conservative. Director of Political Affairs for the Congress of Racial Equality, Ed Brown (1973) has claimed that in Harlem alone blacks wager "at least" 300 to 500 million dollars a year on numbers. If only 300 million dollars were wagered yearly, this betting gross would represent about \$300 for every black person in Harlem or 16 percent of family income. In the 94 percent black Bedford-Stuyvesant section of Brooklyn, Lasswell and McKenna (1972: 55-62) examined numbers gambling between 1963 and 1970. Their estimates are the most reliable of any. They found that impoverished residents spent between 2.5 and 5.1 percent of per capita income on numbers bets. Admittedly, these estimates of betting volume are crude and vary widely. Nonetheless, even the lower estimates indicate that numbers betting represents a significant waste of money by poor people.

Why do poor blacks waste money on numbers? The sociological literature on gambling is scanty (Scimecca, 1971: 56; Tec, 1964: 105) and it offers only two efforts to explain numbers gambling as opposed to gambling in general, the usual focus. The two offer exclusively cultural explanations. Carlson (1940) called attention to "the culture complex" underlying

the numbers gambling of Detroit blacks. This complex included dream interpretation, folklore, and music, social roles, spiritualism, ceremonial festivities, fad and fashion in playing style, and an extensive gambling jargon. McCall (1963) stressed the "sympiotic" relationship between spiritualist cults and numbers gambling, claiming that the superstitions mutually reinforced each other. The cultural source of the superstition McCall identified as animistic religions of West Africa and Caribbean "hoodoo." In view of the heavy folklore surrounding the numbers complex, cultural explanations have an undeniable plausibility. Moreover, the metropolitan black press continues to depict numbers gambling as a "soul" preoccupation (E. Brown, 1973) while reminding readers that blacks invented the game. In all cases, what is evoked is a specifically black cultural heritage rather than a generalized "culture of poverty." Nonetheless, the prevailing cultural interpretation of numbers gambling among blacks comes down to a cultural-consumption interpretation of poverty among this group.

The evidence supporting the cultural side of black numbers gambling is too strong to deny, but a close look at numbers gambling turns up three telling anomalies. First, of cultural theories of gambling in general, the preeminent is Devereaux (1968; see also Thurner, 1956) who stresses the incompatibility between gambling and the Protestant work ethic. The obvious difficulty is that black numbers gamblers are predominantly Protestant fundamentalists whose gambling can only occur despite the restraint of this religious tradition. Hence, the cultural baggage of blacks does not provide un-mixed support for numbers gambling. Second, a cultural explanation of black numbers gambling cannot account for the 70-80 percent of numbers gamblers who are now nonblack. This objection was less serious when numbers was an exclusively black preoccupation, but the subsequent diffusion of the game to nonblacks compels the conclusion that cultural continuities originating in the black heritage cannot give the whole explanation. The supposed improvidence of the lower class

offers a class cultural explanation for numbers gambling by the black and nonblack poor. However, the actual distribution of gambling among social classes lends little evidentiary support. Li and Smith (1976) reported national survey data which show that the propensity to gamble is positively associated with socioeconomic status, a result repugnant to any class cultural theory. The best evidence cited in support of a negative relationship between socioeconomic status and gambling is Tec (1964; see also Newman, 1972: 85). However, Tec concentrated only on football pool gambling, an admitted preoccupation of the working class. Numerous studies (Bloch, 1951: 218; Commission, 1975: 13) report that identifiable social groups have favorite gambling activities. For example, the Fund for the City of New York (1972) reported that low-income gamblers preferred numbers, but high-income gamblers preferred casino games; at middle-income levels, sports and race track betting prevailed. This mosaic of gambling preferences implies that the correlation between socioeconomic status and gambling depends upon which gambling game is under scrutiny.

Third, a fundamental assumption of any cultural theory of gambling is the expectation that rates should remain stable over long periods of time in reflection of cultural divergences. But the history of lottery gambling in general (Light, 1974: 52-3) and numbers gambling in particular (Carlson, 1940: 4, 138, 158; Caldwell, 1940: 30) shows abrupt shifts in participation: lottery gambling increases in frequency in economic hard times and declines in periods of prosperity. Cultural theory cannot account for cyclical fluctuation.

On the other hand, the increase in numbers gambling in periods of business depression immediately suggests the anomie theory (Durkheim, 1951: 241-46). Merton (1957: 149) also identifies the numbers gambling of blacks as a form of "innovative deviance." Big prize lotteries offer the possibility of immense wealth to winners, and sociological treatments of these have commonly asserted that working-class bettors hope to strike it rich (Stock-

ing, 1932: 558; Marcum and Rowen, 1974: 30; Ianni, 1974: 110), a conclusion congenial to anomie theory (Tec, 1964: 62; Zetterberg, 1962: 122). A related view has developed in economics since the analysis of Friedman and Savage (1948) reversed the economists' long-standing belief that gambling is always irrational. Even the enemies of gambling within the economics profession now acknowledge that gambling is "rational when a person's wish to obtain an otherwise unattainable large prize is very large" (Rubner, 1966: 52). Eadington (1972: 24; see also Off Track Betting Corporation, 1973: II) assumes that numbers gambling satisfies this condition and, thus, justifies its economic rationality.

Anomie theory fits big prize lotteries, but it does not fit numbers gambling because numbers bettors do not expect to change life-style when they win. On an average bet of one dollar, a numbers winner receives a pot which may range from \$500 to \$600. Players are expected to tip the runner ten percent of winnings, a practice which reduces net gain to \$450-\$540. In the depressed Bedford-Stuyvesant sector of Brooklyn, slum dwellers averaged only 50 cents a numbers wager, so their expected return was only \$225 to \$270 in 1970. These calculations exaggerate expected return because the game permits less radical bets than the three-digit gig. In "single action," for example, a bettor selects only one digit at odds of 10 to 1. The maximum return in this popular bet is only \$6, less the runner's tip. These returns fall far short of riches permitting a change in life-style.

Personal Saving

Sellin (1963:19; see also Ianni, 1974: 110) supposes that numbers gambling thrives because a "segment of the population enjoys betting" and does not regard it as harmful. But the entertainment theory does not correspond with the bettors' understanding of the numbers game. On the contrary, they vigorously disclaim betting for the thrill, fun or sport of it. Numbers gamblers view the game as a rational economic activity and characteristically

refer to their numbers bets as "investments" (Carlson, 1940: 138-9).

Most gamblers understand their numbers betting as a means of personal saving. This ubiquitous self-justification is the crucial prop for the entire gambling order (Eadington, 1972: 29; Ianni, 1974: 78). The bettor's justification for this seemingly preposterous misconception arises from unsatisfactory experiences with depositary savings techniques. Once a numbers collector has a man's quarter, they aver, there is no getting it back in a moment of weakness. If, on the other hand, the quarter were stashed at home, a saver would have to live with the continuing clamor of unmet needs. In a moment of weakness, he might spend the quarter. Therefore, in the bettor's view, the most providential employment of small change is to bet it on a number (Grow, 1939: 213; see also Whyte, 1943: 41). "The dime or quarter which one bets is scarcely missed," writes R. Brown (1973), "but when one 'hits' the payoff is a chunk of money large enough to be really useful to the winner." Bettors do win (Johnson, 1971: 42). On an average day, the betting public receives back in "hits" fifty percent of its total wager. From the bettors' perspective, numbers gambling is a means of converting change into lump sums; in effect, a savings method (see Samuelson, 1973: 423; Commission, 1975: 17).

The methodical style of numbers gambling also indicates that bettors have adopted a long-range perspective, suggesting a rational savings strategy. The Fund for the City of New York (1972: Appendix 15) found that 72 percent of numbers bettors placed a bet "two or three times a week" and 42 percent bet every day. The Off Track Betting Corporation (1973) concluded that: "The typical numbers player currently wagers on the game between 2 and 4 times a week." Even more strikingly, the Fund for the City of New York (1972: Appendix 25) found that 41 percent of numbers bettors had been betting on the game for ten years or more, and 59 percent for six to ten years. Indeed, the largest bettors were those who had been playing the longest. The frequency of wagering and the decades-long perseverance of numbers gamblers outlines an average

playing career which encompasses 1,300 trials. In a decade of gambling at this rate, a gambler confidently can expect to hit at least once (for \$550) against his total investment of \$1,300. Viewed from a decade's perspective, the expected return of a numbers gambling career approaches the expected value of the game (Ignatin and Smith, 1976; Ianni, 1974: 78; Eadington, 1972: 205).

Numbers games attract funds which would not otherwise be saved in depository accounts (cf. Rubner, 1966: 36). First, numbers gambling is convenient. Numbers runners make a regular circuit of their customers who thus do not have to go out of their way to bet. In addition, numbers stations are located in newsstands, pool halls, cigar stores and groceries which people visit on an average day. Therefore, even people who have savings accounts find it convenient to lay a dollar on a number while at the barber shop rather than risk making no "investment" at all in the day. The fund for the City of New York (1972: 57; see also Haller, 1970: 623) concluded that a legalized numbers game would require seven to ten thousand outlets to compete with the illegal game in convenience. In 1970, Bedford-Stuyvesant alone contained 1,345 numbers runners whose business was making it easy to bet (Lasswell and McKenna, 1972: 111).

The friendly atmosphere of numbers gambling encourages "saving." People choose to deal with a numbers runner whom they trust and like (McKay, 1940: 112-3). Therefore, interactions with this person are enjoyable. An established numbers station is usually operated in a small grocery store, candy shop or beauty parlor. Stores of this sort often serve as neighborhood hangouts as well as retail outlets (cf. Whyte, 1943: 143; Firey, 1947: 190). When placing a bet, a gambler has an opportunity for a few moments of sociable interaction with whomever is hanging around. The sociable atmosphere of a numbers station thus stimulates to "saving" these persons who would otherwise simply have spent all small change.

Numbers gambling also appeals to the race pride and community spirit of the ghetto public. The real extent of altruistic

motivations is unclear, but the experience of state lotteries suggests that altruistic motives do induce some people to gamble. When asked why they purchased Connecticut state lottery tickets, 82 percent of bettors mentioned financial reasons and 33 percent the benefit to the state.¹ In some cases, blacks have actually gotten together in order to set up an unemployed friend as their numbers runner (McKay, 1940: 112-3). However, even in cases where the direct support of a friend is not involved, blacks understand their numbers gambling as a local form of work-relief (Drake and Cayton, 1962: II, 493; Ottley, 1943: 155; Black Enterprise, 1973: 44; Ofari, 1970: 44-5). Numbers gambling syndicates do provide a lot of employment in black communities. Lasswell and McKenna (1972: 168) found that numbers gambling syndicates were the largest employers in the slum, second only to the federal government. Numbers collectors frequently double as town criers and fund raisers on their rounds. One black informant, an experienced collector in Detroit, observes that passing the hat for hard-luck cases was a regular function of his daily rounds. In this manner, the people who regularly dealt with him put themselves into a loose federation for mutual assistance in time of need.

Consumer Credit

In addition to drawing savings out of an impoverished population which finds saving difficult, numbers-gambling banks also make credit available to poor people who would otherwise be unable to obtain it (Dominguez, 1976: 38). The capital fund from which this credit derives is the gambling play of the neighborhood. The methods by which this capital returns as credit to the local economy are sometimes circuitous and sometimes direct.

Numbers runners make direct loans to customers. These direct loans are of two sorts. First, numbers runners sometimes permit the needy to borrow the where-

¹ I am grateful to John T. Macdonald and Fillis W. Stober, State of Connecticut, Commission on Special Revenue, for showing me these unpublished tabulations.

withal to bet. Eighteen percent of numbers players in New York City acknowledged placing bets on credit (Fund for the City of New York, 1972: Appendix 29). Among those who bet a dollar or more per day, this percentage increased to 23. Second, some runners lend cash to steady customers. Nine percent of numbers players in New York City had borrowed cash from their numbers runner for some purpose other than betting. Among persons who usually bet a dollar or more per day, this percentage increased to 15 percent. These percentages are modest, and there is no indication of how recourse to credit varies with income or color. On the other hand, a 1970 sample of California households (Day and Brandt, 1972: Table 6.2) found that eight percent of white and 15 percent of minority households with annual incomes less than \$7,500 reported borrowing money from a credit union. Therefore, the direct credit service of numbers gamblers is roughly comparable to credit unions in a low income population.

A wider credit conduit is the return to the local community of numbers gambling profits as loan shark's capital. This return is the standard underworld employment of numbers gambling profits (Seidl, 1968: 32-3; Kaplan and Matteis, 1968). Consumers and small businessmen are the principal customers of slum loan sharks who actually frequent banks in order to approach disappointed loan applicants, sometimes being waved or pointed out to these by the platform official (Seidl, 1968: 16-7; 109-19, 139; Congressional Record, 1967). In many cases, numbers gamblers and loan sharks are the same individual, who simply transfers money from one pocket to another when he changes roles (Anderson, 1974: 25, 127, 130-1; Ianni, 1974: 80, 136; Knudten, 1970: 140).

Business Investment

Numbers racketeers have been the largest investors in black-owned business or ghetto real estate and the chief source of business capital in the ghetto (Roebuck, 1967: 142; Drake and Cayton, 1962: II, 487; Caldwell, 1940: 153; Strong, 1940: 133; see also Whyte, 1943: 145; cf. Ofari, 1970: 46). In addition, numbers bankers

have been virtually the only sources of business capitalization available to local blacks lacking collateral or credit rating (Cook, 1971). As a result of these loans, black-owned businesses not actually owned by numbers bankers were often in debt to them (Drake and Cayton, 1962: II, 469). Finally, numbers bankers have been leading philanthropists in depressed black neighborhoods, making donations to churches and athletic teams, and providing Christmas and Easter baskets for the poor (cf. Perucci, 1969; Whyte, 1943: 142-5).

MAINSTREAM FINANCIAL INSTITUTIONS: MALFUNCTION

One way to appraise the importance of numbers gambling in the financial life of the slum is to compare this subterranean financial system with the saving and investment services of mainstream financial institutions, such as banks. Even a casual review of relevant literature proves that mainstream institutions do not now, nor have they ever in the past (U.S. Immigration Commission, 1911: 216) been able to provide the same standard of financial service in depressed communities which they routinely provide in affluent ones (Hiltz, 1971; Dominguez, 1976: 18). The chronic malfunction of mainstream financial institutions in the slum leaves an enormous service gap in which a diversity of popular financial institutions—including the numbers racket—plausibly may flourish.

Saving

Poor people in general and nonwhites in particular make less use of banks for savings or checking accounts than do non-poor (Lewis, 1968: 190-2; Irons, 1971: 420). Low income is the most obvious cause: the less money people earn, the less they have to deposit in banks. National survey data confirm this expectation (see Table 1). The consumption of banking services declines as income declines. Of those who own no savings account, 75 percent (1972) and 80 percent (1970) explained that lack of "money left over" after paying bills was the reason

Table 1. Usage of Banking Services by Color and Family Income, 1966 and 1972

	Have a Savings Account (%)		Have a Regular Checking Account (%)	
	1966	1972	1966	1972
All Persons	52	73	56	76
Family Income:				
\$15,000 & over	{ 74	84	{ 84*	85
10-14,999		81		83
6-9,999	68	72	78*	76
4-5,999	47	{ 58	63*	{ 62
Under 4,000	28		38*	
Color:				
White	60	74	71*	80
Nonwhite	30	61	23*	48

Sources: Opinion Research, 1966: 33, 71; Harris, 1972: 49, 55.

* Regular and/or special checking account.

(Harris, 1972: 73; Opinion Research, 1966: 39). Among those who had no checking account, 45 percent (1962) and 46 percent (1966) blamed lack of money, the modal explanation.

Investment

A result of nondepositing in banks is noncreation of capital funds in them. This shortage begins the "capital gap" in the inner city, a condition which strangles consumer, mortgage and business credit alike (Garvin, 1971: 445). Moreover, such funds as do make their way into deposit accounts in low-income areas do not, in general, find local investment outlets. Because they lack collateral and have irregular and low income, poor residents make bad risks for bank loans (King, 1929: 18-9; Neifeld, 1939: 169; Dominguez, 1976: 19). In addition, the sums poor people borrow are normally too small to permit profitable lending (U.S. Senate, 1970: 89, 160; 1968: 404). Even credit unions require federal subsidies to offer loans to the poor at legally permissible rates of interest (Cargill, 1973). Therefore, consumer finance companies, like banks, also avoid poor families (Booth, 1973: 71).

Small business loans in poverty areas are more risky than investments in corporate or government securities (Cross, 1969: 45, 46; U.S. Senate, 1970: 148). Adverse profit considerations also have shaped mortgage lending policies in inner cities. Savings and loan associations

routinely refuse to issue mortgages in "red-lined" areas deemed to be in deterioration (U.S. Senate, 1975). The undesirability of consumer, business or mortgage investment opportunities has resulted in a flight of capital from the inner city to more profitable suburban locations (see Orren, 1974: 145-78). A source of interracial tension, this flight of capital owes more to economic calculation than to color prejudice. In 1971, for example, all U.S. banks had 13 percent of total assets in government securities whereas the nation's 20 black-owned banks had 30 percent of their assets so invested. The ultra-conservative investment policies of the black-owned banks led Brimmer (1971) to observe that the black banks were "diverting resources from the black community into the financing of the national debt." This investment policy contributes to the "almost total void of low cost credit and capital" in the inner city (U.S. Senate, 1968: 408).

Bank-Community Relations

Nonfinancial barriers to the normal operation of depository institutions arise from the mutual lack of sympathy of bankers and the poor. The cultural ethos of a bourgeois society provides bankers with no basis for an indulgent view of poor people, and the problem is more acute when the poor are also nonwhite. Blacks have complained for over 200 years that

white bankers discriminate against them (Light, 1972: 19). This persistent complaint turned up in Harris' (1972: 94) research for the banking industry. In this survey, 43 percent of nonwhites but only 18 percent of whites agreed that banks made it "too difficult" for nonwhites to obtain loans.

Banking industry research (Harris, 1970: 97-8) confirms that the loan turnaround rate "has been highest among lower income groups and nonwhites." Bankers also have acknowledged the interpersonal problems which arise when white credit managers refuse nonwhite customers (U.S. Senate, 1970: 195-6). However, banking industry spokesmen have denied that bankers' race prejudices were the source of the problem (Haugen, 1968: 103; U.S. Senate, 1970: 160; see also Marsh, 1971). The practical line between prejudice and an adverse but impartial bank decision is difficult to draw because of the connection of color characteristics with economic marginality. A U.S. Senate (1968) investigation addressed allegations that the racism of white bankers was responsible for the inner-city financial gap. However, the extensive hearings provided no evidence of more than a clash of cultural standards; and subsequent research also has unearthed no evidence that the color antipathies of bankers distort their business judgment.

Nonetheless, poor minorities fervently believe that bankers discriminate against them and discourage their patronage (Day and Brandt, 1972: 102). This belief nourishes the antipathy of these people to banks (Cross, 1969: 51, 54, 66; Kurtz, 1973: 55). Banking industry research shows a decline of confidence in banks as family income declines (Harris, 1972: 28; 1970: 46), but color makes an autonomous contribution. Black people are more alienated from banks than would be expected on the basis of income or educational attainment alone (Root, 1966: 65). Selby and Lindley (1973) found that Atlanta blacks were less prone than whites to maintain a checking account, even holding constant the blacks' generally lower income and educational level. Similarly, banking industry research (Harris, 1972: 49) found that among families

with incomes under \$6,000 a year, the poorest class, 62 percent had checking accounts but only 48 percent of nonwhites did (Table 1). More generally, surveys record a level of disenchantment with business and banks which is higher among nonwhites than among the poor. For example, Harris (1972: 131) found that among families with annual incomes less than \$6,000, 37 percent rated banks "very high" in their concern for helping the local community, but only 28 percent of nonwhites agreed. Harris (1970: 130) also found that among families with annual incomes less than \$6,000, 34 percent perceived bankers as "highly concerned" with helping their community, but only 16 percent of nonwhites made this friendly rating.

Naturally, many expressions of mistrust reflect hostile stereotypes rather than harsh experience. Harris (1970: 46; also Hiltz, 1971: 996) reported that nonwhites indicated less familiarity than whites with every type of financial institution except the sales finance company. Ignorance and public image in principle are open to change through "education" which bankers now view as the key to extending their market in black and low-income areas. The banks' record of recent extensions in response to "advertising and promotional activities" proves that poor people's unfavorable attitudes are open to suasion (Opinion Research, 1966: 74).

However, the malfunction of mainstream service in inner cities is neither epiphenomenal, nor easily rectified within a profit system. To extend service markets in low-income neighborhoods, depository institutions must surmount the barriers posed by the residents' poverty as well as their ignorance of and antipathy toward banks. Although the record of recent extensions proves these barriers are not insuperable, the dollar costs of attracting new bank customers increase as the income of the new customers decreases, but marginal revenues decline. Hence, profit-making institutions cannot provide normal service levels in poverty areas. As a result, the private burden of supporting the savings-investment cycle in such localities falls upon whatever financial institutions the poor improvise.

CONCLUSIONS

Banks combine the savings of depositors to create a capital fund for business, mortgage and consumer investments. Numbers banks mimic this rhythm, first taking in the "savings" of the poor, then returning capital to the poor community in the form of usurious loans, free loans, philanthropy and direct business investments by racketeers. Therefore, numbers gambling banks are an irregular financial institution.

Prevailing economic conditions in black neighborhoods mutely suggest that numbers banks have not provided a level of financial service sufficient to sustain economic development, even though they helped to close a gap left by the chronic malfunction of mainstream financial institutions. Numbers banking did not, therefore, represent a "sensible, strong, and adequate response to environment" (Glazer, 1971: 42). From the point of view of local economic development and collective social mobility, the numbers-loan shark system is less efficient than rotating credit associations, another financial improvisation of urban blacks (Light, 1972; Bonnett, 1976).

This explanation extends and amplifies the strictly cultural view of numbers gambling which has hitherto prevailed in sociological literature. Numbers gambling of blacks actually reflects the conjoint influence of institutional as well as cultural causes. On the institutional side, the malfunction of mainstream financial communities in low-income communities creates a financial problem. Residents reach into their cultural repertoires for solutions. The solutions they extract have consequences for the rate and character of local economic development.

The case of numbers gambling suggests that cultural and institutional causes may operate in tandem, and their segregation in sequential all-cultural or all-institutional historical phases is not an adequate resolution of their tension. Cultural repertoires offer whole or partial solutions for institutionally-induced disabilities. Therefore, the institutional disability and the cultural solution coexist. When institutions obstruct, victims cope. There is no

reason to suppose that all victims' cultural repertoires contain the same remedies nor that all remedies have identical consequences. True, numbers gambling is a remedy, but a wasteful one; and the chronic malfunction of financial institutions which encouraged this remedy does not render numbers gambling a fully satisfactory alternative.

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NOTES ON THE CRIMINOGENIC HYPOTHESIS: A CASE STUDY OF THE AMERICAN LIQUOR INDUSTRY*

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This case study of a single economic enterprise, stressing a symbolic interactionist perspective, examines the five tiers which make up the American liquor industry: distillers, distributors, retailers, drinkers and the legal order. Consistent with the interactionist method, attention is given to the relationships and actions that constitute the worlds of this social structure. The alcoholic beverage and the drinker-consumer relationship is a central focus of attention from level to level within the tiers of the industry. Special attention is given to criminogenic activities within and among the five tiers of this industry. The historical context and social interactions from which these activities arise are given emphasis. Finally, a tentative schema is suggested for analyzing criminogenic activities within the American liquor industry and, perhaps, within other industries as well.

Whiskey and alcoholic beverages have been produced in the United States since 1640 and have been taxed intermittently by the federal and state governments since 1791. The 18th Amendment and the Volstead Act of 1919 stopped the growth of the industry and made the production of alcoholic beverages (except for medicinal, sacramental or scientific research purposes) illegal. While a few distillers remained in production under strict governmental control, the majority of distillers were driven out of business to be replaced by bootleggers (Sinclair, 1964; Gusfield, 1963; Kane, 1965).

OVERVIEW

In this article I wish to examine, from a symbolic interactionist perspective, the organizational development of the American liquor industry since Prohibition.

* I wish to thank Patricia T. Clough for her assistance in this research. Graduate students in Sociology 414-416 at the University of Illinois gathered ethnographic data on the public drinking establishments discussed in the text. Portions of this research were supported by the Graduate College Research Board and the Department of Sociology of the University of Illinois. The comments of Evelyn K. Denzin, Sidney J. Kronus, William Cockerham, Stanley Cross and Julian Simon were particularly helpful. Finally, I wish to thank Catherine Daubard for her assistance in the ordering and reconceptualization of the entire research question.

Blumer's (1969) proposals for the foundations of a theory of organizations suggest that the interactionist examines and stresses the informal, as opposed to the formal attributes of any organizational complex. Rather than viewing organizations in rigid, static terms, the interactionist sees organizations as living, changing forms which may outlive the lives of their respective members and, as such, take on histories that transcend individuals, conditions and specific situations. Rather than focusing on formal structural attributes, the interactionist focuses on organizations as *negotiated* productions that differentially constrain their members; they are seen as moving patterns of accommodative adjustment among organized parties. Although organizations create formal structures, every organization in its day-to-day activities is produced and created by individuals, individuals who are subject to and constrained by the vagaries and inconsistencies of the human form. Organizations ((including the licensed alcoholic-beverage industry) are best conceptualized as complex, shifting networks of social relationships. The sum total of these relationships—whether real or only symbolized, whether assumed and taken for granted or problematic and troublesome—constitute the organization as it is sensed, experienced, and acted upon by the individual or relational

member. Power, control, coercion and deception become central commodities of negotiation in those arenas that make up the organization.

Hamilton et al.'s (1938:3-4) image of an industry is well-suited to this analysis of the liquor industry. They remark:

In a literal sense, there is no such thing as an industry. . . . Instead . . . there is only a host of individuals . . . engaged in a varied assortment of personal activities—the digging of coal, the smelting of ore, the advancement of personal fortunes. . . . They are human beings who engage in human activities. . . . It is amid this babble of tongues, this confusion of purposes, this drama of divergent dramas that industry is to be found. . . . Yet industry is a name for what is at best a loose aggregate of business units engaged in performing a single service or producing a single commodity. . . . An industry is like an individual . . . it has a character, a structure, a system of habits of its own. Its pattern is out of accord with a normative design; its activities conform very imperfectly with a charted course of industrial events.

This investigation, starting from such assumptions, utilizes a case study method and focuses upon interactions and negotiations among members of this industry. Data have been drawn from all levels of the industry, but in particular the state of Illinois and the twin cities of Champaign-Urbana. The attention to negotiations among organizational members led directly and immediately to the consideration of criminogenic activities. I shall examine the extent to which criminogenic and quasi-legal behaviors have served the interests of the members of this industrial complex (see Bell, 1960; Gross, forthcoming). I hope to show that this criminogenic tendency (that is, the intended and unintended violation of the legal code) is present at all levels of the industry and can be traced to the historical context of the industry's development, to its present structure, and to the relational patterns which exist within what appears to be a single economic enterprise. Basic to the analysis which follows is the assumption that the organizational structure of any industry involves multiple tiers, levels or social worlds of interaction. Taken together, these multiple segments

constitute the organization as its members know it (Hamilton et al., 1938). Five such tiers, manufacturers, distributors, retailers, the legal order and drinkers, will be identified. Criminogenic activity evolves as a result of interaction among (as well as within) each of these tiers.

As one moves from the local level to the state, regional and national levels of the industry, the actual drinking transaction and the drinking act become less problematic. What emerges as problematic is the production, distribution and sale of a single product—the alcoholic beverage. This product—however bottled, branded, endorsed, packaged, priced or displayed—must somehow reenter the system where its last contender exited, be this the storeroom of the wholesaler, the shelves of the retailer or the refrigerator of the consumer. These two objects—one an actor (the drinker-consumer), the other a product (the alcoholic beverage)—emerge as central to the American liquor industry. They must be brought together, over and over again, and American society has given the liquor industry the license and mandate to join these two commodities in any of a variety of ways (Abrahamson, 1938:395-429).

HISTORICAL CONTEXTS

The interlocking effects of Prohibition and Repeal set the historical context for the post-1934 expansion of the liquor industry, and their enactment served to make the liquor industry a unique enterprise within the American marketplace.

Perhaps the most striking effect of Prohibition/Repeal was the concentration of alcohol manufacture in the hands of four large distillers—(1) Distillers Corporation (Seagrams, Ltd.), (2) Schenley Distillers Corporation, (3) National Distillers Products Corporation and (4) Walker (Gooderham & Worts, Ltd.). By the mid-1920s the Big Four had absorbed some 60 distillers in Illinois, Indiana and Kentucky (East, 1952; Oxenfeldt, 1951). The Big Four, unlike many of their smaller competitors, were able to survive Prohibition because, concomitant with their cushion of capital, (1) they were able to establish

or maintain Canadian bases of operation and/or (2) they were able to receive government sanction for the production of alcohol for legal uses (Oxenfeldt, 1951).

Prohibition and the public furor accompanying it convinced the large distillers that, if the production of liquor was to be not merely legal but also profitable, the public image of the liquor industry (and of alcohol consumption) must be transformed. This attempt at dramaturgical management took two directions: (1) to transform the image of the distilling industry itself which had been tainted by the violence and corruption of bootleg production and (2) to transform the act of alcohol consumption to a socially-approved act. The latter was especially important because temperance forces and anti-saloon sentiment among large segments of the population did not immediately dissipate with Repeal. The following statement well illustrates the tactic used by distillers to create a more favorable image for alcohol consumption:

In order to forestall attacks from various dry organizations, which the WTCU spearheaded, we emphasized one of the basic principles of the Distilled Spirits Institute [DSI]: moderation, moderate drinking. Don't over-sell, don't over-drink. [Frank Schwengel, retired brigadier-general and marketing advisor for Seagrams]. (Durrell, 1967:3)

A concomitant technique was to disassociate the distilling industry from tavern consumption. According to a prominent legal consultant for DSI (field interview, July 1975), after Repeal, distillers shifted from a saloon concept of consumption to a carry-home model. Distillers turned their economic efforts to the construction of a "rational," non-tavern, retail-outlet sales program.¹ By separating themselves from taverns, the distillers placated the anti-saloon-league advocates and succeeded in placing the blame for alcohol misuse upon the saloon drinker.

¹ Brewers were given taverns, or so it appears. As one distilling executive remarked, "There are only ten bars in Chicago where I want our product sold" (field interview, June, 1975). Today, approximately 75% of all alcoholic beverages are consumed in off-premise locales.

The passage of Repeal was proposed, of course, under the condition that not only would the production and sale of alcohol be legalized, but also that government regulation and taxation would be imposed. Much of the propaganda extolling government regulation and taxation (and, thereby, gaining support for Repeal) was financed by large distillers and brewers (Dobyns, 1940:6). For some groups, the economic benefit of alcohol taxation held sway. Much publicity stressed the loss of tax monies from liquor sales combined with the high cost of attempting to enforce Prohibition. Business, chafing under the taxation burden of World War I, was particularly susceptible to this argument and threw support behind the Repeal movement. Propaganda, supported by the large corporations (particularly DuPont), went so far as to promise that the recent and unpopular income tax could be eliminated by the use of tax monies from alcohol. Other groups, even some sincere temperance groups, were influenced by the supposed benefits of government regulation. The argument was emphasized that Prohibition bred more social ills than it cured and that the bootleg trade was far more detrimental than would be a well-regulated, legal industry (Dobyns, 1940:19-27).

It was hoped that a conciliatory attitude toward regulation and taxation by the distilling industry would pay off in the long run by moderating public opinion of the industry itself and of alcohol use. In the meantime, the liquor industry encouraged government control as the price it was willing to pay for legalization.

As it turned out, government regulation never proved to be much of an inhibition to profits. One reason for this was that the liquor industry itself helped to write the rules. Within ten days of ratification of the 21st Amendment, a Code Authority was created, consisting of the top executives from the large distillers. President Roosevelt directed the Code Authority to draw up a set of guidelines for the federal regulation of their industry. Owsley Brown (president of Brown-Foreman Distillers), L. S. Rosensteil (Schenley's) and Frank Thompson (Seagram's) created the Distilled Spirits Institute (DSI). In the im-

mediate post-Repeal days, the industry, through DSI, worked closely with the federal government—a government-industry relationship which may be without precedent in American history (Durrell, 1967).

DSI encouraged high government regulation and taxation as a cover behind which to hide and reform their collective self-image. While appearing to meekly accept a weak position, they learned from their industrial counterparts in oil, railroads and automobiles how to cover their ever-expanding behaviors through the newly emerging and complex antitrust laws. The liquor corporations kept a low profile, concentrated on dramaturgical management, and busied themselves behind the scenes in the direction of vertical and horizontal integration. Furthermore, Repeal put a great deal of the responsibility for regulation of the liquor industry upon state and local officials (as well as national officials). This placed liquor corporation representatives in a situation where they could corrupt the very persons who were intended to control them.

A final consequence of Prohibition/Repeal was that government regulation introduced what came to be termed the *three-tier system* of distribution in the wine and spirits industry. Under this system,

the three levels of supplier, wholesaler and retailer must be maintained separate and independent of each other, and the supplier must sell only to the wholesaler, and the latter only to the retailer. The wholesaler, therefore, stands between the retailer and the manufacturer. (Switzer, 1975:5)

Hidden in the above language is one basic point which had been the touchstone of the Anti-Saloon League; namely, that distillers and brewers owned, or staked tavernkeepers, imposed quotas upon them and forced them to take money out of the pockets of family men (Sinclair, 1964; Gusfield, 1966). "Tied taverns" or "tied houses," as they were called, permitted outside interests to control the politics, moralities and drinking patterns of local communities. By placing the wholesaler between the manufacturer and the retailer, the state and federal governments attempted to break the tied-tavern relationship—at least on the surface.

OBDURATE REALITIES

At the national level, Repeal produced a complex set of laws and procedures unique to every state in the United States (see Distilled Spirits Council, 1974). These procedures include tax and its collection procedures, size of retail containers, local option rulings on retail sales and license permits for distillers' representatives. Also at the national level, antitrust laws and federal tax statutes² set boundaries for corporate activity.

At the state level, each State Liquor Commission sets the regulations and modifications of the three-tier system of distiller, distributor and retailer. There are two state models. *Monopoly states* permit the sale of bottled distilled spirits only in state-owned stores. *License states* are states where distilled spirits are sold under a licensed, private enterprise system. This system operates with three variations. For example, in Illinois distributors are given territorial monopolistic rights for the distribution of a particular brand-name product. In addition, licensed resident manufacturers (rectifiers) may sell beverages manufactured within the state directly to retailers (Switzer, 1975).

States also set minimum age limits for consumption of alcoholic beverages and, in some states, this may be differentiated between hard liquor and beer or wine.

At the local level, local liquor commissions (which in Illinois are headed by the mayor) set laws regulating the number of and fees for liquor licenses, decide who receives liquor licenses, and define operating procedures for retail establishments (i.e., hours of operation). Local communities also may set limits such as

² Government taxation and regulation created a dual effect upon the current system. Liquor taxation is the most expensive factor in producing every gallon of distilled alcohol. Federal taxation is \$10.50 per gallon, state tax may be as high as \$2.50, and some communities impose a local option tax. This situation causes distillers to comment that the cheapest product in their industry is the alcoholic beverage. Because federal and state taxes must be paid by the distiller before the product leaves his warehouse (i.e., before payment of account), the liquor industry is a high capital, low-labor business—a characteristic which discourages competition from small-scale entrepreneurs.

minimum age for consumption of alcoholic beverages and impose a local excise tax on alcoholic beverages.

These legal procedures, variably enforced and understood, influence the operation of this industry at every tier-level. Such codes also define the circumstances for criminogenic behavior at each level. Members of one tier may be unaware of, or only partially understand the laws which apply to any other tier. In addition, they may be unaware of, or misunderstand those laws and regulations which apply to their own level (field interviews, 1975).

FIVE-TIER STRUCTURE OF THE AMERICAN LIQUOR INDUSTRY

As Hamilton et al. (1938:3-4) observed, there is no such thing as a single industry. At least five different tiers resolve into distinct social worlds which constitute those collective realities that make up this particular industry. The 21st Amendment recognized the three tiers of distiller, distributor and retailer. Closer inspection reveals the presence of two additional tiers: first, drinkers themselves and second, those individuals and agencies committed to the enforcement of the liquor laws—state and local liquor commissions, the police, the FBI, the Treasury Department, and the Bureau of Alcohol, Firearms and Tobacco (BAFT).

Distillers

At the executive level of rival distilling firms, hostile, not cordial or congenial relationships appear to be the pattern, although these participants will seldom permit personal animosities to enter into profitmaking decisions. Oxenfeldt (1951:478) notes that the executive offices of the large distillers are all located in New York City, within a short radius of one another. (There has been a recent regional shift to Chicago.) The executives meet often and know one another well. With few exceptions, they belong to the same organizations and clubs, including DSI and the Licensed Beverage Industries Institute. They also contribute to the same

charities, all in the collective name of the industry. Oxenfeldt (1951:478) states:

There is no close personal friendship between the top executives of the major distilling corporations . . . their relations vary somewhere between friendly rivalry and intense personal antagonism.

Conversely, separate distillers tend to be tightly bound through kin relationships. Of the four major whiskey producers, all but one are family controlled, with control extending into pre-Prohibition days. (For an illustration of one such family, see Gaines, 1975.) The same pattern appears to hold for brewers and wineries, at least until quite recently (see Horowitz and Horowitz, 1965).

Two highly visible phenomena at the distilling level are the emphases placed upon in-house legal advice and upon advertising. Every major distiller, brewer and vintner employs a legal staff. Its chief function is to monitor the changing laws within each state, to keep in touch with relevant legal representatives of the industry, and to represent the best interests of the industry to local, state and national political entities. They ensure that their corporations do not grossly violate any statute or ruling that would inhibit the sale of their products. These staffs also enter into negotiations with distributors when territorial contracts are reviewed annually and new quotas set (see Distributor section).

The respective advertising arms of the industry bid against one another at the agency level to capture ever-larger drinking audiences (Bretzfeld, 1955; Simon, 1969). Advertising points to perhaps the largest and most heterogeneous sector of the liquor world: the drinker, the ultimate point of contact for all the interactions that make up this world. The drinker can make or break new programs. His purchasing patterns determine package programs and advertising campaigns, and his shifting moods—regarding taste, product and brand—enter into the long-range planning programs of the distiller, distributor and retailer.³

³ One example will serve to highlight the centrality of the drinker-consumer in this process (but also see

Distillers also engage in product promotion within their own ranks. One distiller gives its corporate executives a "Friday package" which consists of a bottle of gin, vodka and blended whiskey. This is expected to last one week, and they are expected to display these products when they host social occasions. Each Friday afternoon at 4:00 PM these executives leave their offices with their gifts wrapped in brown paper bags. Their corporate identity is maintained, although the price of their gift is hidden (field interview, July, 1975).

The most striking characteristic of the distiller level is that such activity is a huge and complex concern, national and even multi-national in nature. Large corporations view themselves as Big Business and define their activity as modern, "rational" promotion of a product similar to any other product. The attitude of the modern corporate executive toward his product equates the sale of whiskey, beer and wine with the marketing of standard grocery items. The vice-president of sales for a large corporation which holds sole United States rights to import one of the four top scotches in the world remarked, "Scotch is no harder to sell than shoestrings or overalls. It's a matter of marketing, promotion and packaging" (field interview, July, 1975).

Distributors

As in the distilling tier, family ties bind many of the distributorships in the area studied. Eight of the ten wholesalers in the

seventeen-county area under consideration (downstate Illinois) are related either through marriage or direct kin lineage.

The most important advertising commodity or medium for the distributor is the street salesman. He or she links the distributor to the retailer and thereby connects the customer to the distiller. Distributors (through street salesmen) must court retailers to maintain a clientele, and this courting process takes the form of special attention, extra visits and other favors. Distributors are required, at the same time, to conform with state regulations. For instance, the state of Illinois demands that all beer be paid for upon delivery and gives 30 days for all other alcoholic products. If a retailer or tavern owner fails to meet a payment deadline he or she goes on a delinquent payment list that is filed in the state capitol each Wednesday. No distributor can legally deliver to a retailer whose name is on the delinquent list.

In Illinois, distributors typically receive a territory for the sale of a particular distiller's products. Wholesalers cannot sell outside of their territories, but the agreement is that only one contract will be awarded per territory. Territories, however, are not always clearly boundaried, and wholesalers often find themselves in a situation where a particular distiller has "dualled" their territory (such a move may also be a threat or attempt to force the distributor to meet higher quotas of product sales). This means that two wholesale houses are in fierce competition to sell the same product to the same retail outlets. "Dualing" leads to deals in an attempt to control territories.

At the state level, the distiller is supposed to verify his territorial commitment to specific distributors by annually filing a list of accredited distributors who can sell his products. Typically, in order to obtain such a territorial monopoly contract, distributors must meet quotas. When contracts are terminated, termination is generally initiated by the distiller, who will argue that the distributor has not given his product sufficient attention over the last sales period. Since the distributor depends on distiller contracts, he must move a certain amount of the product to survive. He

the discussion of street salesmen in the Distributor section). After eight years of planning, testing and field experimentation, Heublein, Inc., in the fall of 1975, introduced a new product named "Hereford Cow" which comes in chocolate, mocha and strawberry flavors. Aimed at youth, it was introduced simultaneously in two test cities, Champaign-Urbana and the South Side of Chicago. Hereford Cow was forced onto the shelves of all the bars served by the local distributor (see discussion of off-invoicing). Within four weeks it was rejected by all bars. While its initial sales in South Side Chicago of 150,000 cases in two weeks far surpassed expectations, it has since dropped off in sales. Heublein is searching for a new product for this particular audience (field interviews, August, 1975).

must convince retailers to stock a certain amount of that distiller's products. At the same time, if the retailer feels that he is being overloaded with unsaleable products, he is likely to take his business elsewhere. In this way, the street salesman must help the retailer to please his customers. However, the distributor can be caught between the public and the distiller. One example comes from a street salesman who serves small racial and ethnic retail liquor outlets in South Chicago. The drinkers in this area prefer to purchase (in pint-sized quantities) only the most prestigious scotches, bourbons, gins and vodkas. However, they change their taste patterns almost weekly, leaving the salesman either understocked or overstocked for his outlets. (This drinker, who comprises over 50% of the drinking population in Washington, D.C., Chicago and New York City, is particularly problematic for the distilling industry [see *Liquor Handbook*, 1975].)

Some distillers do, however, attempt to establish distributor loyalty to their prime items. One major distillery has an exclusive club to which only presidents of distributorships can belong. Membership in the club is passed on from father to son; hence, the name "Happy Sperm Club." Should a father fail to produce a son, he must marry his daughter to a male who is willing to enter the business. Once the male heir assumes control, the father relinquishes his membership in the club. A common practice of this club is to schedule a two-week vacation in some exotic resort area for distributors. Such notables as Gloria Steinem, Tom Hayden, Huey Newton, Merle Miller, Walter Hinkle, Bill Cosby and Don Rickles have lectured and entertained at these functions.

Distributors represent the weakest link in the three-tier model. They can be rejected by retailers or find themselves either "dualled" or without a contract from the distiller. They are in a poor bargaining position and are under constant pressure from the distiller to meet new quotas. Retailers can turn elsewhere; without distiller contracts, franchises and retail accounts, the distributor is out of business.

Retailers

Retailers include both package outlets and on-premise sales (taverns). In 1944, 34 families controlled 60 of the 80 local taverns in Champaign-Urbana. Since then, the number of taverns has more than doubled and the same families control a large proportion of these enterprises. One family controls all but one of the six local retail sales outlets (excluding grocery stores and drugstore chains). That family now has a virtual monopoly on retail sales. Family control typically extends through three generations. Intense family loyalty is engendered and a kind of entrepreneurial pride characterizes these industries. For instance, one of the principal liquor retailers in Champaign-Urbana, when asked to comment on his chief competitor, stated, "It is my goal to drive that SOB out of the business. Even if I have to lose money on a particular deal, I will make a fool out of him." Retailers also disrespect outsiders who rely upon chain-store assistance in the sale of alcoholic products. A large up-state retailer remarked,

I started in 1934 with a corner shop and couldn't even get a sign in the door. I paid \$200 a month rent. I paid off the bootleggers, the police; I kept up my bills. I didn't let the outsiders drive me out. What do these people in the chain stores, the grocery stores and drug stores think they can do? They don't know the business. They'll never drive me out. (field interview, June 1975)

The modern retailer reads the *Liquor Store Magazine* (1974). He permits the distiller to rent shelf space in his store for prominent display of products. He thinks in terms of more efficient ways to increase profits.

The retailer must renew his license annually. When licenses are renewed (they cost from \$1,250 to \$1,500), the applicant is fingerprinted; local liquor commissioners use this occasion to remind the applicant that it is a special privilege to hold such a license. Licenses can be suspended from two to thirty days, typically for serving underage drinkers; and holders of liquor licenses cannot hold elected public office in the state of Illinois.

The local retailer feels a resentment

toward the legal order, feeling that it deprives him of his political and economic rights. They resent the outside chain-store entry into their market. (Between 1967 and 1975, the number of retail licenses in Champaign-Urbana doubled, especially those for the off-premise consumption of beer, wine and distilled products. All but two of the new permits went to grocery and drugstore chains.)

While retailers must maintain a cordial relationship with the local legal structure, the nature of their business often demands that they engender a regular clientele. These two demands can conflict with each other, as when drinkers resent being asked to show proof of age or to wait in line while others do so. Bars or taverns often find that they must create a unique social world within their establishments to produce patron loyalty. One means for doing this is to adopt or manipulate the relevant social objects or symbols of the clientele they wish to attract. This can include the display of fraternity mugs and paddles, holding "playboy bunny" nights, sponsoring intramural football teams and, in some cases, hiring university football players as bar bouncers. This serves, in part, to connect the bar with the university's athletic department.

Drinkers are often regarded by bar owners as fickle. One local bar which had been existence for 25 years changed hands in the summer of 1975. The new owner, whose father had in fact owned the property, completely changed the decor and pinball arrangements in the "new bar." He immediately lost a regular crowd that had roots going back 25 years. He is still in the process of regaining that lost crowd.

The retail level connects most directly with the drinker. The retailer must, however, negotiate with all other tiers in the industry. It is the most conspicuous, yet perhaps the most complicated tier of all.

The Drinker

The drinker is the presence behind every other tier. Studied, courted, deceived, he keeps the system going. As we shall show later, distillers, distributors and retailers routinely violate or challenge the civil-legal code, yet are seldom taken

to court. It is the drinker who receives the most direct scrutiny by law enforcement officials and who is the most likely participant in the system to bear the legal burden of illegal production, distribution, sale and consumption. The drinker's use and misuse of alcoholic beverages serve as a reminder that the object called alcohol is consumed and acted upon under a variety of social definitions and circumstances, some of which lead to unlawful behavior.

The Legal Order

The legal order consists, at the *local level*, of liquor commissions, the police and the courts. Retailers and drinkers receive the closest surveillance at this level. At the *state level*, state liquor commissions monitor the conduct of retailers, distributors and distillers. At the *national level*, BAFT, the FCC and FTC, and the Treasury Department (more generally) are responsible for locating illegal liquor transactions.

The federal and state monitoring of liquor laws and liquor violations is weak. In Illinois, at the state level a six-person Liquor Control Commission, consisting of a president, secretary and director plus three adjunct members, oversees the operation of the state's liquor code. This commission is reappointed after each gubernatorial election and has a nine-member investigative team that covers the entire state. The federal government, except through the understaffed operations of the BAFT and FTC, is also in a weak position to monitor violations of the liquor codes. The failure of the state and federal governments to cooperate has led to a situation that currently is being exploited by organized crime (Cecil, 1974:2).

At the local level, at least in theory, the administration of the civil-legal code is much more intense. The fact that all enforcement branches (liquor commissions, police and courts) may be intertwined politically potentially can mediate and distort the function they are intended to fulfill.

At the legal level, the liquor industry is a very loosely-monitored, semi-understood and taken-for-granted system,

as the following interview with the previous president of the Illinois Liquor Control Commission reveals. When asked about Regulation 6, Article 4, which requires that all distillers place on file the territories wherein they have granted distributors exclusive sales rights, he remarked, "I have never heard of the rule; if it was important I would know about it" (field interview, June, 1975). A prominent legal consultant for the distilling tier confessed that often members of the state liquor commission call *him* when they have questions concerning the meaning of the law. Certainly, few drinkers are aware of the laws that limit the number of retail outlets in any city, nor are the prices of licenses commonly known. Additional complexity is introduced when multinational corporations buy up specific distillers and purchase exclusive sales rights to specific brands. One retailer remarked, "You never know from day to day who owns these damned corporations or what their policies are" (field interview, August, 1975).

TIERS: COOPERATION, COMPETITION AND CRIMINOGENIC ACTIVITIES

The present research focused on the organizational development since Repeal of the five tiers of the industry, with special attention given to criminogenic activity, be these behaviors of omission, commission, evasion or outright illegality. We shall examine such behaviors within each tier as well as those which emerge from the interrelationships among tiers.

Repeal opened the way for a small group of distillers at the national level to capitalize on a relatively new and ambiguous situation. Not only did they have to redefine themselves for the federal government, but they also had to formulate a collective image of self that could be sold to the public. The following discussion of each tier suggests that the industry has succeeded in becoming an institutionalized American economic industry. Gross instances of corruption and illegal behavior became clouded behind complicated corporate laws and local statutes. In the end, drinkers, tavern owners, retailers, wholesalers, manufacturers and

the legal structure all profited. Deception and illusion became marketable commodities. Bootleg liquor gradually disappeared and up-graded alcoholic products appeared on the market. A normalized system that assumed a taken-for-granted nature emerged, was solidified and, gradually, became institutionalized into local, state and national laws.

The multi-tiered liquor industry in the mid-1970s appears to be a highly stable enterprise. However, hidden behind its interactional structure are a variety of criminogenic activities, usually quite deliberately calculated, which economically benefit one or more participant or tier in the system. The economic benefits are not confined just to profit margins on the stock market, nor to the dollars produced by a single bar or retail outlet at the end of a year. Rather, the economics of the liquor industry translate into private, personal economies of style, career, prestige, power, collegiality and friendship. At each tier, different economies are battled over. This produces different criminogenic activities at each level. Often clouded behind elaborate symbolic campaigns and structures—e.g., a new drink for the young urban sophisticate—the industry and its participants enhance their own positions at someone else's expense. The competitive interactions that tie tier members together encourage intra-industry exploitation and criminogenic conduct (Benson, 1975; Gross, forthcoming). We shall present evidence in support of this argument, beginning with the distiller who is the most powerful force within the industry.

Distillers

Distillers violate as well as challenge antitrust laws. Large corporations can hide behind complicated corporate structures which allow them to gain major holdings in competing lines or to diversify within the same product line.

A case in point is offered by the history of Heublein, Inc., which produces vodka, mixed cocktails, dry gin, cordials, beer, rum, table wines, sherries, and all wines sold under the names of Italian Swiss Colony, Inglenook, Lejon Petri and Gam-

barelli & Davito. Subsidiaries of Heublein, Inc. include Allied Grape Groups (82% controlled), United Vintners, Inc., Kentucky Fried Chicken Corporation, Heublein (Canada, Inc.), Heublein International Ltd. and Smirnoff Beverage & Import Company. The 1968 acquisition of controlling interest in United Vintner's Inc. led the FTC to issue a complaint that Heublein's was in violation of the Clayton Anti-Trust Act. Heublein's denied the charge, asserting that its interest in United Vintner's gave California grape growers a marketing edge to meet industry competition. The FTC dropped its charges.

Major distillers, such as Heublein's, flirt with antitrust laws. They are seldom brought to court, and when they are, high-ranking corporation executives often profit by relinquishing their shares in a competing corporation for a handsome profit.⁴

Distillers often negotiate with one another over the relative prices they will set for their respective products. Federal law requires that no distiller market a product at a price lower than the price he lists at a designated time in an affirmation state. Deceit, deception, misrepresentation and "unintended leaks" of information abound at these times. At least one large scotch manufacturer is now under suit for his latest filing procedures. Lax monitoring at the state and national levels leads members of the industry to sue one another over pricing procedures and pricing violations. In this sense, they police themselves.

Not only do manufacturers violate antitrust laws but they also exert excessive pressure to meet quotas upon distributors

who, in turn, pass this pressure along to the retailer. In response to such pressures, retailers often deceive the customer into believing that he is getting a good buy on a particular product. A specific problem that ties the distiller to the wholesaler and to the retailer is the *off-invoice strategy* which arises from competition among wholesalers. Distillers put pressure on wholesalers to move a particular product by threatening to withhold supplies of a top-selling item. They demand that wholesalers buy more of this product than they can normally move in a month's time. To get his money out of the unnecessary inventory, the wholesaler will give retailers extra liquor, off the invoice, as long as the retailer buys a portion of the premium liquor. This violates several laws. The retailer may make the sale, not ring it up on his cash register and, thereby, avoid federal and state taxes. The wholesaler, who has paid the proper taxes on the products he has received, violates federal and state laws by keeping a second set of books which falsify his invoice orders to the retailer (see Farberman, 1975, for a similar instance in the auto industry).

A third form of distiller-manufacturer behavior, which does not violate any specific laws but which deceives the customer, is the practice of *multiple branding*. Abrahamson (1938) reports that distillers believe that consumers have only the haziest notions of the differences between whiskeys. Accordingly, the more brands a distiller can offer, the greater the likelihood that he can capture a broader sector of the market. In 1951, it was estimated that there were over 30,000 rival brands of whiskey on the national liquor market which were controlled by less than 40 separate distillers. Single companies may have as many as 50-100 different brands. National Distillers markets ten brands of bourbons, each appealing to a different audience, each marketed by the same distiller. Immediately after Prohibition, when the stock of aged whiskey was at an all-time low, blended whiskeys came on the market. In order to encourage the purchase of their products, distillers prefaced all their labels with the word "old"—thus, Old Taylor, Old Rip Van Winkle, Old Charter, etc. (Abrahamson, 1938:414).

⁴ A former president of a major United States distillery gave the following information in an interview, July, 1975.

In 1934, his brother joined forces with a Canadian distiller and they profited greatly in post-Prohibition days. In the 1950s, the brother joined another firm which acquired the rights to a particular brand of scotch. At that time, the firm held controlling shares in the three top-selling scotches in the world. In 1968, the FTC brought an antitrust suit against his firm. The antitrust suit was tied up in the courts for six years. The FTC won and forced him to sell his shares in the company. He sold his shares for a 50% profit. The result of the FTC's suit was to make profits for the offender.

Branding produces product identification, which is intended to produce self-identification on the part of the drinker. The distiller endeavors to cultivate drinkers who will purchase only prestige items. The distiller manipulates the drinker and attempts to maintain the illusion that certain brands are still distilled by the same families that originated the product. Most, if not all, original scotches and bourbons initially tied to specific families have long since been purchased by large-scale corporations who simply maintain the original brand name and label.

Quasi-tied houses represent a third form of distiller-brewer manipulation of the legal code. While the three-tier model prohibits the intervention of the distiller-brewer in the actual sale of his product, informal agreements arise which tie taverns to manufacturers. This tie is made through the wholesaler (who himself may be tied to a particular distiller or brewer) who does favors for tavern owners. Wholesalers make special runs. They stop by up to three times a week to chat with tavern owners and managers. They fill special orders and make special deals to favored customers. A *norm of reciprocity* (Gouldner, 1959) is expected to operate. In return for special services, the tavern owner is expected not to solicit or to indulge in the services of another distributor, even if his "tied" distributor cannot deliver the products he needs. This norm locks the tavern owner into a compromising and, at times, uneasy relationship with the wholesaler.

Distributors

As noted earlier, distributors are prohibited from selling outside their prescribed territories. However, *territorial violations*, in which distributors cross one another's territory and make illegal sales, represent a frequent form of illegal behavior on the part of the distributor. A *norm of silence* operates; one will not tell on another wholesaler if that one will not tell on him. Similarly, retailers who can make profit purchases are not about to report that they bought from a wholesaler outside their territory. (In Illinois, there is

the famous rumor of a 4,000-case purchase of a Canadian blended whiskey by a retailer from a distributor who was 200 miles outside his territory. The retailer, one of the largest in the state, could laugh about the matter and the wholesaler in question could deny it. The case was never investigated.)

Distributors can negotiate with one another. In Champaign-Urbana, in response to a recent labor strike at the Anheuser-Busch factory in St. Louis, the local distributor holding the sole rights to Busch products found himself out of stock. He began selling what he termed "brand X" which, upon inspection, was found to be Old Style beer. The local Old Style distributor denied selling it to the Anheuser-Busch distributor. The matter came under brief investigation by the Executive Director of the State Liquor Commission but nothing came of it (Weiss, 1976). Some distributors agree to ignore infractions and permit each other to serve old customers, usually family friends, who reside in another's territory (field interview, September, 1975). Such agreements engender goodwill and lead distributors to help one another out when they find themselves short of some needed product.

Distributors must negotiate the payments they receive for delivered goods to retailers. As mentioned before, all beer must be paid for upon delivery and all other alcoholic products within 30 days or the retailer goes on a delinquent payment list. No wholesaler legally can distribute to a retailer whose name is on the delinquent list. However, *delinquent list manipulation*, in which a distributor withholds a name from his delinquent list, constitutes another semi-illegal act on the distributor's part. Variations include rebuying overstocked products with kickback funds or extending illegal credit to a retailer by doctoring the invoice books.

The *Wall Street Journal* featured an article noting that distributor kickbacks to retail stores seem common in New York City (Kwitny, 1975). The author notes that at the wholesale level: (1) salesmen carry cash from store to store to give kickbacks to retailers; (2) wholesalers ship free loads

of whiskey to retailers and account for the missing merchandise by reporting a warehouse theft; (3) distributorships list salespersons who receive high commissions which actually go to retailers who patronize certain houses; (4) some wholesale houses print lists indicating the amount of kickback money that is available when a retailer purchases certain products.

Distributors also can make agreements with specific retailers or tavern owners to supply them with specific goods or services, any or all of which may be illegal or semi-illegal. In 1944, the Illinois Liquor Control Commission Code made it illegal for any distributor to provide a tavern owner with walk-in coolers, free glasses, free calendars, clocks, bar equipment, mirrors or outside display signs. The economic slump of 1971-1974 led distributors to begin enforcing this law informally, telling their favorite tavern owners that "the feds had come down on them and they could no longer provide these services" (field interview, August, 1975).

Retailers

The retailer is the most entrepreneurial and independent operator in the system and is, thereby, most free to manipulate, deal and negotiate agreements that are in his own best interests. Retailers who achieve sufficient size are in an enviable position for they can control wholesalers and exert pressures through wholesalers back to the manufacturer. Some retailers bypass distributors and deal directly with the distiller—a direct violation of the 21st Amendment. Retailers can manipulate distilling corporations into making special deals with them simply because they can move large volumes of a particular product. In some situations, a retailer can make arrangements to be served by a distributor who actually resides in another territory. In these instances, the size and power of the retailer lead him to take control over the local market distribution of a particular product. In one instance, a retailer virtually controls both a wholesale house and a distilling house. He buys outside his territory and refuses to permit his major distillers to deal with his chief local com-

petitor. If they deal with that individual, he threatens to cancel all contracts with the house in question.

Such illegal and semilegal behaviors enable community retailers to amass the power necessary to control local competition. A retailer bent on running a competitor out of business can, through deals, buy a product in sufficiently large quantity to systematically undersell his competitor.

Retailers can negotiate with one another, i.e., they may agree to set a bottom price on a certain product. Groups of tavern owners may agree to take a hard or soft line on the enforcement of the legal drinking-age law and they may or may not cooperate in the organization of a communication network that warns fellow bar owners of an impending police raid. Retailers can agree to cater only to certain drinking audiences and in a campus town such agreements often flow along the organizational lines of housing units, sororities and fraternities. On the other hand, outright battles may occur when a new bar owner enters the public drinking arena and attempts to take stable patrons from other bars.

Many tavern owners violate the laws every day. They serve underage drinkers and they fail to meet local building, sanitation and wiring codes. Bribes to building inspectors are not uncommon, nor are bribes to the local political structure. Tavern owners' violations come under the direct scrutiny of local liquor commissions, and they are frequently monitored by police departments through raids and periodic checks. Retailers are in a direct position to receive kickbacks from distillers and wholesalers as well as being the beneficiaries of delinquent-list manipulations. In a similar fashion, tavern owners and retailers give preferred customers charge accounts and reduced prices on certain beverages. Both practices are prohibited by local statutes.

The Legal Order

The political administration of local statutes can permit preferred citizens to obtain liquor licenses when others are denied such privileges. Blocks of votes can be purchased when local, state and national

politicians take stands on expanded liquor-licensing or increases in the tax on alcoholic products (see Henderson, 1934; Anderson, 1976).

Rubenstein (1973:420) has made the following observations (based on his field work with the Philadelphia Police Department from 1968 to 1971) on the politics of liquor control:

The state monopoly over liquor licensing has converted the right to drink into a source of political capital—cash and favors—which is exploited by state and city governments and their agents, the police, to assure compliance with their interests. Politicians exact enormous fees for selling and transferring lucrative licenses, and the police are obliged to maintain the efficacy of the system by harassing those who do not adhere and protecting those in favor.

Rubenstein could be describing several cities in the state of Illinois. Consider the following on the shakedown of tavern owners in the city of Chicago:

Former County Board Chairman — was found guilty on two charges that he shook down tavern owners in exchange for issuing or renewing their liquor licenses. . . . — has been accused of extorting \$17,500 from [two tavern owners] in exchange for helping [tavern owner] obtain a liquor license. (Fisher, 1976)

In Champaign-Urbana, the prices for liquor licenses, which nominally cost between \$1,250 and \$1,500 depending on the class of liquor type, were reported to range from \$500 to \$20,000 (field interviews, June-August, 1975). One individual reportedly received a second tavern with a renewed license in return for giving up a license. One applicant told interviewers he had been assured he would receive his license and proceeded to engage in expensive modifications of his new drinking establishment. Well into the project, he was informed that his license application was problematic and he would have to "speak to" certain persons about his proposed license (field interview, June, 1975).

At the local level, corruption, payoffs and conflicts of interest can be observed in any of the following areas: (1) license applications, (2) license renewals, (3) protection of licenses, (4) protection against raids and (5) alterations of zoning laws so

"dry" areas can be converted into "wet" areas where new licenses can be awarded.

At least two tavern owners over the last 30 years purportedly were driven out of business because they refused to "protect" their licenses. In another report, a dry township was converted into a wet township (as negotiations were underway to establish a large out-of-town shopping center) by the movement of a construction worker into the dry township for one month. A construction firm set up a house trailer for him and paid all his expenses. At the end of the month he called for a referendum to vote the township wet. Since he was the only resident in the township, the vote carried. The owner of the construction firm was a local politician.

The Drinker

Space prohibits any detailed discussion of the drinker as a separate tier within the American liquor industry (for a recent review of the drinking pattern of Americans, see Liquor Handbook, 1975). Observations gathered from distributors, retailers, tavern owners, distillers, importers and brewers do, however, support the following conclusions. First, the American drinker is a willing supporter of the industry that produces the alcoholic beverages they consume. That is, they purchase what is offered to them. Second, they are somewhat cynical, if uninformed, about the products they purchase. Third, the everyday drinker has little knowledge of the complex interactions that bind bar owners, retailers, distributors, distillers and the legal order to one another. Drinkers assume that the products they desire will be present when they want them. Laws, codes, liquor commissions, territorial boundaries and tied houses are not relevant realities for the drinker. The complex social structure that brings a particular brand of bourbon into the hands of a drinker is unknown or only barely glimpsed. Yet that structure exists because the drinker *qua* buyer assumes it will exist. The success of the industry has been, and continues to be based on the actualization of this assumption (see Cavan, 1967; LeMasters, 1975; Roebuck and Frese, 1975).

SOME NOTES ON
CRIMINOGENIC BEHAVIOR WITHIN
THE AMERICAN LIQUOR INDUSTRY

This case study of a single economic enterprise, stressing a symbolic interactionist perspective, has examined the five tiers that makeup the American liquor industry. Consistent with the interactionist method, attention was given to the relationships and actions that make up the world of this social structure. The alcoholic beverage and the drinker-consumer relationship constituted a basic focus of attention from level to level, tier to tier.

Any discussion of criminogenic behavior is complicated by the fact that there is no clear-cut theoretical consensus of what constitutes a criminogenic act. It can be assumed, however, that in order for a criminogenic act to be produced, an individual or corporate structure must (1) be in a situation wherein they have the *opportunity* to produce such an act, (2) have the *means* at their disposal for producing that act and (3) possess a *motive* or explanation to account for that behavior.

Although a discrete theory of the emergence of criminogenic behavior within a complex organization is beyond the scope of this paper, a few tentative observations which fall within the schema suggested above can be made.

Opportunity

Concealment, by secrecy or ignorance or both, appears to be a factor in some illegal and semilegal acts whose existence depends upon being hidden, either from other tiers or from the general public. Multiple branding, price fixing, or conflicts of interest are examples of behaviors which would not bear close scrutiny. The family bonds within the tiers of distiller, distributor and retailer probably facilitate concealment.

Perhaps the most striking characteristic of the liquor industry is *complaisance* on the part of the drinker-consumer. The drinker, although cynical, appears to find the convenient delivery into his hands of the desired alcoholic beverage to be the most relevant reality. Throughout the industry, although most participants assume

that the legal code is broken, few individuals care to pursue the matter. This may be particularly true for within-industry behaviors such as territorial violations, but it also appears to hold true for such illegalities as sale to minors, complicity of the political system and protection of licenses. Like organized crime, the liquor industry depends upon delivery of a product whose procurement is desired and whose price does not exceed what the customer is willing to pay (Vold, 1958:227). The liquor industry, as Al Capone suggested, is a "legitimate racket" (Sutherland, 1940:12). Although complaisance appears to contradict the role of concealment, this may be only a superficial paradox. The rule of the game appears to be that participants will close their eyes to all but the most obvious of indiscretions.

The complaisant and taken-for-granted attitude, both within and without the industry, taken toward criminogenic activities suggests that below the articulated legal structure there exists an *informal structure*, one which often contradicts or supersedes the formal structure. The informal structure may define as "legal" activities which are defined as "illegal" by the formal structure. The taken-for-granted reality of the former legal order is, perhaps, more illusory than it is concrete.

Means

The efficacy of many regulations depends upon accurate *self-reporting* by levels within the industry itself. Distillers supply figures for tax accounts, distributors are responsible for delinquent-list reporting, and retailers share in accounting for invoice figures. *Scarcity of penalties* and *weak enforcement* of laws often allow the industry to operate unmolested. *Structural ties between the political order and enforcement agencies* (such as those between local liquor commissions and the police) belie separation of power between legislation and implementation. Such ties collapse into one unit—the liquor industry—the essential ingredients of power, control and corruption. Bribery of officials (or shakedown of

retailers, depending on the viewpoint), protection of licenses, and alteration of zoning laws are particularly applicable in this category.

Motive

The multi-tier nature of the liquor industry seems to be linked to both crimes of competition and crimes of cooperation. Obdurate realities for each tier appear to be quite distinct and to create problems unique to participants within the same level. Individuals within each level often referred to themselves as "we" and definitionally aligned themselves with other members of the same tier. This *quasi group solidarity* may produce cooperational crimes such as price fixing, mutual concealment of violations and warnings of police raids.

Alienation from the law (usually the feeling that laws are unfair or unrealistic) also appears to be linked to crimes of cooperation, especially those which require cooperation between tiers to circumvent the law. Consensual definition of regulatory codes as "out-of-date" or "crazy" was used, directly or indirectly, as an *a priori* justification for breaking or bending the law (field interviews, 1975). Further, the widespread attitude among all levels that the liquor industry has a virtual mandate from the public (i.e., the passage of Repeal) provides the rationalization that many of the regulatory codes (defined as holdovers from Prohibition) are superseded by the "higher law" of Repeal.

Although the frequency of criminogenic behaviors and their significance for the industry overall awaits further studies, field interviews found the assumption of such behaviors to be common talk among all levels. This produces a *callousness of attitude* which crosscuts all tiers and appears to be a factor in crimes of competition. The assumption that other participants have few scruples fosters the belief that survival in such an arena depends upon adoption of the same attitude. This belief becomes the *sine qua non* for the presence of criminogenic activity in any organizational complex.

Clearly, the opportunities to engage in

criminogenic activities are not problematic for the participants in the American liquor industry. Indeed, in large part, they structure the situations, engender the motive and produce the means which allow them to engage in such activities.

It is not clear whether governmental control of such substances as alcoholic beverages engenders the kinds of industry-initiated criminogenic activities observed in the several tiers of the liquor industry. Bribery, kickbacks, antitrust violations, payoffs and the circumvention of legal codes may or may not be specific to this industry. It awaits further case studies of an historical and observational nature to determine whether or not criminogenic conduct is basic to the survival, growth and success of American economic enterprises. Until such studies are available, we can only conclude with the following observation of a high-ranking official in one American distilling firm:

We break the laws everyday. If you think I go to bed at night worrying about it, you're crazy. Everybody breaks the law. The liquor laws are insane anyway. (field interview, August, 1975)

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MULTIPLE ROLES AND ROLE STRAIN: SOME NOTES ON HUMAN ENERGY, TIME AND COMMITMENT *

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Sociologists generally invoke a natural "scarcity" approach to human energy, stressing the overdemanding nature of multiple roles. In contrast, a seldom used "expansion" approach provides an energy-creation theory of multiple roles rather than a "spending" or "drain" theory. Empirical literature only partially supports the scarcity approach view that multiple roles inevitably create "strain." Moreover, human physiology implies that human activity produces as well as consumes energy. We need a comprehensive theory that explains both the scarcity and the abundance phenomenology of energy. Such a theory requires careful analytical distinctions between time, energy, and commitments. It is argued that particular types of commitment systems are responsible for whether or not strain will occur. A theory of scarcity excuses explains how strain or overload is generally rooted in one such system. Scarcity excuses get implicit support from scarcity theories, and a sociology of these theories suggests their ideological basis.

When sociologists theorize about multiple roles, they often appeal to some basic "stuff" of social interaction—a biological attribute called "energy." No one has critically analyzed the use of this concept or specified its concrete reference. It seems to be posited as a prime term that needs no further explication. I have found two principal images of this energy attribute, and for convenience they may be called the "scarcity" and "expansion" approaches. As most modern sociologists take the scarcity approach, I shall consider it first in some detail before turning to the much rarer "expansion" approach. Later, I shall consider two other concepts—time and commitments—and show how these have been so confused

with energy that it has been impossible to avoid bringing in all manner of ideological excess baggage in treatments of multiple roles.

The Scarcity Approach to Human Energy

Freud (1961) presented one of the clearest expressions of the scarcity approach, claiming that civilizations and lovers are enemies who jealously compete with one another for the individual's libido, or energy. Civilizations get built through energy that is borrowed from sexuality, energy that would otherwise be consumed in erotic or familial relationships. For Freud, since people do not have enough energy to be *both* good civilization-builders and good lovers, civilizations must block the various channels through which energy could flow unrestrictedly toward sexual objects. They accomplish this blockade by normatively prohibiting homosexuality, by restricting childhood sexuality, and by enforcing the norm of monogamous marriage. In this way, "civilization is obeying the laws of economic necessity, since a large amount of the psychical energy which it uses for its own purposes has to be withdrawn from sexuality" (Freud 1961:51).

Note that Freud writes here of an *eco-*

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nomic contest within the psyche. In so doing, he abandons any attempt to arrive at a substantive understanding of human energy and instead shifts our attention to the imagery suggested by a metaphor. This economic metaphor, which may be called the "spending theory" of human energy, is one of the two most common energy metaphors appealed to by modern social scientists, who seem to have borrowed it uncritically from the language of everyday life. There, we speak of energy in terms of supply and demand. It is said to be allocated to, and then consumed in this or that activity; it is spent or expended, invested or saved.

The other metaphor to which the scarcity theorists appeal is borrowed from the plumbing trade. It sees human beings as walking plumbing systems who have various outlets for their energies, which are said to flow through various channels and, finally, down various drains. This plumbing metaphor, which I shall call the "drain theory" of energy, is found not only in the language of everyday life, but, like the spending theory, it has been exploited by sociologists in formal discourse. Thus we find Bittner (1963:935) claiming that radical social organizations have to worry lest their members' energies "leak out" of their own organizations. And Coser stretches the metaphor to what must surely be its outermost limit, when he suggests that "all Utopian communities were concerned with channeling the emotional energies of their members into the brotherhood rather than letting them *dribble away* into private and exclusive channels" (Coser, 1974:140; *italics mine*). And logically, why not? If human energy can drain away, then why should it not be able to "leak"? Or "dribble"?

The spending and drain theories both typically see energy as a scarce quantity of natural "stuff" that is each individual's private possession. Both imply that it is used up every day in the sum total of daily activity. The drain theory assumes that our reservoir of energy is precariously small, something akin to a mud puddle that can readily drain away or dry up altogether. The spending theory likewise implies a tiny amount of energy that can be expended completely. It is as if we

begin the day with an energy "allowance," which we proceed to spend in our various activities, each costing us some of our allocation, until finally, having exhausted our resources by the end of the day, we fall asleep, receive our allowance for the next day and begin the cycle anew.

The Scarcity Approach: A Review of Some Theoretical Literature

The scarcity approach has been employed in a number of sociological settings, including general theory (see Moore, 1960:814) and social criticism (see Riesman, 1961:64). Most commonly, we are likely to find the scarcity approach as an adjunct to theorizing about the alleged difficulty of managing multiple roles. Moore's (1963:108) succinct claim is typical: "Given the scarcity of time and energy, the probability of role conflict for the multiple joiner is somewhat more than abstract and hypothetical." Note that Moore's scarcity claim is expressed not as a variable but as a pre-theoretical given, so no empirical evidence need be paraded about to support it. But note also the momentous theoretical consequences of this scarcity background assumption, for without it, the conflicts of multiple-role players either disappear altogether or else they must be assigned to other causative factors.

The linking of the scarcity approach with theories of multiple roles is best seen in three highly influential works: Goode (1960), Slater (1963) and Coser (1974). All three have proposed rather general theories, which others have often borrowed to theoretically ground a wide range of more specific empirical researchers.

Coser states in the first sentence of his recent book that

organized groups are always faced with the problem of how best to harness human energies to their purposes. . . . Competition for loyalty and commitment is a perennial problem because these are scarce resources. Not only do human beings possess only finite libidinal energies for cathecting social objects, but their resources of time are similarly limited. As a consequence, various groups having a claim on individual's

energies and time compete with one another in the effort to draw as much as they can, within normative limits, from the available pool of resources. The struggle over their allocation is as much a root fact of social life as is the competition of users of scarce resources in economic affairs. (Coser, 1974:1)

On this scarcity premise, Coser (1974:3) goes on to applaud "modern non-totalitarian societies," in which "people are expected to play many roles on many stages, thus parcelling out their available energies so that they can play many games." On the other hand, radical and millenarian social organizations come under attack because they are "greedy"; they demand *all* of the individual's time, energy and allegiance and thus tend to "devour the whole man" (Coser, 1974:18).

Goode is likewise concerned with the impact of all the claims made on multiple-role players. Unlike Coser, however, who argues that one's obligations in the modern, non-totalitarian world are underdemanding (relatively speaking), Goode (1960:485) argues that "the individual's total role obligations are *overdemanding*. . . . Role strain—difficulty in meeting given role demands—is therefore normal" (my emphasis). The individual's problem, therefore, "is how to allocate his energies and skills so as to reduce role strain to some bearable proportions." It is "the same problem," Goode (1960:487) adds, that "he faces in his economic life: he has limited resources to be allocated among alternative ends." And he solves this "problem" either by avoiding certain roles altogether or by making a series of "role bargains" in which his interest is to "demand as much as he can and perform as little" (Goode, 1960:495).

Slater, by a much different route arrives at a position similar to Goode's. His explicit starting point is a consideration of Freud's claim that civilization and lovers are antagonists who must compete for the individual's limited energy. In Slater's hands, Freud's thesis becomes generalized. The concept of civilization is replaced by the more generic "libidinal diffusion," which is *any* process of spreading out one's energy among a number of objects for a common purpose. And

Freud's concept of "lovers" gets replaced by "libidinal contraction," which is any process of restricting one's energy to a very narrow range of objects. Like Freud, Slater assumes an "inverse relationship between dyadic cathexis and societal cathexis: . . . If we assume a finite quantity of libido in every individual, then it follows that the greater the emotional involvement in the dyad, the greater will be the cathectic withdrawal from other objects" (Slater, 1963:343).

With Slater, then, one is led back to Goode's conclusion that the individual's total role obligations are overdemanding. People do not have enough energy for everyone, so some compromises must be made. Thus, for example, intense dyadic intimacy loses out to the interests of diffusion. "The [diffusion-oriented] marriage ritual then becomes a series of mechanisms for pulling the dyad apart." And later, "the marital partners are to a considerable extent drawn apart by their participation in same-sex groups in the community, particularly in the occupational sphere" (Slater, 1963:353, 357).

The Scarcity Approach: A Review of Some Empirical Literature

We have seen that the scarcity approach to human energy undergirds a number of rather general theories of multiple roles. These theories help set the stage for a sociologized version of the Hobbesian war of all against all: Each group in an individual's set of affiliations becomes the potential enemy of every other group, all warring for the individual's scarce time and energy. Role strain and role conflicts are thus inevitable and unavoidable, whether they are seen (with Goode) as difficult to manage or (with Coser) as not so difficult. Let us follow this scarcity perspective into a number of specific empirical researches dealing with multiple roles and see how it conditions the analysis.

Many studies of communes appear to be grounded in the scarcity approach. Kanter (1972:70), who summarizes much of this literature and who acknowledges both Slater and Coser, concludes that in stable utopian communes, "the person must in-

vest himself in the community rather than elsewhere and commit his resources and energy there, removing them from wherever else they may be invested." Citing Slater, she argues that stable nineteenth-century communes thwarted exclusive two-person sexual bonds and even close friendships among their members because these "represent competition for members' emotional energy and loyalty. The cement of solidarity must extend throughout the group" (Kanter, 1972:86). And again citing Slater, she notes a suppression of the nuclear family, since this institution can likewise threaten communal cohesion "by draining off emotional energy" (Kanter, 1972:89; for similar conclusions about communes, see Talmon, 1964; 1965; Zablocki, 1971:117, 225, 267; Coser, 1974:ch. 9).

There is apparent evidence that peer group solidarity is a consumer of people's energy both inside and outside the context of utopian communities. Slater (1963:349) claims that "strong opposition to dyadic intimacy is often found in youth groups which are formed on the basis of common interests. . . ." Bott (1971:279) sees a conflict between sex-graded peer groups and marital dyads, a conflict based on the "crude psychological assumption" that "there is a 'limited fund of sociability.' "

Nelson (1966) is one of many social scientists who has tested Bott's thesis. Nelson's data suggest that cliques "be considered as structural alternatives to the companionate family. It follows from the idea of a constant 'lump' or 'fund of sociability' that wives participating in cliques are less likely to make companionate demands on their husbands" (Nelson, 1966:668). When a wife with a companionate orientation to marriage *also* has frequent clique contacts, she "is confronted with competing and somewhat overburdening demands for her finite fund" (Nelson, 1966:670), and this claim also is apparently supported by Nelson's data.

Rainwater (1964), in discussing four "cultures of poverty," likewise sees a pattern of highly solitary, sex-graded networks and a rigid segregation of marital roles. His data point to a "low value placed on mutuality in sexual relations"

(Rainwater, 1964:463), a pattern which he sees as an extension of a low degree of marital intimacy generally. Rainwater (1964:464) adds that "it is possible that in such a system, a high degree of intimacy in the marital relationships would be antagonistic to the system since it might conflict with the demands of others in ego's network."

There is a wealth of empirical work dealing with the conflicts between occupational roles on one hand and familial and marital roles on the other, and this work is often grounded in a scarcity approach to human energy. Edgell (1970) concerns himself with "spiralist" professionals—people who combine a hierarchically-ordered career with residential mobility. Reviewing some of the empirical literature in this area, he summarizes as follows: "the spiralist at the child-rearing stage of the family cycle is faced with a dilemma: he can be 'married' to his work or his home and family" (Edgell, 1970:319). He adds that "the spiralist who attempts to subscribe to both influences" will be the victim of role conflict, "since any degree of commitment to one role will detract from his commitment, and chances of success, in the other, simply in terms of the availability of time and energy" (Edgell, 1970:320).

Cuber and Harroff's (1965: ch. 3) intensive interview data revealed an overabundance of "utilitarian" rather than "intrinsic" marriages. There were many more instances of "conflict-habituated," "devitalized" and "passive-congenial" marital relationships than there were "vital" or "total." These data seem to support the scarcity interpretation, since all the respondents were civilization-builders who should not have enough energy left over for intimacy and vital sexuality with their spouses (for similar conclusions about occupations and families that draw, at least implicitly, on the scarcity approach, see Dizard, 1968:55; Ridley, 1973:236; Rapoport and Rapoport, 1971:286–7).

The Scarcity Approach: Some Empirical Difficulties

The foregoing review of some theoretical and empirical literature necessarily has

been illustrative. My aim has been to show that any analysis of multiple roles can prompt the invocation of the scarcity perspective as soon as the analysis turns up even the *potential* for role conflicts or overload.

What happens when the analysis turns up some multiple-role players who do *not* appear to be struggling with role conflicts or suffering from role strain or overload? In many of the above-noted studies, while considerable empirical evidence is offered that multiple-role players tend to run out of time and energy, there is also evidence of a minority of each sample who do *not* seem to be experiencing the effects of scarce personal resources.

Thus, Nelson's (1966:668) data showed that 33% of the women with frequent clique contacts also made "companionate" rather than "traditional" demands on their husbands. Were they deliberately taxing their energy resources? And 35% of the women with both frequent clique contacts and companionate orientations to marriage were not apparently "overburdened" by their multiple roles and they did *not* express dissatisfaction with their husbands' understanding (Nelson 1966: 670).

Rainwater's (1964:461) data revealed that 20% of the "lower-lower"-class women were apparently *not* fully drained by their intensive kin and peer networks, as they made "very positive" statements about their enjoyment of sexuality with their husbands.

Dizard's (1968) study turned up minority instances in which high increases in the husband's income (allegedly reflecting increased occupational involvements) coincided with *increases* in the level of marriage adjustment, as reported by both husbands and wives, and there were many instances of such increases on every indicator of the marital adjustment scale used in that study (Dizard, 1968:55).

Cuber and Harroff's (1965) "vitality" married and "totally" married middle-aged couples also need to be explained. Like the couples with "utilitarian" marriages, they had extremely demanding careers, and yet they also turned out to be devoted companions and good and virile lovers. How are we to explain their appar-

ent abundance of energy, minority though they might be?

In short, if energy is seemingly abundant for some people, then we can no longer appeal to some universal human condition or natural fact to account for those instances in which it is found to be scarce. Therefore, we will need a more comprehensive theory than the naturalistic scarcity approach to explain why some people get into overload problems with multiple roles, while other people in the same settings do not.

These considerations suggested a sharper focus on how human energy functions generally. Both the spending and the drain metaphors imply that social activities always cost us a net loss of energy. To determine whether this is true we would have to know what this energy *is* that's being drained or spent, and what its source is. Unfortunately, sociologists seem to go no further than to qualify energy as psychic, as libidinal, or as emotional. Lowen (1971:18) argues convincingly that "if we are to avoid becoming mystical, we must regard the concept of energy as a physical phenomenon, that is, capable of being measured. We must also follow the physical law that all energy is interchangeable. . . . We work with the hypothesis that there is one fundamental energy in the human body whether it manifests itself in psychic phenomena or in somatic motion."

We might turn to physiology for some clarification. In living human cells, there is a universal energy molecule called adenosine triphosphate or ATP, which is converted from glucose. When muscle fibers contract (as they do in both physical and mental activity), they consume ATP. This consumption results in the conversion of more glucose into ATP, either from the glycogen reserves stored in the liver or, if the body is under greater stress, from body fat. The conversion process is facilitated by the release of adrenaline from the adrenal glands, but adrenaline release is best facilitated by muscle contractions and by a decrease in blood sugar owing to the using up of glucose. The circle is complete: the body stimulates the *production* of ATP from glucose only through the *consumption* of ATP in ac-

tivity; hence the process of production of human energy is inseparably a *part* of the consumption of energy. Activity is thus necessary to stabilize the production of human energy, and even while we are spending it we are also converting more of it for later use. (For a useful summary of this process, see Smith and Smith, 1973:39, 173-4, 235-6.)

It follows that the drain and spending perspectives on human activity are not supported by the facts of physiology, except when food intake is inadequate¹ or when the level of stress or exertion from activity is so great that not only the glycogen reserves are being exhausted but the bodily fat reserves are being used up faster than they can be converted. *Under all other conditions of daily activity, the energy potential of the body at any given moment is physiologically abundant rather than scarce.*

The Expansion Approach to Human Energy

Durkheim provided a sociological approach to human beings that could take note of the energy-producing element of social activity. "Life is not simply a precise arrangement of the budget of the individual or social organism, the reaction with the least possible expense to the outside stimulus, the careful balance between debit and credit. To live is above all things to act, to act without counting the cost and for the pleasure of acting" (Durkheim, 1953:86). Rather than human beings having to pay a price for their social involvements, Durkheim argued again and again, they come away from them far more enriched and vitalized than they are when left to their own "resources." "Sentiments born and developed in the group," he claimed, "have a greater energy than purely individual sentiments" (Durkheim, 1953:91; see also 1965:427).

Here, then, is an energy-expansion theory of activity and group life rather than a spending or a drain theory. It sees the available supply as abundant and ex-

pansible. Also, it focuses our attention on the energy attribute as a socio-culturally conditioned *variable* rather than as a biological fact of nature, thereby enabling us to ask important questions about its social production and not simply its consumption. Goode felt that when the family is pervaded by a sympathetic atmosphere, it uses up so little of the individual's energy that he has a lot left over for his more demanding roles (see Goode, 1960:493-4). But given the expansion approach, the possibility arises that family activities may *produce* more energy, precisely because of the sympathy therein. It is not that the family's freedom from the strict ranking of performances drains us less, but that any supportive and sympathetic atmosphere gives us more. In other words, perhaps some roles may be performed without any net energy loss at all; they may even *create* energy for use in that role or in other role performances.²

The Social Construction of Human Energy

I now had two rather different approaches to human energy. I could not deny the partial validity of the scarcity approach, as its draining and spending implications were not simply a figment of sociologists' imagination (witness Holmstrom's [1972:97] claim that "over half the professional women mentioned having problems, at least some time in their careers, of not having enough energy, of fatigue, of being tired and weary in a physical sense"). But neither could I impugn the partial validity of the expansion approach, as it seemed to offer an explanation of the gaps in the findings of those guided by a scarcity approach (witness Holmstrom's interviewees who did *not* mention problems of not having enough energy). In seeking a more comprehensive theoretical reformulation, I begin with the premise implied by human physiology: in terms of the reserves of

¹ To be sure, extreme malnutrition may create a chronic energy-drain for millions of the world's population.

² I know of scarcely any sociological treatments of multiple roles that work explicitly or implicitly with the expansion approach to human energy. Exceptions are an excellent paper by Sieber (1974) and Homans (1950).

energy for activity, human beings have abundant and perpetually renewing resources. The key questions for sociological theory then become (1) under what socio-cultural *conditions* does this energy potential become freely available for given activities and (2) under what socio-cultural conditions does it become unavailable? My thesis may be stated in the following four points:

(1) Rather than energy simply flowing out mechanically in response to everyone's demands until it runs out, persons *construct* their response to the demands of others. Thus, they implicitly *decide* how to use their energy and whose demands will be honored, if anyone's.

(2) People may *withhold* the full flow of their energy into a given role, thereby freeing that energy for engaging those role performances that are the most greedily demanded and heavily sanctioned or for embracing those that are simply the most valued culturally or personally. In this way it is possible for an individual with multiple roles to manipulate them so that only one of those roles gets the lion's share of his energy (cf. Coser, 1974, who seems to argue that through some miracle of social structure, multiple roles will automatically protect a person from being "absorbed" by a single role).

(3) If activity results in a feeling of energy loss, this situation is more a function of the variable *stance* of persons toward their activity-clusters and their role partners within them, rather than a simple matter of every role performance costing us some of our scarce biological resources.

(4) In the daily long run, we have ample energy for all of our role partners regardless of the other energy "expenditures" made earlier in the day. Abundant energy is "found" for anything to which we are highly committed, and we often feel more energetic after having done it; also, we tend to "find" little energy for anything to which we are uncommitted, and doing these things leaves us feeling spent, drained, or exhausted. With this perspective, let us return to multiple roles and the issue of why it is that some multiple-role players get into overload problems while others do not.

Time, Energy, and Commitment: Some Analytical Issues

Sociologists invoking the scarcity approach often have confounded time, energy, and commitment, treating them as if they are interchangeable scarce resources. Thus Coser begins his book by writing of competing claims on people's scarce *time* and *energy*, but "greedy institutions" are defined as ones that "seek exclusive and undivided *loyalty*" (Coser, 1974:4, my emphasis). This mixing of concepts does not create difficulties when analysis focuses only on the limiting cases in which total claims are made on a person's time and energy and commitments by a single social organization. The problems emerge only when we focus on those more common instances in which several social organizations and role partners make *conflicting* demands on a person's time, energy or commitment. In these cases, what is the "greedy" claimant demanding? More time, energy, commitment, or some combination? To say, as Coser (1974:2) does, that modern occupational demands on a person allow him sufficient free "time for his family or other non-occupational associations," is to say nothing of how he *uses* that free time, how he uses his energy in it, to whom he is committed (if anyone), or whether he even uses this possible free time non-occupationally (many people do bring briefcases home with them or voluntarily remain at the office after working hours). I may spend much time in the physical presence of my family, but use my energy for non-familial purposes during that time (e.g., by reading). Furthermore, my wife may press claims only on where I locate my use of time and not on my energy ("Can't you prepare for your class at home instead of at your office?"), or she may press for both my time and energy ("Can't we do something together tonight?").

The issue of commitment complicates the analysis much further. Indeed, an important empirical issue is to what extent increasing claims on a person's time and energy are really veiled complaints about the level of commitment of that person. In other words, some claimants may rest

content with having the unconditional *commitment* of other persons even while making few or no demands on their time and energy. Sieber (1974) points out that very small, occasional inputs of time and energy, especially in the form of dramatic displays, may be enough to demonstrate continued high commitment to certain role partners, who will be satisfied with that alone. On the other hand, some people who feel deprived of a role partner's commitment may still feel dissatisfied even if they are granted more inputs of time and energy from that role partner. Operationally, it could prove difficult to sort out what people really want from what they think or say they want, especially insofar as some people who claim to want more time and energy from a role partner may be psychologically protecting themselves from the knowledge of their partner's under-commitment to them.

These examples should suffice to show that there are, at the very least, three analytical variables involved in these issues—energy, time and commitments—and that it is essential to treat each of them as distinct from the others. Failing this, we have little chance of discovering when it is that multiple roles create strain or overload problems and when they do not. Our analysis of human energy has uncovered no naturally occurring deficiencies that can explain these problems. Let us consider time and then commitment.

There are some, like Moore (1963:4–11), who argue that time is naturally finite and scarce, and this view can serve those who argue that multiple roles necessarily give rise to strain. "Even if human energy is flexible and expansible," the argument would go, "there are only so many hours in the day, and time given here is time taken away there" (see, for example, Dizard, 1968:76).

To be sure, time may be *experienced* as scarce, particularly for North Americans, but this appears to be more a function of arbitrary cultural agreements and priorities than of some naturally occurring deficiency built into the human condition. Among the St. Lawrence Island Eskimos, "neither the concept of 'spending' as a process of transaction with an external agent nor the concept of 'time' as a dissec-

tible entity which can be used as the object of such a transaction has any validity. The cultural focus of daily activity is not on 'time' as a hollow shell which is to be filled with completed tasks or accomplished relaxation. Rather, the focus is on the *tasks* themselves which must be done" (Hughes, 1961:92, italics in the original). And the "tasks themselves" seem to include a full round of what we would call "leisure activities," although "the idea of time being 'set aside' for this purpose is unfamiliar" (Hughes 1961:93). If the scarcity of time is a universal human problem, these Eskimos apparently have not yet learned about it.

Given this consideration, a modified scarcity argument about time might go as follows: When social institutions in a complex society become segregated from each other (such as modern families and work-places), *then* time becomes experienced as universally scarce, since so many of one's activities will isolate one from so many of one's role partners to whom some time is owed." Note, however, that this formulation treats the experience of scarce time not as a *natural* occurrence but as a product of arbitrary socio-cultural arrangements. *If* place of work is distant from residence, then time may, of course, become a constraining resource. But to hold this situation as evidence that the human *condition* is constrained by scarce time is surely to beg too much, for it leaves unanswered the question of what the *priorities* were that led to places of work becoming distant from residences to begin with.

Moreover, even given the work/home segregations, there is still evidence of considerable variation in the way that time is structured and experienced within that framework, and some of these variations reveal an abundance of free time (which does not necessarily mean that people use it for any of their role partners). Komarovsky (1967:340–1) says of her blue-collar families that "there is no lack of togetherness in the sense of time spent in each other's physical presence," and that "shortening of the work day, smaller families, and the withdrawal of many economic functions from the home have given these couples long evenings and weekends

together." One study of 54 female elementary-school teachers has effectively treated as a variable "the number of hours an individual spends on work-related matters outside the working time formally required by the organization" (Gechman and Wiener, 1975:521-3). Not surprisingly, those who score higher on "job-involvement" tend to be the same ones who devote more "personal time" to work. Presumably, these individuals will tend to experience time as existing in shorter supply as well. Cuber and Harroff's (1965) respondents were all "top influentials" in their prestigious careers, but those who had "intrinsic" marriages seemed to arrange their lives so as to maximize time together as a couple, as evidenced by the man who "passed up two good promotions because one of them would have required some travelling and the other would have taken evening and weekend time" (Cuber and Harroff, 1965:56).

To be sure, people often have to work at making these kinds of accommodations, given the work/home segregation. The point to be made is only that time does not *present* itself to us as a prefabricated scarcity even in the modern setting. Like energy it is flexible, waxing abundant or scarce, slow or fast, expanded or contracted, depending upon very particular socio-cultural and personal circumstances. In short, we need to see the experience of both time and energy as *outcomes* or *products* of our role bargains, rather than assuming (like Goode) that they are already constituted for us as scarcities even before our role bargains are made.

Let us now consider commitment, the factor which I view to be the decisive one in whether or not some form of strain or overload will be experienced. I shall speak of a commitment when a person wants to carry out a more or less elastic cluster of performances over time, in light of his/her assessment of the importance of that cluster of performances. By "more or less elastic cluster of performances," I wish to emphasize the idea that most commitments point to a shifting number and range of rather ill-delineated *potential* performances, rather than to a set of ironclad and

numerically constant behaviors having clearly defined parameters which everyone knows. (The exception is ritualized performances.)

There may be any number of reasons why one wants to carry out a cluster of performances, why one thinks it is important to do so. Provisionally, I suggest four principal reasons, or elements of commitment: (1) spontaneous enjoyment of one or more of the specific performances in the cluster; (2) spontaneous loyalty to one or more role partners who want me to do it, or who will be pleased if I do it; (3) anticipation of some perceived reward, such as wealth, power, prestige, sympathy, approval, favorable self-image, etc.; (4) avoidance of perceived punishment, such as stigma, disapproval, status-loss, loss of rewards, etc.

Any one of these elements alone may generate commitment, although sheer enjoyment must surely be the quickest way to do so. At times the four elements may overlap, but at other times commitment may be maintained by virtue of only one of the elements being strongly present, even when the values of the other three elements would, by themselves, make for *non*-commitment. When all four elements independently and strongly support a given cluster of performances, the commitment level will be extremely high.

I suggest that commitments as here defined can be expanded indefinitely. This hardly implies that we can commit ourselves meaningfully to everything we can think of. It simply means that there is no inherent limit on the expansion of our commitment levels *within the scope of our own ongoing activities and the role partners we encounter within them*. Commitments are not governed by any natural scarcity principle that has yet been discovered outside the imagination of sociologists, and I postulate that increases in the entire range of our commitments are always possible. Whether any commitment *will* expand is a function of (1) any increase in value of one or more of the four elements of commitment or (2) any increase in the number of performances (within the cluster) that is assessed to be important. Further, any such increase would stimulate (1) increases in the sub-

jective energy level experienced during and after every interaction and (2) increases in the satisfaction with our role performances of every one of our role partners. Moreover, (3) the use of time will be found to be flexible enough to facilitate the above two changes, even to the point of accommodating additional units of time allowances to every one of our role partners. This last accommodation is always possible because of the creative strategies that human beings may adopt to "make time"—for example, compressing the time for certain activities through compactness or greater efficiency, doing two or more things at once, etc.

Systems of Commitments and Limits on the Expansion of Time and Energy

Without assuming an original condition of scarcity, how is it possible for overload (in the form of scarce energy or time) to occur as a regular phenomenon of human experience at all (disregarding those instances in which it occurs through situational crisis or urgency)? To say that high commitments expand energy and time while uncommitments contract these resources may be a good starting explanation, but it is insufficient. Many people are very highly committed to their jobs and devote enormous amounts of time and energy to them, but still feel they have more to do than they have time or energy for, as numerous studies (principally from the University of Michigan's Institute for Social Research) have shown (for a convenient summary, see French and Caplan, 1972). At the very least, these studies show that *sometimes* there are limits to how much energy and time can be expanded even by very high commitments.

To determine the nature of these limits, we first need to study the individual's total range of commitments as a system. That is, how do individuals assess the value or importance of a given role or activity-cluster in *relation* to their other roles and activity-clusters.³ Ideal-typically, the shape of this commitment system may

take three forms: (Type I) a system of equally positive commitments, in which anything one typically does is seen as just as good (worthy, important) as anything else one typically does; (Type II) a system of equally negative commitments, in which anything one typically does is seen as just as bad as anything else one typically does; and (Type III) a system of over- and under-commitments, in which one or more typical activities and roles are seen as better, more important, more worthy of one's efforts, etc., than any of one's other typical activity-clusters and roles. Note that a commitment in a Type I system and an over-commitment in a Type III system both have positive valences, and that the difference between them can be determined only by knowing the shape of the whole system of commitments to which they belong. Note also that in a Type III system, we do not see over- and under-commitments as positive and negative, respectively. Rather, *both* the over- and under-commitments occur on the plus side of a person's commitments. Both have positive valences, but the over-commitments have *higher* valences than the under-commitments.

It should now be apparent that Becker's (1960) conception of commitment is unacceptable for the present analysis. Becker sees commitment to entail the making of a series of "side bets" on a given line of activity, such that the expense of abandoning that line of activity would be too great. One has staked one or more originally extraneous interests on the continued performance of a given activity. Becker thus assumes a Type III system, because people's willingness to stake many of their extraneous valuables on a single line of activity implies that their *other* lines of activity (on which extraneous valuables have been staked) are seen as relatively expendable. Becker's "commitment," then, implies a system of over- and under-commitments, in my terms.

While individual systems of commitment may, of course, be rooted in personal idiosyncrasy, we are interested in the likelihood that such systems are *culturally* patterned for specific societies and for specific groups, classes, sexes and other social categories within societies. I

³ A very similar view has been taken recently by Gerson (1976:797-8).

suggest that in Type I cultures (whether societal or sub-societal), the phenomenon of scarce time and energy will be minimal. Equally high commitments to all of one's activity-clusters and role partners will maximize the production of energy and time for all of them.⁴ Here, what limits the expansion of energy and time for a given activity-cluster is the presence in one's total role and activity systems of other interests to which one is equally committed. And one's role partners will stand ready to release a person to his other interests, periodically because they, in turn, have their *own* outside interests which they will deem equally worthy of their energy and time. In brief, we might say that a natural "balancing act" is occurring here. Each of one's roles and activity-clusters will "swell"; each will become as elaborated as it can, within limits provided by the fact that all of one's other roles and activities will become likewise elaborated.

Type II cultures are probably limiting cases, as the absence of any positive commitments would generate diffuse apathy (see Aberle et al., 1950). Here, any expansion of energy and time is limited by the absence of any interest held to be worthy of one's efforts. The social production of energy is at a minimum. Empirically, the place to look for instances of this type would be within subcultures having a nihilistic, despairing or defeatist world view. Here, some time and energy are being produced, but only for the sake of the rejection of life, which, paradoxically, becomes a life-sustaining commitment.

Type III cultures surely constitute the most empirically common type, and the remainder of this paper will be devoted to analyzing their features. Concerning

under-commitments, the phenomenon of scarce time and energy should be very much in evidence. Here, what limits the expansion of time and energy is the omni-presence of one's over-commitments. No matter how much I might "value" my under-committed interests, my time and energy for them are always constrained by the fact that I have "better" or "more important" things to do.

Concerning over-committed interests, we should expect an enormous production of energy and time for them. Nevertheless, if some of my role partners are also over-committed to the cluster of activities in which I and they are commonly engaged, there may still be more to do than the already high production of time and energy can accommodate. Over-committed role partners will tend to expand their projects indefinitely, and since identity strength is likely to hinge much more on the success of these performances than on any others, no one involved in them is apt to release his/her role partners from doing their share of the work. Inevitably, then, over-commitments will lead to the swelling of the activity-cluster to which they are oriented; either the number of specific performances within the cluster will expand, or one or more of the performances within the cluster will become more elaborated, or both.

The tendency, then, is for over-committed interests sooner or later to begin to encroach on the time and energy being produced for one's under-committed interests.⁵ To cope with the demands of these swelling activity-clusters, over-committed people will want to contract their involvement in those activity-clusters to which they are under-committed. They may do this by decreasing the number of performances within them, by compressing them so that they require less attention and less elaboration, or by delegating others to do them. In

⁴ On the normative level, an empirical approximation of this type may be found in Zablocki's (1971) study of the Bruderhof, whose theology enjoins them to joyfully "bear witness" to the presence of the "holy spirit" in everything they do, no matter how commonplace. Zablocki (1971:31) suggests a parallel between this theology and Zen Buddhist teachings: "what they have in common is the desire to destroy the notion of a secular sphere of life. The message is that there are no activities, however trivial, that cannot be permeated by the divine spirit."

⁵ On the psychological level, over-committed interests will give rise to worry and anxiety, simply because the immensity of one's investment in these interests will lead to constant preoccupation with the performances associated with them.

turn, the *phenomenon* of scarce time and energy will become further heightened. As this happens, over-committed people will frequently need to give accounts to any role partners to whom they are under-committed.

Scarcity Accounts as Excuses

It is suggested here that when people are under-committed to a course of action or to a given role partner, they try to avoid being held accountable for their minimal or non-performances. To this end, they may be found appealing to a category of accounts called excuses, which Lyman and Scott (1970:114) define as "socially approved vocabularies for mitigating or relieving responsibilities when conduct is questioned."

My thesis in this connection is that popular appeals to the scarcity of time and especially of energy are tailor-made to serve as culturally honorable excuses for the under-committed. Scarce time may serve as an excuse for any person who is generally insulated from observability by a given role partner. ("I would have written, but I haven't had the time.") But scarce energy excuses can be advanced even failing this condition, because of the utter subjectivity and intangibility of the energy concept. That is, my energy level is a hidden entity, and even those role partners who can observe me full-time may have difficulty challenging my claim to a low level, so long as I am careful to refrain from any sign of vigor in my engagement of alternative activities.⁶

Scarce energy and time excuses are neither appealed to nor honored at random. Those who offer them and those who honor them do so by referring to very particular cultural priorities and standards of adequate role performance. I hypothesize that the cultural honorability of

these excuses will vary inversely with the level of cultural evaluation and rewarding of the activity-cluster within which the excuse is made. In the United States middle and upper classes, where occupational work is more highly valued and rewarded than any other activity-cluster, scarce time and energy excuses typically will not be honored within work activities, but they often will be honored outside the occupational arena. It is still probably true that a man who returns home from the "hard day of work" has the *right* to be too tired for further activities to which he is under-committed, unless his wife is a feminist who wants to modify the typical cultural agreements.

When culturally-favored roles (such as work) are themselves differentiated into a hierarchy of presumed importance, those people holding the highest positions will have the least latitude for scarce time and energy excuses within those positions (nor would they want to appeal to them), but they will also have enormous power of appeal to these excuses *outside* of those positions. A doctor cannot be too tired to attend to an emergency, but he probably can be too tired for a great variety of marital and family activities. On the other hand, for the traditional housewife, whose daily activities are not even reckoned culturally as real work, there is little power of appeal to any excuses.⁷

Sociological Ideology and Theories of Multiple Roles

When Coser, Slater, Goode and others claim that human energy, time, and even commitments are rooted in some inherent human condition of scarcity, they provide some unintended theoretical support for these scarcities as excuses. Insofar as they state this scarcity situation as fact without going through the usual procedures for garnering evidence, we must conclude that they are giving us ideology. But what ideology is it and why have they cast their lot with that side?

⁶ No duplicity need necessarily be charged to the layperson's appeal to these excuses. On the contrary, it is likely that under-commitment to an activity or role partner does indeed result in rapidly decelerating feelings of energy, even in the mere anticipation of that activity. Many of my under-committed students report that when they sit down to study, they not only feel little energy but often feel suddenly overwhelmed by the need to sleep.

⁷ As the Cosers (1974:92) point out, "power depends on resources, and women who do not have occupational resources are in a very poor position to share it equally with their husbands."

I shall turn once more to the work of Goode and Coser, as these sociologists have taken some pains to elaborate the postulates of their multiple-role theories. When Goode (1960:490) says that "some sort of overall value hierarchy seems to be accepted in every society," he apparently is postulating the universality of invidious distinctions between activity-clusters and between types of role partners, and, hence, the ubiquity of under- and over-commitments. At the same time, he recognizes that a given individual's role decisions will be greatly constrained by this "overall value hierarchy." This means, in effect, that individuals will be under pressure to direct their flow of performances to the more highly valued activities as their first priority—to those activities in which their performances are finely graded or ranked. They will be aided in this priority by the presence in their role system of "unranked or grossly ranked performance statuses" (such as family roles), which "permit the individual to give a higher proportion of his energy to the ranked performance statuses" (Goode, 1960:494). Clearly, as Goode (1960:493) points out, in Western society "it is mainly the occupational statuses" that are the most finely graded. Note that this means that, for Goode, the inevitable scarcity that he thinks plagues people's role resources will not tend to impinge on their performances and force them to make compromises until *after* the occupational roles have already been performed! If, indeed, it is "quite possible that what gets done is not enough," this possibility will surely actualize last in occupational activities. Thus Goode's theory postulates as normal the Western tendency to over-perform in occupations and under-perform in all other roles. If it is not for him inevitable that we choose occupational activities for our over-commitments, it is inevitable that we over-commit to something, since all societies make *some* invidious distinctions between activities. Hence, it might just as well be occupational activities as any others. In principle, then, Goode is ideologically on the side of anyone who, having over-committed to one set of activities, must struggle (more or less un-

successfully) to meet the "overdemanding" claims of all those other role partners left over in his total role system. In practice, however, since Goode is really talking about Western society, he has cast his lot with those who over-commit to their occupational roles and must then struggle with the consequences.

Like Goode, Coser is concerned with the individual's total role system, but we saw earlier that for Coser (1974:3), it is only "greedy institutions" that make one's total role system overdemanding; in contrast, modern non-totalitarian societies enable people to "parcel out their available energies so that they can play many games."

At first glance, it appears that the ideological intent of Coser's analysis is simply to bolster Western pluralism and individualism. He is on the side of those who, unencumbered by any monolithic greedy organization or role partner, can weave their way through a diversity of roles, making only partial commitments to each of their role partners, of whom no one is able fully to absorb them and drain their scarce time and energy. This ideological focus is reasonably secure when Coser centers his search for greediness outside contemporary Western societies. But Coser wanted to extend the scope of his theory, and he thus came to focus his search for greedy institutions *within* modern Western societies, which he earlier had exempted from the tendency toward greed. Accordingly, he turns (collaboratively with Rose Coser) to the modern nuclear family in terms of its greedy claims on the housewife. We learn that it is not the husband but the wife who "is expected to devote most of her time, as well as her emotional energies, to their family" (Coser, 1974:89). The ensuing analysis then focuses on many of the inequities that so many modern feminists have already brought to our attention. The status of the woman is determined by her husband (Coser, 1974:90). She tends to be excluded from the most prestigious occupational positions and, even when she gains entrance, she is still expected to shoulder most of the burdens of house-keeping and childrearing (Coser, 1974:90-2) which are not even reckoned as

"real work." And "it is she who bears the major burdens of residential uprooting and social dislocation" (Coser, 1974:97), occasioned by her husband's career mobility.

The Cosers treat the "greedy family" as a mere anomaly of modern life that will soon disappear. They "foresee a new family constellation in which both he and she are the family's providers, co-managers of the household and true partners in the care of their children" (Coser, 1974:100). What they fail to realize is that the greedy family for the woman is just the underside of the same coin whose top side is the greedy occupation for the man. They themselves claim that in American society, occupational status determines one's position in the stratification system and also one's power in the family, but they make no normative judgments about this situation; rather, they want the woman to increase her power by scaling up *her* occupational involvement to bring it up to par with the man's. But how could the woman scale up her occupational involvement without the man scaling down his own, since (in the Cosers' own analysis) it was the man's over-commitment to his occupation and his under-commitment to family activities that resulted in the family having a greedy hold on the woman to begin with? And how many men with prestigious positions will be motivated to scale down their investment in work, as long as one's position in the stratification system is determined by it alone, and family activities are defined as necessary "dirty work" and a waste of time?

Ideologically, then, Coser is on the same side as Goode; he has cast his lot with men who are over-committed to their occupational roles, and he holds up this over-commitment to women as well. While he argues that people with multiple roles can "navigate" among their "various obligations" (Coser, 1974:4), he knows that in practice Western people are first and foremost oriented to work; *after* work they struggle to navigate among all their remaining roles. Since in the process they often tend to experience a deficiency of time and energy, we are then given a

theory of *naturally* occurring scarcity to account for the deficiency.

The Sociology of Scarcity Approaches to Multiple Roles

Why do sociologists such as Goode and Coser offer us multiple-role theories that ideologically promote the over-commitment to occupational roles? To answer, we need only recall the one conclusion agreed upon by everyone who has studied the relationship between work and family life: strain, overload, and role conflict tend to be greatest for those who are most involved in, and oriented to success at work. Moreover, there is agreement that the class of professionals most often satisfies this condition of being oriented to occupational success. Sociologists are themselves professionals, and those who frequently publish and have widespread reputations are likely to rank highest on any scale of job involvement, orientation to success at work, and the like. Therefore, when these sociologists theorize about multiple roles it would be quite curious to find them somehow immune to their own personal realities, which have taught them that time and energy for pursuing interests other than their careers are perpetually scarce. In short, their ideological agenda is to pay homage to the occupational role because this has been found to be the greatest source of rewards in their own experience. Their unwitting support for scarce time and energy excuses, implicit in their appeals to a biological condition of scarce human energy, is hardly an invitation for people to be irresponsible. It is again an expression of their own personal realities, of the fact that time and energy do become experienced as scarce whenever people become over-committed to some of their activities and under-committed to others.

Note that the ideological support for occupations is also a reaffirmation of the existing system of social stratification, which, as the Cosers remind us, is *based* on the occupational division of labor. When the Cosers invite women to increase their occupational involvements, they are not proposing that women just go

out and get any job. Rather, they mean that they should go out and get enjoyable jobs—jobs that are highly prestigious and rewarding—jobs that rank high in the system of social stratification and that are, accordingly, scarce. Goode's support for the stratification system is more indirect but, by accepting the inevitability of a value *hierarchy*, he winds up supporting the system of stratification in the same implicit way that the functional theorists of stratification do.

Summary

In this paper I have argued that human resources of energy and time are flexible. They expand and contract, depending on very particular systems of commitment that determine their availability. I have suggested that scarce energy and time as *phenomena* (along with theories of role strain that ground these scarcities in some human condition) are specific to only one such system of commitments—a Type III system of over- and under-commitments. We need to know much more about all three types of commitment systems. What, for example, are the structural circumstances that foster the making of over-commitments? Presumably, any severe crisis situation will generate an over-commitment to resolve it, but extreme poverty may force people *chronically* to over-commit their resources to the activity of getting food and just "getting by." It may be that a modicum of affluence is a necessary (though not sufficient) condition for a Type I system of equally high commitments, thus suggesting that differences in individual commitment systems may have a socially stratified dimension. On the other hand, insofar as systems of social stratification are based on the notion that some activity-clusters are more important than others, it may be that Type I systems could only be typical in relatively strata-free societies. The answers to such queries must await further study.

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CITY DIFFERENCES AND NONDIFFERENCES IN THE EFFECT OF RACE AND SEX ON OCCUPATIONAL DISTRIBUTION*

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The empirical and theoretical analysis of intercity variation in black-white occupational differences constitutes a thin but persistent thread running through the last 30 years of sociological literature. At least nine separate statistical analyses have been reported in major sociological journals from 1951 to 1976, starting with Turner's (1951) attempt to test "some hypotheses frequently found in the race relations literature" concerning the relative occupational position of the Negro male in the labor forces of large American cities. This paper reassesses this entire literature on theoretical, methodological and empirical grounds. Our strategy here is to ask some key, underlying questions which never have been considered by earlier empirical studies of city differences in the race-occupation relationship. We find unexpected answers to these questions and these answers lead us to reexamine the dominant methodology of previous studies of the relative occupational standing of blacks and whites in U.S. metropolitan areas. In our methodological reexamination of earlier studies, we ask some additional basic, underlying questions which have not been considered in previous research, and the answers we find enable us to explain exactly how certain methodological conventions in this literature misled earlier researchers. As a further validation of our methodological arguments, and for theoretical reasons developed in this paper, we also investigate intermetropolitan variation in gender differences in occupational distribution. Finally, we draw upon recent theoretical and empirical research on the social evaluation of occupations to derive a theoretical explanation of why sexual differentiation and racial differentiation in the occupational structure does not vary substantially across U.S. metropolitan areas.

Research and Theory on Intercity Variation in Black-White Occupational Differences

Statistical study of intercity variation on occupational differences between white and black men starts with Turner's (1951) efforts to apply data from the 1940 U.S. Census of Population to some then-current ideas about race relations. Turner makes no specific citations of the theoretical literature which motivates his efforts, but we may reasonably infer from later attempts to extend his work that it is essentially the same body of writing that Williams (1947) synthesizes in his classic

volume, *The Reduction of Intergroup Tensions*. In that work, Williams suggests that hostility toward minority groups varies directly with (a) the rate at which the minority population increases in size relative to the non-minority population and (b) the ratio of the incoming population to the resident population. This hypothesis links the demography of the black population to the psychology of white Negrophobia and racial discrimination.

In order to investigate this and other hypotheses, Turner first constructed an "index of racial occupational equality" in each of 90 cities which had populations of 100,000 or more in 1940; he then measured the covariation between this index and certain other attributes of cities. Turner's findings were not entirely consistent with Williams' propositions, but by 1954, Allport had elevated Williams' remarks to the level of "a sociocultural law" (as

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quoted by Blalock, 1956:227). Using methods similar to Turner's on 1950 Census data from the South and, separately, the non-South. Blalock (1956; 1957) produced findings which gave only ambiguous support, at most, to Williams' proposition. Glenn (1963; 1964) developed socioeconomic scores (SES) for major occupational groups and examined the relationship between black and white occupational SES in Standard Metropolitan Areas (SMAs) in 1950. For the non-South only, he finds a moderate negative correlation between the percentage of an SMA which is black and the SMA's ratio of black occupational status to white occupational status. He argues that where the percentage of blacks is large, blacks "overflow" into good occupations, thereby masking the effects of discrimination on racial differences in occupational attainment. Glenn (1963; 1964) also finds a positive correlation between white occupational status and the percent of the population which is black, leading him to argue that the white ability to exploit blacks varies directly with the percentage of the population which is black. Similarly, Cutright (1965) attempts to test the hypothesis that whites benefit from the presence of blacks in the labor force and presents correlations of about .98 between his index of white occupational benefit and the proportion of the experienced civilian labor force which is black in SMAs in 1950. Cutright reports similar results for 1960, and he finds no substantial differences when his analysis is restricted to the South or non-South, or to places with small or large black populations. Bahr and Gibbs (1967) make much of the high correlations they find between two measures of racial inequality and the index of dissimilarity (Delta) between the white and black male occupational distributions. Jiobu and Marshall (1971) estimate a path model which explains 80 percent of the variance in the index of dissimilarity between the white and nonwhite occupational distributions in 74 large urban places. Jiobu and Marshall's analysis is essentially an ecological and demographic analysis which stresses the role of a city's geographic location, black population growth, residential ghettoization, and industrial

composition in producing occupational differentiation between the races. Finally, Spilerman and Miller (1976) follow Spilerman's (1968) dissertation to develop a path model explaining Turner's index and several variations of it. Spilerman and Miller utilize 1960 U.S. Census Bureau data, and they stress the theoretical import of Turner's (1951) observation (confirmed by their analysis) that the Turner index is correlated with the proportion of a city's work force which is employed in manufacturing. Spilerman and Miller infer from this result that the pattern of ethno-racial organization in a city is affected by the structure of opportunities for socioeconomic attainment which are available to its residents.

If these studies of the city-race-occupation relationship tell us anything at all, they offer two basic conclusions. First, for about three decades, the sociological literature has indicated that *there is substantial intercity variation in the race-occupation relationship*. The assumed large size of city differences in this relationship has served as an implicit justification for testing hypotheses about, and building models of the relationship between a city's degree of racial occupational differentiation and its other characteristics. Second, one can conclude from past literature that *models and hypotheses which have been constructed to explain intercity variation in racial occupational differentials have been believed to have important implications for a wide variety of theoretical concerns, including (a) the social psychology of discrimination, (b) the relationship between the demography of the labor force and the structure of occupations and (c) the interrelations between industrial, occupational and ethno-racial patterns of social organization in contemporary America*. We now attack both of these conclusions on empirical, methodological and theoretical grounds. In a word, we find that variation in the race-occupation relationship between large metropolitan areas is trivial. We draw a number of general methodological inferences on the basis of our findings. We also search for intermetropolitan variation in the sex-occupation relationship. Consistent with our findings pertaining to

race, we find only trivial city differences in the sex-occupation relationship. However, we are able to apply some of the methodology which has been utilized to investigate intercity variation in the race-occupation relationship to "investigate" intercity variation in the sex-occupation association. This procedure provides us with an "interesting" but quite misleading causal model which is consistent with past findings pertaining to racial occupational differentials; thus, our analysis of sex differentials provides additional validation of some of our criticisms of past research on city differences in racial occupational differentials. Finally, we ask why urban areas should *not* be substantially different from each other with respect to racial and sexual occupational differentials. Our answer is theoretical and speculative, but it draws on a substantial body of recent empirical work on the social evaluation of occupations.

Measuring City Variation in the Race-Occupation Relationship

To assert that there is intercity variation in racial differences in occupational distribution is equivalent to asserting that the relationship between race and occupation varies across cities. Statistically speaking, that statement is an assertion that if one cross-tabulated workers' race by their occupation by their city of residence, one would observe a three-way interaction among these variables. If this race-city-occupation interaction were substantively insignificant in size, then it would be reasonable to conclude that intercity variation in occupational differences between the races is not sufficiently important to justify sustained empirical analysis of theoretical explanation. Thus, our present task is to measure the size of the city-race-occupation interaction.

To measure the size of the three-way interaction between city, occupation and race, we utilize published data from the 1970 U.S. Census of Population pertaining to the racial composition of the labor force in each of the 125 standard metropolitan statistical areas (SMSAs) which had a population of 250,000 or more persons at

the time of the census.¹ In the analysis immediately following, we utilize the Census Bureau's 12-group major occupation classification to represent the occupational distribution in each SMSA, and we use data on two racial groups only—blacks and whites. These data are arranged in a cross-tabulation of SMSA by race by occupation. To facilitate comparison of our findings to existing analyses by other researchers, our examination of racial data pertains only to males, and we perform separate analyses for the South and non-South.

In order to measure the size of the three-way interaction between SMSA, occupation and race, we apply log-linear techniques for the analysis of cross-classifications developed recently by Goodman and others (see Bishop et al., 1975). We use the widely available Goodman-Fay computer program, ECTA. These methods allow one to fit various models to a cross-tabulation and to generate measures of how well the model fits the observed data. A model in this context is merely a statement of which possible relationships among the cross-tabulated variables are actually present in the data. If a model which does not include the triple interaction fits the data very well, then we can safely infer that city differences in the race-occupation relationship are not, on the average, large. We use two complementary methods to determine how well a log-linear model fits the observed frequencies in the table. The first method is based on the percent of cases misclassified by a model; the second method is based on chi-square statistics. To apply the first method, we fit a model to the data which allows different numbers of persons in each SMSA, different numbers of persons in each racial category, different

¹ Tabulations used in this paper are published in U.S. Bureau of the Census, 1973. Data for this tabulation were taken from the 20 percent sample of population by the Census Bureau, but published figures have been inflated to yield estimates of population values. To deflate these figures to approximate sample values, we divided them by five before analysis. Necessary data are not published for SMSAs with populations less than 250,000 persons. For convenience only, we treat the terms "SMSA" and "city" as synonymous words throughout this paper.

numbers of persons in each occupation, *and* the three pair-wise relationships between SMSA, race and occupation. Since this model does not allow an SMSA-race-occupation interaction, it forces the race-occupation relationship to be the same in all SMSAs. If the relationship varies substantially across SMSAs, the model will misclassify many cases. We have calculated that this model misclassifies only 1.1 percent of the cases in the non-South and 1.9 percent of the cases in the South—a spectacularly good fit. Calculating the percent of misclassifications separately by race, we find that 0.6 percent of the nonsouthern whites are misclassified, and 1.1 percent of whites in the South are misclassified; 6.3 percent of nonsouthern blacks are misclassified, and 6.0 percent of blacks are misclassified in the South. Thus, we find that city differences in the race-occupation relationship have only minute effects on the occupational distribution of whites and very minor effects on the occupational distribution of blacks. (The reader should bear in mind that misclassifications can be caused by the effects of variation in employment patterns, clerical errors in data handling and sampling variability—percentages misclassified give high estimates of the importance of the three-way interaction.) Indeed, if city differences in the race-occupation relationship are ignored completely, we can fill out the SMSA-race-occupation cross-classification with 99 percent accuracy in the non-South and 98 percent accuracy in the South, using only the two-way marginals of the three-variable table to guide us.

Our second method for determining the importance of the SMSA-race-occupation interaction is based on chi-square statistics. The chi-square statistic for a given model can be regarded as the amount of association which remains in the cross-tabulation *after* the model has been fitted; one can compare chi-square values for different models to measure the importance of interaction effects in much the same way that one uses the analysis of covariance to measure the importance of adding interaction effects to a regression model (see Goodman, 1972 for amplification of the analogy between his methods

and least-squares procedures for estimating variance components; Hauser et al., 1975 provide a careful empirical application of Goodman's techniques for decomposing the association in cross-tabulations). We use chi-square statistics in the following way to calculate the importance of the SMSA-race-occupation interaction. First, we fit a model which allows different numbers of persons in each city, different numbers of persons in each occupation and different numbers of persons in each racial category, but no relationships between these variables. Thus, the chi-square for this first model (CS_1) is attributable to the relationships which are *not* included in the model: the three pair-wise relationships between race, SMSA and occupation, and the triple interaction between these three variables. In the second model, we allow different numbers of persons in each city, different numbers of persons in each racial category, different numbers of persons in each occupation *and* the three pair-wise relationships between race, SMSA and occupation. The only relationship that is excluded from the second model is the triple interaction. Consequently, the chi-square statistic for the second model (CS_2) indicates the amount of the association which is attributable uniquely to this three-way relationship. In common language terms, the chi-square for the second model indicates the amount of association due to SMSA differences in the relationship between race and occupation.

In order to judge the size of the chi-square statistic for the triple interaction (the second model), we compare it to the chi-square statistic for the first model. This procedure allows us to express the association due to the triple interaction as a fraction of the association in the table which is due to all relationships between race, occupation and SMSA, including the three-way relationship (when comparing CS_2 to CS_1).

Table 1 gives the chi-square values when the two models are fitted to data on southern SMSAs only, and data on SMSAs on the non-South, following previous analyses. Looking at column A of Table 1, notice that in the South the chi-square for the first model is 274,809.6 and

Table 1. Chi-Square Statistics, Degrees of Freedom and Ratios of Chi-Squares for Analyses of SMSA-Race-Occupation Cross-Tabulations, for the South and the Non-South

Model	Likelihood Ratio Chi-Square Statistics	
	South A	Non-South B
1. (Race), (Occupation), (SMSA)	274,809.6 (954)	555,464.4 (1,897)
2. (Race, Occupation), (Race, SMSA), (SMSA, Occupation)	8,436.5 (451)	11,625.7 (902)
3. (Race, SMSA), (SMSA, Occupation)	165,609.2 (462)	166,286.5 (913)
Ratios of Chi-Squares		
CS ₂ /CS ₁	3.1%	2.1%
CS ₃ /CS ₁	5.1%	7.0%
Number of SMSAs	42	83

Source: Computed from U.S. Bureau of the Census, 1970 Census of Population. Detailed Characteristics, Final Report PC(1)-D series.

Notes: (a) Degrees of freedom shown in parentheses below corresponding chi-square statistics.

(b) Regional definitions are those of the Census Bureau.

(c) These models are hierarchical. Thus, models 2 and 3 specify the three one-way marginals as well as the pair-wise relationships which are indicated.

the chi-square for the second model is 8,436.5. *In proportional terms, this indicates that the triple interaction is only 3.1 percent of the association between race, occupation and SMSA in the South, and only 2.1 percent in the non-South.*

Leaving aside questions of statistical significance for the moment, it seems appropriate to ask if two or three percent is a substantial amount of the association in the race-occupation-SMSA cross-tabulation. We think that these percentages are not substantial, and we suspect that few persons would consider an equivalent amount of any measure of association worthy of sustained substantive interpretation. *So we think that the data give no reason to believe that much effort should be spent attempting to explain city differences in the race-occupation relationship.*

However, we can be more generous to the argument that there are large city differences in the race-occupation relationship if we are willing to accept the position that the race-SMSA association and the occupation-SMSA association are irrelevant to present concerns and, therefore, should not be included in the denominator of the ratio which is used to calculate the proportion of association which is at-

tributable to the three-way interaction. We can think of a good reason for *not* accepting that position,² but there is no point in arguing the matter, since we can show that even if one were to accept it, the empirical results would still support our contention that the SMSA-race-occupation interaction is small enough to be considered inconsequential.

In order to subtract the chi-square due to the association between race and SMSA and occupation, we specify a third model which allows for the observed univariate distributions of race, SMSA and occupation, and which also allows for the observed bivariate relationships between SMSA and occupation and between SMSA and race. The only relationships which are not specified by this model are the relationship between race and occupation and the SMSA-race-occupation triple interaction. Thus, the chi-square statistic for model three indicates the association which is due to the combined effects of the "main" relationship between race and oc-

² This position is analogous to arguing that in the analysis of covariance one should not allow for additive effects of the subclasses before estimating the importance of the interactions between regressors and subclasses.

cupation and the additional association which appears when the race-occupation relationship is allowed to vary across SMSAs. The chi-square statistics for this model are shown in the row of Table 1 corresponding to the third model.

Looking at the row in Table 1 corresponding to model three, notice that for the South, the combined effects of the race-occupation association and the SMSA-race-occupation interaction produce a chi-square of 165,609.2. For the non-South, it is 166,286.5. We now use these figures as a standard of comparison in evaluating the size of the chi-square attributable to the triple interaction alone. Looking at the appropriate row of Table 1, notice that the interaction amounts to 5.1 percent of the association in the South, and 7.0 percent in the non-South. Once again, it does not seem to us that these results are consistent with the belief that city differences in the race-occupation relationship are important and worthy of sustained theoretical and empirical analysis.

Before going on to gender differences in occupational distribution, two issues need brief attention. First, the SMSA-race-occupation interaction is statistically significant, in spite of its substantive unimportance. However, statistical signifi-

cance is a reflection of the extraordinary sample size (over 9.9 million data cases) rather than the strength of the relationship. Second, it is crucial to understand that we are *not* attacking the use of "city" as an independent variable on the ground that it does not explain much variance. Rather, we argue that intercity variation in the race-occupation relationship makes a poor *dependent* variable because it has so little variance to be explained.

Intercity Variation in Sex Differences in Occupational Distribution

Race is not the only basis for social differentiation in the labor market; women have also suffered from occupational discrimination, and we have utilized Census data on sex differences in occupational distributions to measure the strength of the SMSA-sex-occupation interaction in 1970. Column A of Table 2 provides the results of an analysis which is analogous to the analysis of city differences in the race-occupation relationship shown in column A of Table 1. Looking at column A of Table 2, notice that the triple interaction amounts to a trivial 2.5 percent of the association in the sex-occupation-SMSA cross-tabulation after allowing for all the observed univariate marginals of the sex,

Table 2. Chi-Square Statistics and Degrees of Freedom for Analyses of Sex-Occupation-SMSA Cross-Tabulations, 1970

Model	Likelihood Ratio Chi-Square Statistics	
	125 SMSAs and 12 Major Occupation Groups A	9 SMSAs and 125 Detailed Occupation Categories B
1. (Sex), (Occupation), (SMSA)	3,296,797.6 (2,863)	103,704.8 (2,116)
2. (Sex, Occupation), (Sex, SMSA), (Occupation, SMSA)	82,977.0 (1,364)	3,257.0 (992)
3. (Sex, Occupation), (Sex, SMSA)	2,885,919.4 (1,375)	87,617.1 (1,116)
CS ₂ /CS ₁	2.5%	3.1%
CS ₂ /CS ₃	2.9%	3.7%

Source: Computed from U.S. Bureau of the Census, 1970 Census of Population. Detailed Characteristics, Final Report PC(1)-D series.

Notes: (a) Degrees of freedom shown in parentheses below corresponding chi-square statistics.

(b) Published data on which these calculations are based are population estimates based on the 20 percent sample of population. Therefore, published frequencies were deflated by a factor of five to approximate sample values before calculations were performed.

SMSA and occupation distributions. After allowing for the univariate marginals and the sex-SMSA and occupation-SMSA relationships, the interaction accounts for 2.9 percent of the association. These findings are entirely consistent with our results concerning city differences in the race-occupation relationship.

Although all earlier analyses of SMSA differences in relationships between minority status and occupation that we know of have been based on analyses of major occupation groups only, occupations within these major groups are so heterogeneous that city differences in the sex-occupation relationship might be masked by the grossness of the major occupation categories. Ideally, one would test this possibility by using the 417-category Census detailed occupational classification instead of the 12-category major occupation group classification. But use of the detailed classification was beyond our computational resources when we undertook this research, and so we drew a stratified random sample of 125 1970 detailed occupational categories and a stratified random sample of nine SMSAs of population greater than 250,000 persons.³ We have performed another

analysis of the cross-classification of sex by these 125 occupations by the nine sampled SMSAs. The results of the analysis are shown in column B of Table 3. Notice that the results in column B are consistent with all the other analyses we have performed so far: the triple interaction amounts to only 3.1 percent of the total association after allowing for the one-way marginals.

Replicating Past Findings with 1970 Data

However convincing we have been in arguing that there are no strong city-race-occupation and city-sex-occupation interactions in the 1970 Census data that we have analyzed, it is still possible that these relationships existed prior to 1970 (when the earlier studies cited above were done). Thus, we may have merely discovered that city differences in the race-occupation and sex-occupation relationships disappeared from 1960 to 1970, rather than showing that earlier analyses were incorrect. We doubt that improvement in black occupational status was sufficiently large to make this possibility even plausible, but we consider it all the same. In order to test this possibility, we use the data from our analysis to estimate the coefficients of Spilerman and Miller's model.⁴ If we find the same results with the 1970 data that they found with the 1960 data, then we can be reasonably sure that it is not their data but their method that leads them to different conclusions than our own. Also, since Spilerman and Miller takes pains to establish the comparability of their analyses with those of earlier re-

³ The universe for the sample of occupations was the set of 485 detailed occupation titles in the 1970 U.S. Bureau of the Census expanded occupational classification system. A stratified probability sample was drawn in order to allow particular attention to be given to (a) occupations having relatively high proportions of females in their labor force, (b) professional and managerial occupations and (c) occupations having relatively high proportions of blacks in their labor force. These criteria defined three overlapping subsets of occupations and a residual set of occupation titles which were not included in the three strata:

- (1) Occupations with relatively high (20% or more) proportions of black incumbents;
- (2) Occupations with relatively high (80% or more) proportions of female incumbents;
- (3) Occupations included in the major occupation categories, Managers and Administrators, and Professional and Technical;
- (4) All occupations not included in the above.

Occupations included in more than one stratum could be selected more than once, and their sample weights were adjusted accordingly. However, sample weights were ignored in the log-linear analyses, as were strata, and no occupation appeared more than once in the analyses presented here. Thus, the sampling design merely assured that the analysis gave good "coverage" to occupations in which

minority employment (the subject of this paper) has been most problematic, while allowing all occupation titles to enter into the analysis. Sampled occupations are listed in Stolzenberg and D'Amico (1976).

The sample of SMSAs is composed of eight randomly chosen SMSAs and one self-representing unit which was included in the sample with unit probability. The eight random SMSAs are Erie, Pa.; Denver, Co.; Rockford, Ill.; Lansing, Mi.; Salt Lake City, Ut.; Springfield, Ma. (and nearby Ct.); Atlanta, Ga.; Austin, Tx. The self-representing SMSA is Baltimore, Md.

⁴ At the time this paper was written, the Spilerman-Miller article was scheduled to appear in the August issue of the ASR but publication has been delayed.

Table 3. Beta Coefficients for Replication of Spilerman-Miller Model (Shown with Corresponding Statistics from Spilerman and Miller)

Independent Variable	Dependent Variable			
	Log % of Whites in "Good" Occupations (Log of Denominator of Turner Index)		Log % of Blacks in "Good" Occupations (Log of Numerator of Turner Index)	
	S & M	S & D'A	S & M	S & D'A
V ₁ Proportion Males in Mfg.	-.099	-.1279 (.0365)	.453 [*]	.4711 (.1704)
V ₂ % SMSA Negro	.557	.3766* (.0432)	.247	.2546 (.2021)
V ₃ Log SMSA Size	-.048	.0436* (.0402)	.154	-.0196 (.1873)
V ₄ % Male ECLF in Good Jobs	.946	.9529 (.0401)	.363	.3122 (.1875)
V ₅ Median Negro Male Ed	-.042	-.0752 (.0357)	.216	.2439 (.1678)
V ₆ Median White Male Ed	.062	.0827 (.0416)	.179	-.1208 (.1939)
R ²	.96	.97490	.47	.45165
N	88	41	88	41

Source: Spilerman and Miller's coefficients from Table 2 in Spilerman and Miller (1976). Stolzenberg and D'Amico's coefficients computed from data described in text.

Note: * Difference between 1970 coefficient and 1960 coefficient is more than 1.96 times the value of the standard error of the 1970 coefficient.

searchers, demonstration of comparability between our work and Spilerman and Miller's also will help establish the comparability of our findings to earlier studies based on the Censuses of 1940 and 1950. And, finally, a detailed replication of earlier work based on Turner's index will enable us to diagnose the features of earlier work which led to conclusions other than our own.

Spilerman and Miller's empirical analysis is essentially a refinement, elaboration and synthesis of previous statistical studies of intercity variation in occupational differences between white and black men. That is, they use Turner's index (and several variants of it) to measure occupational differentiation between the races in metropolitan areas, but, unlike other users of this index, they develop a causal model similar to that of Jobu and Marshall (1971) to explain the variation in it. Turner's index is the ratio of the percent of nonwhites in "good" occupations divided by the percent of whites in "good" occupations. Turner defined good occupations as those major occupation categories which are "semi-skilled and

higher" (i.e., occupation groups other than "farm and nonfarm labor, domestic and other service work"; see Turner, 1951:524 or Spilerman and Miller, 1976, for details). The core of Spilerman and Miller's analysis is a pair of regression equations, one predicting the numerator of this index and the other predicting its denominator. Table 3 gives the coefficients of the six variables in the Spilerman-Miller model estimated from 1960 data by Spilerman and Miller, and our estimates of the coefficients of the same variables for 1970.⁵ In Table 3,

⁵ Spilerman and Miller excluded SMSAs with fewer than 3,500 nonwhites and "SMSAs in which blacks comprise less than 90 percent of the nonwhite population." We also deleted SMSAs with fewer than 3,500 nonwhites, but we mistakenly also deleted SMSAs in which blacks comprise less than 90 percent of the nonwhite male ECLF (rather than 90 percent of the nonwhite population as a whole). The effect of this difference between our exclusion criteria and Spilerman and Miller's is obviously trivial. We remind the reader that our analysis is based on SMSAs larger than 250,000 persons, while Spilerman and Miller's analysis is based on SMSAs larger than 100,000 persons. Our analysis excludes nonblack nonwhites.

notice that the coefficients for 1960 and 1970 are generally quite similar. For a more formal test of the similarity between the Spilerman and Miller findings and our own, we can assume that their coefficients are "true" and then test the null hypotheses that each of our coefficient estimates differs significantly from theirs.⁶ Coefficient estimates from our analyses which differ from Spilerman and Miller's by more than 1.96 standard errors (indicating a confidence level of five percent for a two-tailed test) are marked with an asterisk in Table 3. Looking at Table 3, notice that only 2 of the 12 coefficients differ at the five percent confidence level: there is not much difference between the results that Spilerman and Miller obtained using 1960 data and the results we obtain replicating their analysis with 1970 data. Thus, it seems very doubtful that Spilerman and Miller would have drawn different conclusions if they had used the same data base that we utilize in our log-linear analyses of contingency tables. Further, we re-did our log-linear analyses after excluding certain types of cities that Spilerman and Miller deleted from their analysis of 1960 data (see fn. 4). Results of this additional analysis are entirely consistent with all of our other findings in this paper.

Thus, our data are quite comparable with those of Spilerman and Miller and, therefore, with those used by earlier analysts. But how do we reconcile the gross differences between our conclusions and theirs? We think that the answer to this question is that the specification used in virtually all analyses of the Turner index hides the association between the components of the index, thereby confusing a straightforward set of relationships. That is, all studies that we know of which utilize the Turner index examine the hypothesized causes of the index, or the determinants of its various components,

but none considers the relationship among the components of the index itself. We have computed the correlations among the four components of the log of the Turner index.⁷ These correlations are all larger than .92. The correlations between the log size of the white male ECLF and the log of the number of white males who are in "good" occupations is .99977, indicating that the number of white men in "good" occupations in an SMSA can be determined almost exactly from the number of white males in the labor force of the SMSA. Similarly, a correlation of .99847 indicates that the number of black men in "good" occupations in an SMSA can be predicted almost exactly from the number of black males in the ECLF of the SMSA. Thus, once one knows the number of white men and the number of black men in the ECLF of an SMSA, all the components of the Turner index are also known. In plain English, this means that city differences in the race-occupation relationship are so small that one can accurately calculate the Turner index for an SMSA merely by knowing the numbers of white and black men in the SMSA's labor force. Thus, the same conclusions that we draw from our log-linear analyses of city-race-occupation cross-tabulations could have been drawn by examination of the correlations among the index components.

Some further illustration of the shortcomings of the Turner index are vividly illustrated by assuming for a moment that it is a valid indicator of minority-nonminority occupational differences. Making this assumption, we follow Turner's (1951) and Spilerman and Miller's (1976) earlier conclusions that the industrial composition of a city partially

⁶ The assumption that regression estimates are without error makes these tests generous to the hypothesis that the coefficients are different. If we assume that the standard errors for the Spilerman-Miller coefficients are the same as those of the corresponding numbers in our replication, then only the coefficient for the percent of the SMSA which is Negro (in the equation for the denominator) differs across the two years.

⁷ Spilerman and Miller examine the relationship between the index and its components, but they do not scrutinize the relationships among the components themselves. The correlations we computed are as follows: where $x_1 = \log(\text{number of white males in "good" occupations})$, $x_2 = \log(\text{number of black males in "good" occupations})$, $x_3 = \log(\text{number of white males in ECLF of SMSA})$, and $x_4 = \log(\text{number of black males in ECLF of SMSA})$, $r_{1,2} = .92518$, $r_{1,3} = .99977$, $r_{1,4} = .93126$, $r_{2,3} = .92189$, $r_{2,4} = .99847$, and $r_{3,4} = .92840$. (These correlations were computed on 1970 data on non-South SMSAs of 250,000 or more residents which meet Spilerman and Miller's criteria for inclusion in their data base.)

Table 4. An "Interesting" Analysis of City Differences in the Occupational Standing of Women Relative to Men, Using the Log of the Turner Index as the Dependent Variable *

Independent Variable ^b	Standardized Coefficient
Proportion employed in agriculture, forestry and fisheries	.149
Proportion employed in mining	-.211
Proportion employed in construction	-.192
Proportion employed in durable goods mfg.	-.988
Proportion employed in nondurable goods mfg.	-.439
Proportion employed in non-specified mfg.	.295
Proportion employed in transportation, communications and other public utilities	-.076
Proportion employed in wholesale trade	-.264
Proportion employed in retail trade	-.396
Proportion employed in finance, insurance and real estate	.091
Proportion employed in personal services	-.820
Proportion employed in entertainment and recreational services	.663
Proportion employed in professional and related services	-.263
Proportion employed in public administration	-.107
Number of persons in experienced civilian labor force	.011

- Notes: (a) Dependent variable is the natural logarithm of the ratio of the proportion of female labor force participants who are in "good" occupations divided by the proportion of male labor force participants who are in "good" occupations. "Good" occupations are those occupations which are not included in the 1970 Census major occupation categories Laborers, Service workers except private household, and Private Household workers.
- (b) Natural logarithms of all independent variables were taken before performing calculations. These independent variables are proportions of the total experienced civilian labor force that are employed in the various industry groups. Since the log of these variables is used, the number of persons in the ECLF of the city is an additive component of each of the log-ratios. The confounding effect of this common component is held constant by including the logarithm of the number of persons in the ECLF as an independent variable in the regressions.
- (c) For computational procedures and multiple correlation coefficients, see footnotes to the text.
- (d) The industry category "Industry not reported" was excluded from the list of independent variables to avoid redundancy; persons not reporting their industry were included in the number of persons in the experienced civilian labor force.

"causes" the occupational differentiation between minority and nonminority members of its labor force. We then compute the Turner index for sexual differences in occupational distribution for 125 SMSAs using 1970 Census data described earlier and Turner's original distinction between "good" and "less good" occupations. In order to estimate the effects of an SMSA's industrial composition on its occupational differentiation between the races, we then regress the log of the Turner index on the percent of its labor force in each of 14 major industrial categories (the 15th category would be redundant, making the equation impossible to estimate, and is left out of the regression). We hold city size effects constant by including in the regression the log of the size of the experienced civilian labor force (ECLF) in the SMSA. The results of the analysis are shown in

Table 4.⁸ Looking at Table 4, notice that the standardized coefficients for several industry categories are substantial, suggesting that the sex-occupation relationship varies substantially according to the industrial composition of an SMSA. However, we have already shown that the sex-occupation relationship does not vary very much across SMSAs, and so these

⁸ Following Spilerman and Miller, we calculated separate equations for the logarithms of each of the four components of the Turner index. The logs of the numerator and denominator of a ratio are additive components of the log of the ratio, making it possible to use coefficients from these equations to compute the effects presented in Table 4. The R-squared statistics for the four equations are as follows: for the equation for the log number of women in "good" occupations, .5178; for the equation for the log number of women in the ECLF, .4899; for the log number of men in "good" jobs, .14878; and for the log number of men in the ECLF, .4753.

regressions merely serve to illustrate the misleading nature of the Turner index.

By this point, the dangers of using the Turner index seem well established. But readers will recall that two earlier studies (Jiobu and Marshall, 1971; Bahr and Gibbs, 1967) utilized the well-known index of dissimilarity (Delta), rather than Turner's index to measure occupational differentiation between the races in metropolitan areas. Although these authors do not give much substantive consideration to the amount of variation in Delta across SMSAs, it is clear that the measure's standard deviation is far from negligible in both analyses.⁹ Given that Delta has been widely used in studies of racial occupational differences (see, for example, Siegel's [1965] classic paper) and in studies of city differences in residential segregation (see, for example, the benchmark study by Taeuber and Taeuber, 1969), it seems important to determine precisely how Delta could vary substantially across cities when the race-occupation relationship does not.

The reason that Delta varies while the race-occupation relationship remains stable is that Delta is sensitive to the marginals of the race-occupation cross-tabulation. To see this, imagine an uncomplicated occupational structure with only five occupations. Also imagine a simple form of racial discrimination in which there are quotas which limit the proportion of blacks in the labor force in each occupation. Further, imagine that these quotas are more restrictive in some occupations than others, that there are fewer blacks than whites in the labor force, and that job-related skills and abilities are uncorrelated with race. Assume also that the racial quotas for the five occupations are as follows (the numbers chosen here are arbitrary, and others will do as well): occupation I, 20 percent black; occupation II, 30 percent black;

occupation III, 40 percent black; occupation IV, 50 percent black; and occupation V, 60 percent black. Now imagine two cities, "A" and "B," with different occupational structures such that city "A" has 200 white incumbents of each of the five occupations, and city "B" has 50 white incumbents of occupation I, 100 white incumbents of occupation II, 200 white incumbents of occupation III, 400 white incumbents of occupation IV, and 800 white incumbents of occupation V. Clearly, the race-occupation relationship in the two cities is the same, since the racial quotas are the same. But the index of dissimilarity has a value of 15 in the first city and a value of 8.35 in city "B." Thus, Delta quite clearly depends upon *both* the marginal distribution of the occupational structure *and* racial inequality in job opportunities. Fortunately, the log-linear analyses that we use in analyzing city differences in the race-occupation relationship explicitly differentiate between effects of marginal distributions and effects of the race-occupation relationship.

Conclusions

Using log-linear methods, we have shown that the race-occupation relationship varies only trivially across U.S. metropolitan areas in 1970. We have shown that the 1970 data is comparable to data from earlier years, supporting the view that large city differences in this relationship did not exist in any of the years in which it has been analyzed. We then re-examined the methods used by earlier analysts, and we were able to show precisely how these techniques produced misleading statistical results and led to incorrect inferences about city "differences" in occupational differentiation between the races. We also were able to show that the sex-occupation relationship does not vary substantially across large metropolitan areas, and we demonstrated how application of certain methods used by other analysts would lead one to infer that such differences are real and that they are theoretically interesting.

But, in spite of the heavy methodological and empirical orientation of this

⁹ Jiobu and Marshall (1971:644) find a standard deviation of 10.58, as compared to a mean of 41.33. We calculated the mean and standard deviation from data shown in Bahr and Gibbs' Table 1: for the 33 SMSAs for which they calculated Delta, the mean is 51.51 and the standard deviation is 8.47.

paper, the major conclusions that we wish to draw are theoretical, and a return to theory seems an appropriate ending. We begin by stressing that we have *not* found that all cities are alike. Our analyses have been concerned with overall measures of city variation in racial occupational differentials, and we have found that such variation is, in general, small. But we have not found that all *pairs* of cities are identical in this respect. Dallas may be a lot better for black men than Hoboken, for example, but the average difference between the race-occupation relationships in randomly chosen pairs of cities is small. Thus, while persons with a particular interest in a specific city may reasonably consider the (possibly large) anomalies in that city's race-occupation relationship, cities do not, in general, depart from the norm sufficiently to justify the substantial effort that has been expended in understanding intercity variation in racial occupational differentials.

We also remind the reader that our analyses in this paper have been confined to SMSAs of 250,000 persons or more. Thus, we cannot positively eliminate the possibility that our conclusions do not apply to SMSAs of less than a quarter of a million inhabitants. We suspect that urban areas which are small enough to have economies dominated by a single firm or a single industry *may*, as a consequence, have an occupational structure which is marked by the peculiarities of that firm's or that industry's hiring, firing and promotional practices. If these peculiarities include unusually high or low amounts of discrimination, or if they merely create unusual racial differences in occupational distribution without being atypically discriminatory, then places dominated by a single industry or firm will also tend to manifest atypical race-occupation relationships. (Spilerman and Habib, 1976, have considered similar peculiarities in the process of socioeconomic achievement in one-industry towns in Israel.) In contrast, large metropolitan areas tend to have large numbers of employers and diverse industrial compositions, making it difficult for the distinctive practices of a single industry or firm to dominate their occupational structure.

However, we found no large differences between our estimates of Spilerman and Miller's model (based on cities of 250,000 or more) and Spilerman and Miller's estimate of the model based on cities of 100,000 or more inhabitants. This lack of differences suggests that our results would not have differed substantially if we had included cities of 100,000 to 250,000 inhabitants in our analysis. Thus, we can find no reason to believe that our findings do not apply to both medium- and large-sized SMSAs.

We stress that we have *not* found that average levels of *prejudice* are the same in every SMSA. Much of the race relations literature that motivated this paper is based on the hypothesis that the proportion of a city's population that is black and the rate of change in that proportion affect the level of Negrophobia among the city's white residents, and that this Negrophobia, in turn, affects the severity of occupational discrimination against blacks. We have not examined evidence which is relevant to the link between black demographic characteristics and Negrophobia, and so we have no reason to comment upon this relationship. But if cities do not vary in the extent to which their black and white occupational distributions are differentiated, then there can be little reason to believe that demographic or industrial differences between cities are responsible for large differences in the race-occupation relationship. Thus, it would appear that if there are large city differences in Negrophobia, these differences do not cause substantial intercity variation in occupational differentiation between the races. The reason for this, we speculate, is that notions about the propriety of black employment in various occupations are probably more deeply connected with values and norms concerning occupations than with feelings of Negrophobia. Some amplification of this speculation seems in order, and we give some brief attention to it now.

Our reasoning about the causes of geographic "nondifferences" in occupational differentiation between the sexes and races is based on the emergent body of research and theory on the social evaluation of occupations and their incumbents.

For present purposes, only three arguments in this literature are important.

First, in modern Western societies, virtually all individuals seem to be able to make judgments about the social ranking of occupations (see Siegel, 1971).

Second, these social evaluations do not vary significantly across geographic areas (Hodge et al., 1966), between social groups (Reiss et al., 1961: ch. viii) or over relatively long time periods (see Hodge et al., 1964; Deeg and Patterson, 1947; Neitz, 1935). Indeed, Siegel (1970) has provided empirical evidence that U.S. blacks accord occupations the same prestige as given by whites (he finds a zero-order correlation of $+ .9625$ between the prestige rankings given to 66 occupations by whites and the prestige rankings given to these same occupations by blacks). Further, Siegel (1970: Figures 3 and 4) also finds that blacks are virtually identical to whites in the extent to which they downgrade the prestige of an occupation on the basis of the percentage of blacks in its labor force.

Third, the social evaluation of occupations includes judgments of the extent to which incumbency in each occupation is appropriate or inappropriate for members of various identifiable social groups, and these judgments create sex and race differences in occupational distributions. For example, the force of social custom in creating sex and racial differences in occupational distribution has long been recognized even by those whose major efforts have been focused on understanding the workings of the sexless, colorless and unprejudiced invisible hand that governs economic markets (e.g., Taussig, 1928; Arrow, 1971; 1972; Becker, 1957). Indeed, Rossi (1965:1198) has argued that women themselves tend to perceive an incompatibility between employment in scientific or professional occupations and the demands of marriage and childbearing, leading great numbers of females away from these occupations and into vocations which are more easily combined with a role-set centered on home and children. Thus, it would seem that the relative absence of women in certain occupations (and their relative preponderance in others) is probably due to *both* the un-

willingness of employers to hire females for certain jobs *and* the existence of matching widespread preconceived notions by women themselves that females are not suited to employment in certain fields (for some related ideas and evidence, see also Oppenheimer, 1970; Duncan and Evers, 1975).

Fitting the above three propositions together, one would conclude that judgments of sexual and racial appropriateness for incumbency in various occupations are extremely widespread, that they create racial and sexual differentials in occupational distribution, and that these judgments are sufficiently homogeneous that the race-occupation and sex-occupation relationships do not vary much from city to city. Thus, one might speculate that research on occupational evaluations implies that intercity variation in racial differences in occupational attainment should be small or nonexistent. That is, of course, precisely what we find. And while our research is hardly a test of this implication, our findings are entirely consistent with it.

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A SOCIAL INDICATOR MODEL OF CHANGES IN THE OCCUPATIONAL STRUCTURE OF THE UNITED STATES: 1947-1974*

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This paper presents a ten-equation dynamic structural equation model that shows how aggregate changes in the occupational structure of the United States affect each other and are affected by economic, technological and institutional changes. The model postulates a recursive flow of causation from changes in sectorial (agricultural, manufacturing, services) demand and productivity to changes in the distribution of occupations by sector, bureaucratization and status level. Model equations are estimated on annual national data from 1947 to 1972 and are used to make conditional forecasts of the endogenous variables for 1973 and 1974. The equations fit the observed data well, lack demonstrable autocorrelation of disturbances, and forecast the 1973 and 1974 values with considerable accuracy. Empirically, the model facilitates the quantitative estimation of the relative effects of economic growth and technological change on expansion of the service economy, bureaucratization of jobs, and growth of high-status occupations. In particular, when combined with recent findings on the sources of intergenerational occupational mobility in twentieth-century American society by Hauser et al. (1975), the model shows how changes in the pattern of economic growth and productivity from that which has prevailed for most of the post-World War II period may imply a decline in the rates of upward occupational mobility in the absence of other counterbalancing structural changes.

As part of the general increase of interest in the development on noneconomic social indicators in recent years, there has been a corresponding rise in the number of aggregate indicators of social conditions of work in America that are available in time series form. For example, *Social Indicators: 1973* contained about a dozen annual and biennial times series of indicators of employment opportunities, labor force participation, working conditions, and quality of employment life, whereas *Social Indicators: 1976* contains roughly twice that number (Executive Office of the President, 1974; 1977). However, as has been true of most social indicator research in general, research on indicators of work and employment has concen-

trated on the accumulation and description of the indicators rather than on their analysis as components in social system models (Anderson, 1973; Land and Spilerman, 1975). In particular, in spite of the increasing availability of time series on changes in American working conditions, there exist no macro sociological models capable of explaining the causes and consequences of these social changes.

The objective of this paper is to begin to fill in this gap in the sociological literature. More precisely, we specify and estimate a ten-equation dynamic macro social indicator model that shows how aggregate changes in the occupational structure of post-World War II American society affect each other and are affected by changes in economic, technological and institutional forces. The model is dynamic, because it utilizes difference equations to explain rates of change in social indicators; it is macro, because it represents the structure of over-time interrelationships among indicators measured in the form of summary counts, averages, or rates defined on the population.

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The model is based upon a theory of how economic, technological and institutional forces determine changes in the positional distribution of occupations among categories of sector location, bureaucratization and status. Theoretical and methodological strategies for specifying the model are sketched in the next section and individual equations for indicators of changes in the occupational structure are specified in detail and estimated in subsequent sections. Since the theory specifies no significant simultaneous interdependencies among the endogenous variables, the model is recursive at time t . Estimates of the model are obtained using annual national data from 1947 to 1972 and conditional forecasts of the endogenous variables of the model are performed for 1973 and 1974. Finally, implications of the empirical performance of the model for related issues in the study of social change and social mobility are discussed.

Model Specification: Strategies and Overview

This paper applies a general framework for building dynamic macro social indicator models laid out elsewhere (Land and Felson, 1976). That framework consists of three components: (1) a demographic accounting matrix equation for relating population stocks and flows between the component social states of a social condition in successive time periods by using transition coefficients, (2) an interpretation of the demographic accounting parameters (transition coefficients or their stock counterparts) as measures of opportunities of individuals or groups to realize social benefits by changing from one social state to another and (3) an explanation of changes over time in the opportunity structure as functions of aggregate social forces as modeled in a system of difference equations. Thus, a general function for determining changes in social indicators combines autoregressive terms (representing the "dynamic" response over subsequent time periods of the social condition to changes in its environment) and structural equation terms (representing causal hypotheses about relevant environmental changes that affect the so-

cial condition). Since the details of this approach are discussed elsewhere (Land and Felson, 1976), we do not repeat them here.

To proceed with the macro parameterization of changes in work and employment conditions, it is necessary to have at least the rudiments of a theory of macro social change. Given an opportunity-structures interpretation of stock and flow social indicators, it follows that this theory should be capable of accounting for changes in the structure of opportunities. Without pretending to offer a complete theory of change, Land and Felson (1976) suggest, as a theoretical guideline for the specification of dynamic social indicator models, that changes in stocks and flows of social indicators can be accounted for in terms of those social forces that affect the set of competing alternative opportunities. This general guideline assists the formulation of substantive hypotheses incorporating standard sociological, demographic and economic notions of norms, roles, stratification, conflict, population at risk, cohort replacement, demand, supply, cost, and so forth, insofar as they explain variations in the structure of opportunities for transition. As such, the guideline is useful if it facilitates the specification of equations that perform well empirically. It also allows reformulation of other theories and findings so as to be useful for model-building. The application of this theoretical guideline is best demonstrated in specific substantive contexts.

Figure 1 gives a general flow-graph description of how we apply the Land-Felson Opportunity Structures Postulate to the explanation of changes in indicators of occupational structure. To begin, we use the term "occupational structure" to refer to the distribution of jobs by various characteristics of economic performance roles. Although many such characteristics could be distinguished, we deal herein with three classes of job attributes that are relevant to sociological theory, can be measured at the aggregate level in time series form for the United States, and show substantial variation when so measured. The three are: (1) sectorial relocation, pertaining to the transfer of jobs from one economic sector to another; (2) bu-

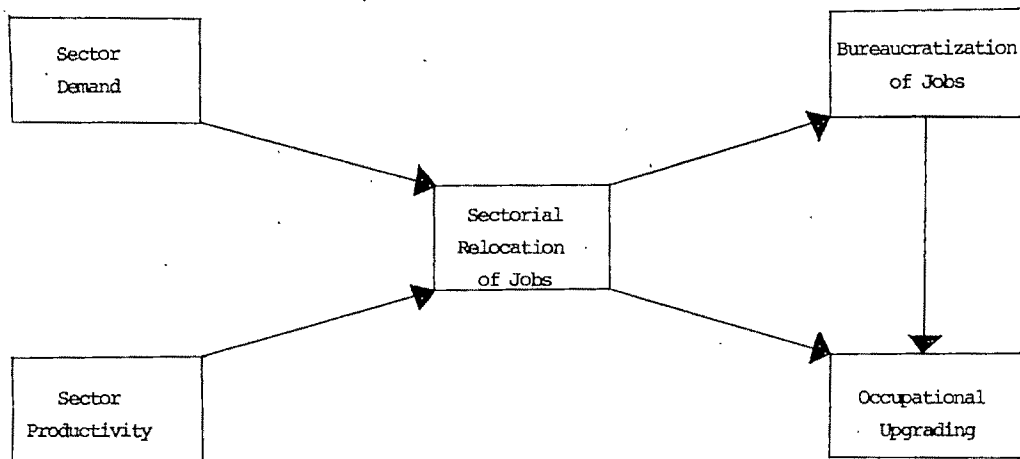


Figure 1. Flow Graph for the General Structural Relations of a Model of Changes in the American Occupational Structure

reaucratization, pertaining to the organizational context within which jobs are located; (3) job upgrading, pertaining to the shift of jobs into higher status categories. In brief, the first category relates to the sectorial distribution of employment, the second to the organizational context of work and the third to the nature of the work done. Although Moore (1966) originally introduced similar characteristics of occupational structures in the context of economic development, Land and Pampel (1977) recently demonstrated their relevance for the description of contemporary social change.¹

The theory underlying the relationships graphed in Figure 1 can be characterized as postulating: (1) that changes in the sectorial distribution of jobs in the United States respond to changes in sectorial (economic) demand and (technological) productive efficiency; (2) that shifts in the sectorial location of jobs affect the distribution of jobs by degree of bureaucratization of the organizational context of work; and (3) that changes in the distribution of jobs by status level are determined by the sectorial relocation and bureaucratization of work. In other words, we argue that the means and extent of economic production in the society create and change the distributions of jobs among competing categories of sector, type of organization and status. Although the assumptions of

the model can be stated in this very general and simplified form, detailed arguments for the specification of individual equations are more complicated and are given in the following three sections of the paper.

Changes in the Sectorial Allocation of Jobs

Although more detailed schemes exist, we will utilize Clark's (1957) distinction between the primary (agricultural), secondary (manufacturing) and tertiary (services) industrial sectors of the economy because these categories maintain a degree of homogeneity of the type of work done in each sector that can be surpassed only by a considerably more detailed classification. Not only would a more detailed set of industrial categories require a greater number of structural equations for determining shifts of jobs among the sectors, but Clark's three-category scheme also has the advantage of having been used in other studies of contemporary social change. For example, noting that since World War II the United States has experienced substantial employment shifts into the service industries (which now constitute roughly 65% of employed persons), Bell (1973) characterized postindustrial society partly in terms of an increased economic orientation towards performance of services rather than production of goods.

Thus, in the model to be specified

¹ Some of the equations to be discussed expand and refine the analyses of Land and Pampel (1977).

Table 1. Ordinary Least-Squares Estimates of Sectorial Relocation Equations

(1.1) Percent Agricultural Employment	
$\%AGR_t = -3.4624 + 0.9988\%AGR_{t-1} + 0.1667AGRGNP_t - 164.5000AGRPROD_t$	
$(1.4377) \quad (13.1080) \quad (1.7158) \quad (1.1800)$	
+e _t	
$\bar{R}^2 = .9930 \quad SEE = 0.2397 \quad DW = 1.6975 \quad df = 20$	
(1.2) Percent Service Employment	
$\%SER_t = 26.0350 + 0.6191\%SER_{t-1} + 0.0330SERGNP_t - 1781.5000SERPROD_t$	
$(2.9800) \quad (3.4335) \quad (3.1241) \quad (2.3454)$	
+e _t	
$\bar{R}^2 = .9554 \quad SEE = 0.7996 \quad DW = 1.6855 \quad df = 20$	
(1.3) Percent Manufacturing Employment	
$\%MAN_t \equiv 100 - \%AGR_t - \%SER_t$	

Variables: %AGR = percent of civilian labor force in agriculture, forestry, hunting and fishing industries; AGRGNP = gross national product of agriculture industries in billions of 1958 dollars; AGRPROD = agricultural productivity defined as agricultural gross national product per 1,000 agricultural employees; %SER = percent of the civilian employed labor force in service industries, i.e., transportation, communication, utilities, wholesale, retail, finance, insurance, real estate, services, and government; SERGNP = gross national product of service industries in billions of 1958 dollars; SERPROD = service productivity defined as service gross national product per 1,000 service employees; %MAN = percent of the civilian employed labor force in construction and manufacturing industries.

herein, we begin with equations determining the percentages of the civilian labor force employed in the agriculture, manufacturing and services industries.² Two classes of economic and technological factors are postulated to account for annual shifts in sectorial employment (Clark, 1957; Moore, 1966): (1) changes in demand for different types of goods and services and (2) changes in the state of production technology as it affects the proportions of capital and labor necessary at each stage of the production process.³ Specifically,

our equations for shifts in sectorial employment rest upon the following two propositions. First, other things equal, an increase in the demand for a sector's product increases employment opportunities in that sector. Second, other things equal, increases in labor productivity in a sector—primarily due to the replacement of human labor by machine labor—reduce the opportunities for employment in that sector. For the purposes of the present model, we take changes in economic demand and production technology as exogenous.⁴

On the basis of these two propositions, we specify that demand will be related positively, and labor productivity negatively, to percent sector employment. Indexing demand by sector gross domestic product and productivity by sector product per worker gives the first two equations of Table 1. The third equation in Table 1 obtains percent manufacturing

² Some extant macro econometric models (e.g., the Wharton model as described in McCarthy, 1972) contain stochastic equations determining the numbers of workers in various industrial sectors, from which the percent sector employment can be obtained by an algebraic identity using the total number of civilian employees as a base. Because of the intrinsic interest of the relative sizes of industrial employment in sociological theories, however, we estimate percent sector employment by direct stochastic formulation of the forces that account for sectorial relocation of jobs.

³ In the context of economic development, Moore (1966) cites a third class of factors responsible for employment shifts, namely, changes in institutional requirements and constraints. For post-World War II American economy, however, changes in both macro and micro social and economic policies are likely to be revealed either in changes of sectorial demand or productivity. Thus, we expect little loss of explanatory power by focusing on the latter two

factors within the context of this historical period for American society.

⁴ Yearly changes in sector demand are usually determined by the Keynesian components of annual macroeconometric models, whereas the rate of change in the substitution of machine labor for human labor typically are handled in the neoclassical production function components of such models; see, for example, Hickman and Coen (1976).

employment (%MAN) by an algebraic identity, which closes the system of the three equations by forcing the predicted, as well as the observed, values to sum to 100 percent.

The equations in Table 1 present metric regression coefficients estimated by ordinary least-squares; t-ratios calculated from the coefficients divided by their standard error in parentheses beneath the coefficients; the squared multiple correlation coefficient adjusted for degrees of freedom (\bar{R}^2); the standard error of estimate (SEE); the Durbin-Watson autocorrelation statistic (DW); and the degrees of freedom (df) in the estimated equations. The equations are estimated for the years 1957 to 1972. However, the number of observations will vary from equation to equation depending on the availability of appropriate data for all the years (see Appendix for data sources).

In addition to the exogenous variables, the two stochastic equations of Table 1 include a lagged endogenous variable. When the values of an endogenous variable take more than one time period (a year in the present model) to adjust to changes in the exogenous or other predetermined variables of the equation, it is necessary to introduce an expression for the rate of change in the endogenous variable with respect to time (Blalock, 1969; Coleman, 1968). Since we are dealing with data in the form of discrete time series, we introduce a finite first-difference $\Delta y = y_t - y_{t-1}$ to account for the behavior of such variables.⁵ When estimated in autoregres-

sive form (i.e., with a lagged dependent variable), such equations allow the rate of adjustment in the endogenous variables per year to be estimated by subtracting the estimated coefficient of the lagged endogenous variable from one (Goldberg, 1958).

Applying this interpretation to equation (1.1) of Table 1, we see that the rate of adjustment of percent agriculture (%AGR) to changes either in demand for agricultural products or in agriculture productivity is very slow: a change of one unit in either of these exogenous variables leads to an adjustment of only 0.0012 units per year in %AGR towards its equilibrium value. This exceedingly slow rate of adjustment of agricultural employment to changes either in demand or in productivity corroborates similar findings for the rate of adjustment of the percent of the population living on farms to changes in the ratio of farm to nonfarm income (Land and Felson, 1977). It also accounts for the highly significant t-ratio of the lagged term in equation (1.1).

Other aspects of the %AGR equation indicate reasonably good performance. With its very slow adjustment rate, the \bar{R}^2 is expectedly high and the standard error is correspondingly small. The latter statistic indicates that, under the assumption of independently and normally distributed disturbances, the observed %AGR should be within 0.48% of the predicted value 95% of the time. The one-tailed probability of the AGRGNP is .05, while that of AGRPROD is only .11. Finally, the Durbin-Watson statistic is large enough to fail to reject the null hypothesis of zero autocorrelation among the disturbances of the equation at the .05 level.⁶ In brief, the

⁵ In the case of the simplest possible linear functional form, we let the amount of change in y -values from time $t-1$ to time t be a function of the value of y at time $t-1$ and the values of the exogenous values which we denote for convenience by the constant a :

$$\Delta y = y_t - y_{t-1} = a + b y_{t-1}.$$

By adding y_{t-1} to each side of the equation, we obtain

$$y_t = a + b^* y_{t-1}$$

where $b^* = b+1$. This is in a form convenient for statistical estimation (cf. Land and Felson, 1976:579-97). Substantively, if the value of the endogenous variable does not adjust within one time period to changes in the exogenous variables, this will show up statistically in the form of a significant regression coefficient for the lagged dependent variables. Conversely, if the value of the endogenous variable is at or very near its implied equilibrium

value within one time period after a change in the exogenous variables, then the lagged dependent variable will have an insignificant estimated regression coefficient. For a more detailed discussion of the behavior of sequences of y -values and the implied equilibria under different values of a , b^* and y_0 , see Goldberg (1958:86) and Land and Felson (1976).

⁶ The Durbin-Watson test statistic was constructed for testing for autocorrelation of disturbances in regression equations that do not have lagged dependent variables present on the right-hand side, and it is well known that the statistic is biased toward acceptance of the null hypothesis of zero

empirical estimates of the %AGR equation conform relatively well to our theoretical arguments.

The statistical properties of equation (1.2) of Table 1 for determining the percent service (%SER) can be similarly interpreted. The estimated rate of adjustment of %SER to changes in demand and productivity, approximately 0.38% per year, is much faster than that for %AGR. Both the demand and productivity indices have significant estimated coefficients in theoretically expected directions. From the larger direct effect of the demand index (SERGNP) relative to the productivity index (SERPROD), it can be inferred that the increase in %SER is due more to increased demand than to increased productivity, a finding consistent with that of Fuchs (1968).

In terms of implications for overall trends in sectorial relocation of jobs, the negative estimated value of the constant term in equation (1.1) indicates that %AGR is declining, whereas the positive estimated value of the constant term in equation (1.2) indicates that %SER is increasing. Depending on what is happening to the percent of the labor force employed in manufacturing, the increase in %SER could be at the expense either of %AGR or of %MAN. Actually, %MAN oscillates around a relatively constant level because increased demand has been balanced by increased productivity (Land and Pampel, 1977). For instance, %MAN was 33.6 in 1950 and 32.2 in 1970. Thus, we infer that the relative growth of the service sector has been primarily at the expense of the agricultural sector, a finding consistent with Nordhaus (1972).

autocorrelation of the disturbances when a lagged variable is present. However, simulation studies for models similar to those used herein have shown that use of a number at or near the "upper bound" of the statistic for determining critical region of the test statistic will correct for the bias in most cases (Taylor and Wilson, 1964). For this reason, and because it is customary to report the Durbin-Watson statistic in statistical analyses of econometric models that contain lagged dependent variables, we follow that practice here, recognizing that there is no basis in statistical theory for a strict probability interpretation of the statistic.

Changes in the Bureaucratic Organization of Jobs

We now turn to the implications of these sectorial relocation trends for changes in the organizational context of work. In particular, we examine the consequences of the post-World War II sectorial relocation of American jobs from agriculture to service industries for the extent to which jobs are concentrated in large formal organizations.

Measurement of structural attributes of bureaucratization of organizations at the societal level is typically based on concepts: (1) structured communication, (2) specialization and division of labor, (3) hierarchical authority and (4) the need for expert, specialized training. For example, Frisbie (1975) developed indices of these concepts suitable for use in cross-cultural analysis. Our measures are based on the same concepts, but the use of annual, national data requires that our operationalizations differ slightly from those of Frisbie. First, following Stinchcombe (1965) and Frisbie (1975), level of structured communication is indexed by the percent of the labor force employed in clerical positions. Structured, written communication requires clerical workers to process official documents. Second, the Gibbs-Martin index is used to measure division of labor. This measure reflects the differentiation of jobs among nine industries (Gibbs and Martin, 1962; Labovitz and Gibbs, 1964).⁷ Third, Frisbie (1975) used proportion of the labor force in administrative, executive and managerial occupations to operationalize hierarchical structure. However, annual data for the United States groups both self-employed and wage and salary administrators, executives and managers together. Inclusion of

⁷ The calculating formula for the Gibbs-Martin index is

$$\text{DIVLAB} = 1 - (\sum X_i^2 / (\sum X_i)^2)$$

where X_i is the number of persons in each industry (Gibbs and Martin, 1962). The index is at its maximum when workers are evenly dispersed throughout the industries. The industries used to calculate the index are agriculture, construction, manufacturing, transportation and utilities, wholesale, retail, commerce, service, and government.

self-employed makes the measure unsuitable. The surrogate index used, recommended by Bendix (1956:211) as the single most useful measure of the internal bureaucratization of the occupation structure, is the percent wage and salary employees. While not a direct measure of hierarchical authority, percent wage and salary workers does reflect the transfer from self-employment to work in larger, bureaucratic organizations. Fourth, we will not concern ourselves with the fourth characteristic, expert training, since changes in educational variables have been studied elsewhere (Felson and Land, 1977). Finally, since these measures of bureaucratization may correlate with other changes in industrialized societies, we concentrate on changes in the indicators themselves rather than attempting to measure some underlying dimension of bureaucratization.

Our objective here is to account for the trends and fluctuations in these aggregate bureaucracy indices. To do so, we utilize two distributional relationships. First, for a variety of reasons having to do with the technology and scale of production, the average number of employees per economic unit in agriculture tends to be smaller than the corresponding average in

the manufacturing or services sectors, and the proportion of units that is very small (one to three employees) tends to be larger in agriculture than in manufacturing or services (Land and Pampel, 1977). Furthermore, not only do service organizations tend to be larger, on the average, than agricultural establishments, but there was a greater increase in the average size of service establishments than in the average size of agricultural (or manufacturing) establishments during the period 1948-1973 (Land and Pampel, 1977). On the basis of these structural relationships, it can be inferred that a shift of workers out of agriculture into services tends, on the average, to put them into larger organizations. Since larger organizations tend to be more bureaucratic than smaller ones, it follows that the sectorial relocation of workers from agriculture to services should produce increases in our indices of the bureaucratic organization of jobs.

From this discussion, we are led to specifications of annual changes in our indices of bureaucratization as increasing functions of percent manufacturing and, especially, percent service employees. This structure is exhibited in the estimated equations of Table 2. In addition, equation

Table 2. Ordinary Least-Squares Estimates of Bureaucratization Equations

(2.1)	Percent Wage and Salary Workers	
	$\%WAGSAL_t = -2.4829 + 0.4693\%WAGSAL_{t-1} + 0.3905\%MAN_t + 0.6212\%SER_t$	
	<div style="display: flex; justify-content: space-around; width: 100%;"> (0.4586) (4.5218) (2.8614) (4.8261) </div>	
	$-0.3626UNR_t + e_t$	
	(4.8261)	
	$\bar{R}^2 = .9934$	$SEE = 0.2887$
	$DW = 1.7511$	$df = 19$
(2.2)	Clerical Workers	
	$\%CLER_t = -13.7030 + 0.5730\%CLER_{t-1} + 0.1870\%MAN_t + 0.2380\%SER_t + e_t$	
	<div style="display: flex; justify-content: space-around; width: 100%;"> (2.4386) (3.1848) (2.2169) (2.5116) </div>	
	$\bar{R}^2 = .9740$	$SEE = 0.2615$
	$DW = 2.3545$	$df = 21$
(2.3)	Division of Labor	
	$DIVLAB_t = 0.5635 + 0.2991DIVLAB_{t-1} - 0.0011\%MAN_t + 0.0009\%SER_t + e_t$	
	<div style="display: flex; justify-content: space-around; width: 100%;"> (5.5998) (2.1343) (2.3982) (2.9196) </div>	
	$\bar{R}^2 = .9237$	$SEE = 0.0019$
	$DW = 1.2569$	$df = 21$

Variables: %WAGSAL = the number of wage and salary workers as a percent of the civilian employed labor force; UNR = the unemployment rate defined as the yearly average of the number of employed persons 16 years and over as a percent of the civilian labor force; %CLER = the number of clerical and kindred workers as a percent of the civilian employed labor force; DIVLAB = Gibbs-Martin index of inter-industry division of labor, using a nine-industry classification of employed wage and salary workers.

(2.1), which determines the percent of the civilian labor force employed as wage and salary workers (%WAGSAL), contains the aggregate unemployment rate as an index of cyclical changes in the demand for workers.

The estimated coefficients of equation (2.1) reveal several interesting properties. First, they show that the direct impact on %WAGSAL of a one percentage point increase in %MAN is less than two-thirds the size of the direct effect of a one percentage point increase in %SER. This is consistent with the findings on sectorial relocation during the estimation period that were noted in the preceding subsection. Second, the small negative, but insignificant, estimated constant term shows that the population constant term is near zero or even negative. Combined with the dynamics of the difference equation, this implies that the implied equilibrium values for %WAGSAL would be very small if %MAN and %SER took on increasingly small values. Thus, the dynamics of the equation are consistent with the relative scarcity of wage and salary work in a predominantly agricultural society. The other coefficients of the equation show that the effect of business cycles on wage and salary employment is substantial and that the adjustment rate of %WAGSAL to changes in the predetermined variables is about 0.53 percentage points per year. Finally, the summary statistics for the equation show that it tracks the time path of the dependent variable very well and that the null hypothesis of zero autocorrelation of the disturbances cannot be rejected.

Consider next the numerical properties of equation (2.2) which determines our index of the extent of structured communication in organizations, namely, the percent of employees in clerical and related occupations (%CLER). In this case, the sizes of the estimated direct effects of %MAN and %SER are of more nearly equal magnitude, and the annual rate of adjustment of the equation towards equilibrium is somewhat slower than that of the preceding equation. However, the significant negative constant term corroborates the observation made above about the smaller degree of implied bureau-

cratization for predominantly agricultural societies. Again, the summary statistics show that the equation performs well.

The final equation of Table 2 represents our attempt to index changes in the third indicator—specialization and division of labor. For this purpose, we study annual changes in the Gibbs-Martin (GM) index of the division of labor among nine industrial categories for employed wage and salary workers. Not only does the GM index have some defects as a measure of the classical notion of the division of labor (Gibbs and Martin, 1962), but it would be preferable to apply the index to a classification of workers by detailed job categories rather than by industries (Labovitz and Gibbs, 1964). Nevertheless, even with these limitations of the index and the data, the estimated coefficients of equation (2.3) yield to an interpretation of the sectorial relocation of workers into services as leading to a greater dispersion of jobs among the industries and, hence, to a greater division of labor index. On the other hand, an increase in %MAN leads to greater concentration among the manufacturing industrial categories and consequently decreases the value of the GM index. With a constant term of about 0.56 and an adjustment rate of about 0.70 units of the GM index per year, the implied equilibrium value of the index at zero levels of %MAN and %SER is about 0.81. This is not inconsistent with other empirical results on the division of labor in more rural societies. For example, using the 253 detailed occupations in the United States Census of 1900 to classify workers, Labovitz and Gibbs (1964) obtained a value of 92.2 for the GM index. In brief, the numerical properties of equation (2.3) are meaningful. However, its overall statistical performance is not as good as that of the other two equations in Table 2; in particular, its Durbin-Watson statistic is indeterminate with respect to the null hypothesis of no autocorrelation of the disturbances. Thus, the equation may be somewhat misspecified, and we reserve judgment until we examine its forecasting performance.

In summary, we find that post-World War II trends and fluctuations in the sectorial relocation of jobs account reason-

ably well for changes in aggregate indices of the extent to which jobs are bureaucratized in American society. We have argued that this effect is transmitted through an increase in the mean size of the organizations into which the jobs are moved relative to those from which the jobs came. Although the data on changes in the mean size of organizations are not sufficiently detailed and frequent to be used in our time-series model, the extant fragments of data do not contradict this explanation.

Changes in the Status Distribution of Jobs

In addition to their consequences for the bureaucratization of work, our model postulates that changes in the sectorial location of jobs account for the post-World War II movement of jobs up the status scale (Land and Pampel, 1977), a movement usually referred to as "upgrading" (Moore, 1966). The basic distributional mechanisms through which this effect occurs in our model are as follows. First, shifts of jobs out of agriculture tend to decrease opportunities for employment in farming and farm-related occupations, which are among the lowest status categories (Blau and Duncan, 1967:277-94). Conversely, the shift of jobs into the manufacturing and service sectors creates greater opportunities for employment at the upper end of the status scale, since jobs in the non-agricultural sectors tend to have greater concentrations in the higher status categories than do those in agriculture (Blau and Duncan, 1967:23-162). The combined effects of these movements at the tails of the status distribution of occupations lead to an increase in its moment of gravity (central tendency, mean). Moreover, increases in jobs at either end of the status scale should be positively associated with the dispersion of the occupational distribution among status categories. However, the total numerical empirical effect of changes in densities at the tails on dispersion of the status distribution will depend not only on the relative sizes of the changes but also on their respective weights in determining the dispersion. Second, large bureaucracies

(created by sectorial relocation out of agriculture) tend to employ more highly trained technicians, professionals and managers than do small organizations, since increasing bureaucratization, on the average, means more specialization and division of labor, as we noted above. Therefore, we expect an aggregate increase in bureaucratization to be associated with job upgrading. These mechanisms lead directly to the specifications of the equations reported in Table 3.

Examining these equations, we find that the estimated coefficients of equation (3.1) in Table 3 for determining changes in the percent of the employed labor force in professional and related occupations (%PROF) show that an increase of one percentage point in either %MAN or %SER translates into a direct impact of about 0.27%. This implies that the proportions of new jobs in either sector that fall into the PROF category are approximately equal. The sizes and statistical significance of the constant and lag terms are consistent with the interpretations that we have given to the equations in Table 2. Furthermore, the summary statistics for the equation indicate good overall statistical performance with no autocorrelation of the disturbances.

The estimated constant term and lag coefficient of equation (3.2) for the percentage of the labor force employed as farm managers or farm laborers (%FARM) can be given an interpretation similar to that for %PROF, except that the coefficient of annual adjustment toward equilibrium is larger for %FARM than for %PROF. In addition, the estimated coefficient of %AGR in this equation shows that a one percentage point decline in %AGR translates into a direct decline of about 0.95% in %FARM. In other words, roughly 95 percent of the decline would be in farm managers and farm laborers. In terms of summary statistics, the equation accounts for most of the variation in the %FARM time series and has a small standard error of estimate, but its Durbin-Watson statistic is in the "indeterminate" region, so that some autocorrelation of the disturbances may be present.

The last two equations of Table 3 determine changes in the mean (MOS) and

Table 3. Ordinary Least-Squares Estimates of Occupational Upgrading Equations

(3.1) Percent Professional Employment

$$\%PROF_t = -20.6480 + 0.6740\%PROF_{t-1} + 0.2722\%MAN_t + 0.2622\%SER_t + e_t$$

(2.3219)
(4.7639)
(2.4929)
(2.2234)

$\bar{R}^2 = .9951$
SEE = 0.2232
DW = 1.7323
df = 21

(3.2) Percent Farm Manager and Laborer Employment

$$\%FARM_t = -0.8257 + 0.1187\%FARM_{t-1} + 0.9533\%AGR_t + e_t$$

(6.6968)
(1.2129)
(8.6812)

$\bar{R}^2 = .9973$
SEE = 0.1588
DW = 1.0499
df = 22

(3.3) Mean Occupational Status

$$MOS_t = 44.1570 + 0.6001\%CLER_t + 0.4365\%PROF_t - 0.2372\%FARM_t + e_t$$

(14.4180)
(5.2195)
(2.6031)
(1.8849)

$\bar{R}^2 = .9947$
SEE = 0.2127
DW = 1.6235
df = 21

(3.4) Standard Deviation of Occupational Status

$$SDOS_t = 0.2659 + 0.7292SDOS_{t-1} + 0.3191\%PROF_t + 0.3304\%FARM_t + e_t$$

(0.1499)
(10.4160)
(3.5608)
(4.2818)

$\bar{R}^2 = .9862$
SEE = 0.1088
DW = 2.0734
df = 21

Variables: %PROF = the number of professional, technical, and kindred workers as a percent of the civilian employed labor force; %FARM = number of farm managers and farm laborers as a percent of the civilian employed labor force; MOS = mean occupational status of civilian employees in Census SIO units weighted by the distribution among occupational categories of civilian employees; SDOS = standard deviation of occupational status in Census SIO units.

standard deviation (SDOS), respectively, of the distribution of occupations by status.⁸ Both equations have good overall statistical properties and are consistent with the arguments stated above, but the

⁸ The mean and standard deviation indices of the status distribution of occupations are calculated by weighting the annual distributions among eleven occupational categories of civilian employees by the U.S. Census Bureau's Status Index of Occupations (SIO). The SIO is based upon the combination of average levels of education and income for employed males by occupation in 1950. For details on the construction of the index, see U.S. Bureau of the Census (1967). Of course, by using a single status index to scale annual reports of the distribution of American occupations, we are implicitly assuming that the statuses of occupations are temporally stable enough during the post-World War II period to make a comparison of annual values of MOS and SDOS meaningful. On this point, see Blau and Duncan (1967:120-1). In addition, there exist several alternative status scales besides the Census SIO scale that could be used to calculate MOS and SDOS. Nevertheless, since these scales are highly intercorrelated with each other, our findings should be quite robust to the particular scale used. Finally, it goes without saying that, provided the data were available, it would be preferable to calculate our MOS and SDOS indices on detailed annual job distributions rather than on the eleven summary categories that we have used. However, the basic qualitative properties of our model should be invariant to calculations based on more refined classifications.

difference between the two are noteworthy.

First, whereas the adjustment of MOS to changes in the predetermined %CLER, %PROF, and %FARM is immediate (the lagged dependent variable is not statistically significant and, thus, is dropped from the equation), the annual rate of adjustment of SDOS is relatively slow (0.27). From this, we infer that the predetermined variables in the MOS equation are sufficient to account for the year-to-year changes in the index, but that the time path of SDOS apparently reacts to changes in other occupational categories besides PROF and FARM. In fact, it may be the case that changes in the latter set off a chain reaction of changes in other categories that culminates in a slow reaction rate for SDOS.

Second, whereas the negative coefficient of %FARM in the MOS equation is only a fraction of the coefficients of %PROF and %CLER, the positive coefficient of %FARM in the SDOS equation is slightly larger than that of %PROF. On this basis, we infer that the movement of jobs into the PROF and CLER categories has been more important in raising MOS than has been the movement out of farm-

Table 4. Conditional (*Ex Post*) Forecasts of the 1973 and 1974 Observed Values of the Stochastically-Determined Social Indicators

	1973				1974			
	Observed	Forecast	Forecast Error ^a	Percent Error ^b	Observed	Forecast	Forecast Error ^a	Percent Error ^b
%AGR	4.1	4.2	-0.1	2.4	4.0	3.6	0.4	10.0
%SER	64.2	66.2	-2.0	3.1	66.5	66.8	-0.3	0.5
%WAGSAL	90.3	90.3	0.0	0.0	90.4	90.5	-0.1	0.1
%CLER	17.2	17.5	-0.3	1.7	17.5	17.5	0.0	0.0
DIVLAB	.8324	.8374	-.005	0.6	.8332	.8418	-.0086	1.0
%PROF	14.0	14.3	-0.3	2.1	14.4	14.5	-0.1	0.7
%FARM	3.6	3.5	0.1	2.8	3.5	3.5	0.0	0.0
MOS	59.7	59.7	0.0	0.0	60.2	60.1	0.1	0.2
SDOS	22.17	22.25	-0.08	0.36	22.11	22.20	-0.09	0.41

^a Error = Observed Value — Forecasted Value.

^b Percent Error = (Observed Value — Forecasted Value/Observed Value) × 100.

ing occupations. But the direct effect of a one percentage point decline in %FARM on decreasing the dispersion of occupations by status is slightly larger than the direct effect of a comparable percentage increase in %PROF on increasing the dispersion.

Empirically, from 1947 to 1972, the %PROF category increased 7.4 percentage points from 6.6% to 14.0%, the %CLER category increased 5.0 percentage points from 12.4% to 17.4%, and the %FARM category declined 6.5 percentage points from 8.5% to 2.1%. Combined with equations (5.3) and (5.4), we find that the changes in %PROF and %CLER account for most of the increase in the center of gravity of the status distribution of occupations, whereas the dispersion of the distribution has an overall decline due to a greater direct impact (decline times the estimated parameter) of %FARM on SDOS than of %PROF.

Forecasting with the Model

While social forecasting usually is not an integral part of sociological analyses based upon cross-sectional survey research, the prediction of postestimation sample period values of the endogenous variables conditional on observed values of the predetermined variables (usually referred to as *ex post* forecasting) is a natural outgrowth of the time-series estimation of a dynamic macro social indicator model (Land and Felson, 1976:596-8). Not only are substantive forecasts feasible

in the context of a dynamic model, but they also provide a statistical means for further evaluating the model's adequacy. If the equations we have constructed actually reflect causal mechanisms and are not simply exercises in curve fitting, then forecasts of subsequent timepoints of the dependent variables, conditional on observed values of the exogenous and predetermined variables, should be relatively accurate. That is, in the absence of true structural change (which can never be completely discounted), a conditional forecast is another means of determining the impact of many possible sources of error on the obtained parameter estimates.

Since the estimation sample period of our model is 1947 to 1972, we can make *ex post* single-year forecasts for subsequent years. Table 4 reports the observed values, forecast values, forecast errors, and percent errors (as a percentage of the observed values) for the endogenous social indicators of our model for which data for 1973 and 1974 are available. As can be seen, the forecasts are accurate for the most part. Specifically, for 1973, none of the forecast errors is greater than 2.5 times the standard errors of the corresponding structural equations, and most of the forecasts are in error by a fraction of the standard errors.⁹ A similar remark

⁹ In making these comparisons, we have used the standard errors of the structural equations. This is a rather strong criterion, since standard errors of post-sample period forecasts will usually be larger

applies to the 1974 forecasts with the exception of the DIVLAB forecast which has an error about 4.5 times as large as the standard error of its equation. This is consistent with our previous analysis of the possible misspecification of this equation, and it suggests that the equation may need reformulation and/or reestimation.¹⁰ The forecast error of %AGR in 1974 is less than twice the standard error of the equation, but, because of the small observed value of %AGR, the percentage error is large. This underestimate of the 1974 value may result from a drop in 1974 farm prices and agricultural GNP. As discussed previously, the rate of adjustment in %AGR to exogenous changes is quite slow and the effects of the drop in 1974 prices may not be felt completely for a few years.

Overall, the forecasts in Table 4 are quite accurate, with 4 out of 18 forecasts having *no* errors. Since 1973 and 1974 were years of early economic expansion followed by a contraction due to the "energy crises" beginning in late 1973, and since these years have proven, therefore, to be particularly difficult for macro econometric models to forecast, such results for our model are promising.

Discussion of the Model

Now that the detailed equations of the model have been presented, several of its general features may be discussed. First, while we have taken changes in sectorial demand and production efficiency as exogenous to the present model, we do not assume that they are absolutely exogenous to the socioeconomic system. Each category of variables can be endogenized by the integration of the present model with models of changes in economic demand and production

technology. Second, although we have postulated a recursive causal structure for the variables of our model with no explicit feedback relationships from aggregate changes in the occupational structure to changes in aggregate sectorial demand or technology, we recognize that such feedbacks may exist through aggregate changes in life styles, through aggregate increases in managerial efficiency, and so forth. However, not only are these effects likely to be quite indirect, but they also are likely to involve substantial time lags. Consequently, there probably is little simultaneity bias in our estimates due to the recursivity assumptions.

The empirical properties of our model are relevant to those classical and contemporary theories of social change that have placed great emphasis on the role of technological and economic change in societal development. For example, two goals of Ogburn's work were to examine how society adjusts to the technological environment (Ogburn, 1950) and how the business cycle affects society (Ogburn and Thomas, 1922). Other classical theorists have examined the role of technology on broader societal development and social equality, while Bell (1973) has discussed the implications of technological change for the United States in recent decades. Although it is not our goal to evaluate these theories, become involved in the controversy over the primacy of technological or cultural factors in social change, or offer an alternative theory of social change, we have demonstrated in precise, quantitative terms the impact of economic and technological change on several aspects of post-World War II America.

Our results are also pertinent to questions raised by recent mobility research. Hauser et al. (1975) have separated the amount of mobility due to the association between father's and son's occupation from that due to the changing marginal distribution of occupations. They found that changes in the marginal distribution of the American occupational structure have been primarily responsible for increased rates of upward intergenerational mobility in the twentieth century and suggested that students of mobility pay

than their sample-period counterparts (for a derivation of and discussion, see, for example, Goldberger, 1964:169-70).

¹⁰ For instance, if no theoretically acceptable alternative specification can be found that performs better than equation (4.3), then it could be reestimated using a statistical technique that does not assume zero autocorrelation of disturbances such as the Cochrane-Orcutt iterative estimation process (Cochrane and Orcutt, 1949).

more attention to the causes of change in occupational distributions. Our model demonstrates the effect of several factors on the growth of high status occupations in the United States since 1947. Technological improvements in agriculture have allowed replacement of farm and farm-related human labor by machine labor, thereby reducing the number of farmers; the consequences of the resulting sectorial relocation and bureaucratization of work have been closely tied to the expansion of high status technical and professional occupations.

The societal implications of these structural relationships are clear. While changes in sectorial demand and productive efficiency were substantial during the 1947-1972 period, the rates of change in these driving variables of our model have decreased in the 1970s. If these rates of change continue to decline, then our model leads to the conjecture that there may be a contraction in the rate of growth of high status jobs. Combined with the Hauser et al. (1975) findings, this implies a decline in rates of upward occupational mobility in the absence of other counterbalancing structural changes.

As a concluding note, we emphasize that the model presented here is only a "baseline" model. Clearly, there are many aspects of aggregate changes in the sectorial location, bureaucratization and upgrading of work in contemporary American society that we have not been able to incorporate into our model. As new and improved social indicator time series become available, surely this initial model can be extended and/or replaced.

APPENDIX

Sources for Time Series Data

AGRGNP, SERGNP: Economic Report of the President, 1974, U.S. Department of Commerce.
%WAGSAL, %CLER, %PROF, %FARM, MOS, SDOS: Manpower Report of the President, various years, U.S. Department of Labor; MOS and SDOS were calculated from published figures; note also that inclusion of 14-15-year-olds in the collection of the data before 1958 may have a depressing effect on MOS, %PROF and %CLER since 14-15-year-olds generally hold low status jobs.

UNR: Handbook of Labor Statistics, 1975-Reference Edition, U.S. Department of Labor.
%AGR, %SER, AGRPROD, SERPROD: estimates of sector employment were obtained from Labor Statistics published by the Organization for Economic Cooperation and Development; the variables include all employed workers-agriculture, non-agricultural, self-employed, wage and salary, private and public; the details of estimation are presented in Land and Pampel (1977).
DIVLAB: Manpower Report of the President, 1975, U.S. Department of Labor; the index includes only persons employed on payrolls of non-agricultural establishments.

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THE STRUCTURE OF INEQUALITY AND THE PROCESS OF ATTAINMENT*

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This paper proposes a model for the process of attaining occupational status and income, where change in attainment is generated by the creation of vacant positions in social structure. The distribution of attainments, or the structure of inequality, is assumed fixed and is described by a simple exponential or geometric distribution function (depending on whether attainment levels are assumed discrete or continuous). Persons leaving the labor force create chains of vacancies in this structure that present mobility opportunities for persons entering the labor force. The implications of the model for the attainment process derived from these considerations for status attainment research and stochastic models for job mobility are discussed.

Introduction

Research on social mobility, status, and income attainment in sociology always has been heavily oriented toward the methodological problems posed by the subject matter under investigation. Thus, the development of indices in mobility research and problems of estimation and measurement in status attainment research have received a great deal of attention. Conceptual issues have been much less of a concern, although they have not been entirely unimportant. The concern for separating structural and exchange mobility in the development of indices of mobility and the concern for the temporal ordering of variables and for causal directions in status attainment research, reflect theoretical assumptions regarding the

forces that generate mobility and achievement. Nevertheless, the dominant research strategy has been inductive, rather than deductive: the accumulation of empirical findings from cross-national and cross-temporal studies is believed to produce a pattern from which a sociological theory of attainment and mobility will emerge.

This situation is in sharp contrast to the approach in economics to the study of one aspect of the attainment process—income attainment. Neoclassical economists have applied a powerful conceptual apparatus to income attainment in the form of human capital theory. The attainment of income in this perspective is conceived of as reflecting a person's productivity as determined by his/her ability and skills. Skills are obtained through education and training at a cost primarily in the form of earnings foregone. Returns on the investments in training and education are obtained in a competitive market where earnings are determined by the marginal productivity of labor. A number of empirical predictions can be derived from this theory—the shape of the age-earnings profile, the impact of wage differentials on demand for education, the allocation of training costs for general and specific on-the-job training, etc. A list of other derivatives from the theory is presented by Becker (1964:7-8). Few such predictions can be made from sociological research on at-

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tainment processes where there is heavy emphasis on estimating the relationship among observed variables, not on modeling the process that produces the observed outcomes.

Human capital theory provides powerful predictions about the attainment process, but this does not mean that it is the only possible, or necessarily the most useful approach to the study of attainment processes. Some basic predictions from the theory do not square well with reality: from the theory one would predict that changes in the distribution of education would alter the distribution of incomes because of the changed supply at different skill levels. Since World War II, no such change can be observed in the distribution of income despite a marked shift in the distribution of education (Thurow and Lucas, 1972). Numerous criticisms of the theory also have been raised because of its apparent failure to account for the processes that are believed characteristic of important segments of the labor markets (Doeringer and Piore, 1971; Thurow, 1975).

Criticisms against a powerful theory, based on the failure of the theory to account for some empirical observations, are often ambiguous. Those who believe in the theory can usually come up with modifications that will save the theory by extending it and altering less important assumptions. Usually, human capital theorists are willing to allow for imperfections in the degree to which the real world approximates the neoclassical world that they assume. These imperfections then may be used to excuse the apparent failure of some empirical predictions. Further, they can point with considerable merit to the theory's ability to account for a number of basic features of observed processes and to the inability of critics to come up with an alternative theory equally parsimonious and with equal explanatory power. Theories are replaced with other theories, not with a set of isolated empirical observations that are subject to different interpretations.

The conception of mobility used in much traditional mobility research could be a point of departure for the formulation of an alternative theory of the attainment

process because of the contrast it provides with basic assumptions of human capital theory.¹ In human capital theory, changes in attainment are assumed to be brought about exclusively through changes in a person's productivity, i.e., skills and experience. The distribution of skills, in turn, is reflected in the distribution of earnings. In traditional mobility research, change in attainment, in contrast, is assumed to reflect changes in positions in a predetermined structure of inequality, without accompanying changes in personal characteristics. Persons can move only to a slot that is available, i.e., vacant, and while a person's "productivity" (as measured by ability, education, and experience) determines which slots a person gets access to, the distribution of attainments reflects the distribution of slots, not the distribution of personal attributes that are relevant for getting access to slots. Such a notion would be consistent with the lack of change in the income distribution in the face of a marked change in the educational distribution that is contrary to the implication of the neoclassical economic theory. It also would be consistent with the attainment processes that characterize primary labor markets (Doeringer and Piore, 1971) and job competition (Thurow, 1975) in the critiques of the neoclassical theory.

The sociological conception of mobility, however, has never been very well specified. It has been used to justify many attempts at separating structural from exchange mobility in intergenerational mobility tables, but this is a decomposition of the total amount of mobility in society, not

¹ The description of the neoclassical economic theory given here may be faulted for characterizing the theory as only a theory of supply, i.e., human capital theory. There is a demand side, that is marginal productivity theory. However, most theoretical and empirical work by economists on the earnings attainment process has focused on the supply side and, for this reason, the characterization seems justified. It could be argued that what follows in this paper is a specification of the impact of demand on the attainment process. The argument, however, would imply that the attainment process can be seen as a matter of supply and demand. This is not necessarily an appropriate conception. For a further discussion of these issues, see Sørensen and Kalleberg (1977).

a specification of the mechanisms of mobility generated by the creation of vacant positions in social structure. Further, since the objective here is to formulate a theory of the attainment process, the focus should be on intragenerational mobility rather than on intergenerational mobility as in most traditional mobility research.

Two tasks need to be carried out. It is necessary to specify how the creation of available or vacant positions generate mobility, and it is also necessary to specify how individual characteristics influence a person's utilization of mobility opportunities. Only a few attempts have been made at carrying out these tasks. With respect to the first task, works by Bartholomew (1972) and White (1971) are the main examples. White's (1971) vacancy-chain model is particularly suggestive of how structurally created opportunities generate mobility by generating chains of vacancies. However, the specification of how individual characteristics influence the utilization of vacancies is not attempted in White's work. Some attempts in this direction have been made by Boudon (1974), however, that resulted in a simulation model and not in a well-specified mathematical model.

The objective of this paper is to suggest a particular solution to the problem of specifying a theory of the attainment process that conceives of structurally-induced mobility as the source of change in individual attainment. This will involve (1) specifying a model for the structure of inequality, i.e., the distribution of possible attainments, then (2) specifying how vacancies occur and move in this structure and, finally, (3) modeling how change in attainments are brought about by the movement of people along the structurally-induced vacancy chains. These are the main tasks of the paper. The final sections of the paper will outline the relationship between the proposed model of the attainment process, status attainment research, and research on intragenerational mobility.

A number of very strong assumptions will be utilized in deriving the model. These assumptions are necessary to simplify an otherwise very complicated

problem. To some, the resulting model may appear highly unrealistic. That the model provides a very simplified picture of reality will not be denied. However, it does account for important features of observed processes and provides a plausible interpretation of research on attainment and mobility.

The Structure of Inequality

The objective is, as mentioned, to formulate a model for the attainment process, where change is brought about by utilizing opportunities for change of position in a predetermined structure of inequality. The positions will be conceived of as jobs, and these jobs may be characterized by the economic, social and psychological rewards they provide incumbents. It is assumed that only a change in jobs can provide a change in the level of rewards. This is a reasonable assumption with respect to most rewards, but it may appear dubious with respect to earnings. There will be real and inflationary increases in earnings within a job as well as some performance-related variation. These real and inflationary increases will be ignored because they usually do not change a person's relative position. Performance-related variation within jobs will be assumed to be of minor importance. One reason is that major performance differences for people in similar jobs are a source of instability and, hence, likely to result in differentiation of jobs.

Stated differently, the basic assumption is that different people in the same jobs will obtain the same rewards, while the same person will obtain different rewards in different jobs. With this assumption, the structure of inequality is given as the distribution of jobs with respect to status, income and other rewards.² Jobs may be vacant or filled, and people may be employed or unemployed. Hence, the distribution of jobs will not correspond to the distribution of people, although it will be

² No attempt will be made here to explain how the distribution of attainments comes into being. For the purposes of this paper, it is taken as a given. The assumptions stated here correspond to the one made by Thurow (1975) that marginal productivity resides in jobs, not in persons.

roughly similar to the distribution of employed people. For present purposes, this distribution will be assumed stable over time.

In the sequel, it will be assumed that there exists a measure of attainment level similar to the measures of prestige or socioeconomic status so commonly employed in status attainment research. As argued by Goldthorpe and Hope (1972), these measures reflect the "goodness" of occupations not the "prestige" of occupations in the usual sense of the word where the referent is to deference—that is, a relational concept—and not to the distributional concept captured by the Duncan SEI, NORC prestige scores, etc. However, the existing measures are ordinal and, though commonly employed as interval scales, this usage does not change their metric properties. The measure of attainment level assumed here is a ratio-level measure with a well-defined zero point.

The attainment level a person obtains in a job is denoted by y , where y varies from zero to infinity. I shall assume a very simple model for the distribution of y . This model is the exponential distribution with probability density,

$$f(y) = -\beta e^{\beta y} \quad \beta < 0 \quad (1)$$

and distribution function,

$$F(y) = \Pr(y \leq y') = 1 - e^{\beta y'}, \quad (2)$$

The distribution is governed by a single parameter β assumed to be less than zero. It describes a pyramidal structure of inequality in society, where the density of jobs decreases as y increases according to the formula,

$$\frac{df(y)}{dy} = \beta f(y), \quad (3)$$

as (1) is the solution to (3) assuming $f(0) = -\beta$. In other words, the distribution is such that the density is highest at the bottom level of attainment and decreases as y increases by a constant proportion with each small increase in y .

The quantity β may be said to govern the shape of the distribution of attainments. The larger β is in absolute mag-

nitude the more evenly jobs will be distributed. This means that if a person has a job providing a certain level of attainment, say y' , β will determine the proportion of jobs providing a level of attainment higher than y' , as the quantity $\Pr(y > y') = 1 - F(y) = e^{\beta y'}$ will be smaller the larger β is in absolute magnitude. The proportion of jobs that are better than the one currently occupied by a person represents possible opportunities for gains in attainment for this person. As β determines the proportion of jobs that are better jobs, it is one of the two quantities that governs the opportunity structure of a society—the other quantity being the rate at which better jobs become vacant, as I will show in the sequel.

The exponential distribution is the continuous variable analog to the geometric distribution that would apply if attainment was measured as a discrete variable.³ The geometric distribution has been suggested by several as a representation of the structure of inequality (Simon, 1957; Bartholomew, 1972; Svalastoga, 1973; Stinchcombe, 1974). Bartholomew (1972) shows that if this distribution is assumed for an organization, a particularly simple promotion schedule will prevail—a property to be used in this paper.

When evaluating the plausibility of the model (1), it should be noted that the quantity y is a construct. Specifying the relation between y and an observable reward will generate an observable distribution that can be used to validate the exponential model for y . Lydall (1959) has shown that, in the case of the geometric distribution of attainment levels, the Pareto distribution of incomes may be generated assuming that the log of earnings provided by jobs is linearly related to attainment. The argument is easily generalized to continuous y . The distribution function is

$$\Pr(w > w') = ke^{-\alpha \log w'} \quad (4)$$

³ The geometric distribution may be derived assuming that the ratio between the number of jobs at level k (where k is the discrete variable analog to y) is a constant proportion of the number of jobs at level $k-1$, or $n(k)/n(k-1) = s$ where $n(k)$ is the number of jobs at level k and s is a quantity less than one and greater than zero.

where w denotes earnings. Here, $-\alpha$ is a function both of β and the relation between y and earnings. Assuming the validity of the linear relation between $\log w$ and y , equation (4) could be used to evaluate the exponential model using observed earnings distributions.

Pareto proposed the model for income distributions that bears his name from inspection of observed income distributions based on tax returns. At that time, no returns were obtained from the lower portion of the distribution, and equation (4) provided an extremely good fit to the upper tail of the distribution. Pareto promoted equation (4) to a law, but subsequent analysis has shown that it does not fit the lower portion of the income distribution very well, and a number of other distributions will be similar to equation (4) in their tails. In particular, the log normal distribution first suggested by Gibrat (1931) provides a better overall fit.

The problem is that in observed distributions the density increases with increasing income in the lower portions, contrary to equation (4). It is well known that persons out of employment or with only marginal attachment to the labor force dominate in this part of the distribution. Equation (4) would be used here as a model for the distribution of the earnings provided by jobs, not personal incomes, and therefore may be less unrealistic. Further, only the distribution of filled jobs can be observed, while equation (4) would describe the distribution of all jobs whether filled or vacant, when used as a check on equation (1). Hence, it may be argued that a lack of fit is due to the omission of vacant jobs from observed distributions.

A model for observed prestige distributions could be generated assuming a one-to-one relationship between y and prestige (or SEI) scores. This may appear to be a reasonable proposal because of the definition of y presented above. However, none of the measures of prestige or socioeconomic status derived from prestige scores (as the SEI index) will result in distributions that can be used to test equation (1). The reason is that prestige scores as mentioned are inherently ordinal. Hence, they may be subject to any transformation that preserves rank order. Each transforma-

tion will result in a new distribution. The one that is observed using currently used measures therefore is completely arbitrary and cannot be used to validate (1).⁴ Only income distributions can be used, but then it is necessary to further assume the validity of a particular specification of the relation between y and income.

Despite the objections that may be raised, the exponential distribution will be used in the sequel as a model for the distribution of job rewards. It leads to a particularly simple and fruitful model for the attainment process and captures basic features of the structure of inequality. These properties are enough rationale for its use as a start.

The Creation of Opportunities for Growth in Attainment

Having formulated a model for the structure of inequality, the task now is to formulate a model of how opportunities for change in attainment are created in this structure. In the next section, the question of how characteristics of individuals affect their ability to take advantage of these opportunities will be addressed.

The structure of inequality will be assumed stable over time. People enter and exit the structure when they enter and leave the labor force. When people leave the labor force, they leave vacant jobs. These jobs will be filled either by new recruits or by people moving from other jobs into the job vacated. Following White (1971), two types of moves may be conceived of—(1) moves by people from filled jobs to vacant jobs, thereby creating new vacancies to be filled by others already in the system or by people entering the system, and (2) moves by vacancies in the opposite direction of the moves by individuals. Chains of moves by persons start when a person enters the labor force and end with retirement (temporary moves out of the labor force will be ignored). Chains of moves by vacancies start with the creation of a vacancy due to retirement (or the

⁴ In other words, no one knows what the structure of inequality is in society in terms of the "goodness" of jobs, for no one has seen it.

creation of a new job) and end by the elimination of a vacancy by a person from outside the system (or by the elimination of a job). Both people and vacancies move among jobs, but the mobility history of a vacancy is something different from the mobility of people. This section will focus on the mobility of vacancies. In the next section, the mobility of people will be linked to the mobility of vacancies.

When a person moves from one attainment level to another, a vacancy moves in the opposite direction. Upward moves by people in the structure represent increases in attainments and correspond to moves downward by vacancies. Only such moves will be considered. Although upward and horizontal moves by vacancies will take place in empirical systems corresponding to downward and lateral moves by people, they will be ignored here. Assuming persons maximize attainments, this restriction implies that only voluntary moves will be considered.

It will be assumed that persons enter and retire at all levels. It is immediately apparent that if voluntary moves are to take place at all, fewer people should enter than leave at some levels; in this way, vacancies will be created for people at lower levels to take advantage of. In work on mobility in organizations, it is often assumed that everyone enters at the bottom and leaves at the top (Bartholomew, 1972). This is obviously unrealistic for the societal structures of inequality considered here. A more realistic, although very simplifying assumption will be made here. It will be assumed that a proportion of jobs will be vacated due to retirements in each time period—the same at all attainment levels. Further, it will be assumed that some of these vacated jobs will be filled from the outside and some will be filled from below. The proportion to be filled from below presents new promotion opportunities and also will be assumed to constitute a constant proportion at each level. The exception is the bottom level, where all vacancies are filled by persons from the outside.⁵

It is assumed, in other words, that new vacancies not immediately filled by new entrants to the labor market are created at a constant rate for each level of attainment. These vacancies will reflect the addition of new jobs to the economy and/or that each person enters a promotion ladder that covers some, but not all attainment levels. By giving rise to vacancy chains, these vacancies create promotion opportunities in society. There is evidence that most job shifts are voluntary (Sørensen, 1975). Hence, the assumption of new promotion opportunities being created in each time period is reasonable, although the assumption of identical rates of new vacancies at all levels may not be realistic.

With these assumptions, one may calculate the rate at which vacancies arrive at a certain level of attainment. This rate will represent the opportunities for gains in attainments presented to persons who occupy jobs at lower levels of attainment. To do this, some new quantities need to be defined. As before, denote by $f(y)$ the probability density of y . The density of jobs at level y is $n(y) = Nf(y)$ where N is the total number of jobs in the system. Let $h(y)$ denote the rate at which new vacancies to be filled from below or new promotion opportunities are created at level y . Finally, let $m(y)$ denote the number of vacancies arriving at y per unit time.

The number of vacancies arriving at y , that is, $m(y)$ will equal the number of new vacancies created at $y + dy$ plus the number of vacancies arriving at $y + dy$ from attainment levels higher than $y + dy$. The number of vacancies arriving at $y + dy$ is $m(y + dy)$ by the definition of $m(y)$, and the number of vacancies created at $y + dy$ is $h(y + dy)n(y + dy)$. Hence,

$$m(y) = h(y + dy)n(y + dy) + m(y + dy) \quad (5)$$

⁵ Vacancies not immediately filled by new entrants to the labor force will move down according to the formula presented below. The corresponding va-

cancy chains are all terminated at the bottom level by new recruits from the outside. In other words, vacancies not immediately filled from the outside will create vacancy chains that move through the whole structure. The mobility chains of a person take place along these vacancy chains, but only over some part of the structure, as the next section will show.

per unit time.⁶ But since the rate at which new promotion opportunities are created is assumed the same at all levels $h(y) = h$ for all y , (5) can be written as

$$m(y) = hn(y + dy) + m(y + dy). \quad (6)$$

The argument that results in (6) may be used to generate an expression for $m(y + dy)$. This quantity will equal $hn(y + 2dy) + m(y + 2dy)$. Hence (6) may be written as

$$m(y) = hn(y + dy) + hn(y + 2dy) + m(y + 2dy). \quad (7)$$

Further, $m(y + 2dy)$ may be written as a function of the rate at which new promotion opportunities are created at the next higher level and the density of jobs at this level. In this fashion, $m(y)$ eventually may be obtained as h times the total number of jobs at higher levels of attainment. The total number of jobs at higher levels of attainment is obtained by integrating $n(y)$ from y to infinity. Hence,

$$m(y) = h \int_y^\infty n(u) du. \quad (8)$$

But $n(y) = Nf(y)$, and $f(y)$ is defined in (1) as $-\beta e^{\beta y}$. Substituting in (8) gives

$$m(y) = h \int_y^\infty Nf(u) du = hNe^{\beta y}. \quad (9)$$

Equation (9) gives the total number of promotion opportunities available to all incumbents of jobs at level y . To obtain the promotion rate per job, it is necessary to divide by the density of jobs at level y . Denote by $q(y)$ the promotion rate that represents the promotion opportunities an incumbent of a job at level y will be exposed to. This quantity will be

$$q(y) = \frac{m(y)}{n(y)}. \quad (10)$$

But $n(y) = Nf(y)$. Hence, from (9) and (1),

$$q(y) = \frac{hNe^{\beta y}}{N[-e^{\beta y}]} = -\frac{h}{\beta}. \quad (11)$$

It follows that, at all levels of attainment, $q(y)$ will be the same, or $q(y) = q$ for all y . The quantity qdt may be conceived of as a promotion probability for persons at a given attainment level: it is the probability that a person may realize a gain in attainment. The derivation has shown that this quantity is a function of h , the rate at which new promotion opportunities are created, and of β that governs the shape of the distribution by determining the proportion of jobs that provide a higher level of attainment. The result is a direct consequence of assuming an exponential distribution of attainments. If some other distribution was assumed, q would not be independent of y , and a much more complicated analysis would have to be undertaken.

It is important to note that q is defined on jobs not on people. While everyone at a given attainment level is exposed to the same q , they are not equally likely to take advantage of it. The extent to which they are able to take advantage of the opportunities represented by q will be argued in the next section to be a function of individual characteristics such as education, ability and background and will be linked to the amount of time already spent in the labor force.

To avoid a proliferation of symbols, q will be in the sequel be taken as equal to $-1/b$ where $b = \beta/h$ —a function of both the shape of the distribution of jobs and the rate at which new vacancies are created.

The Attainment Process

In a structure of inequality characterized by equation (1), it will be the case that at all levels of attainments everyone will be exposed to the same opportunities for increases in attainment as determined by the quantity q of equation (11). This section will address the question of how individual characteristics

⁶ If attainment were to be considered as a discrete variable and the structure of inequality described by the geometric distribution, the parallel formulation would be $m(k) = h(k+1)n(k+1) + m(k+1)$, where k is the discrete variable analog to y . In other words, the number of vacancies arriving at level k equals the number of new vacancies created at level $k+1$ plus the number of vacancies arriving at $k+1$ from above. The conclusions drawn here using continuous y will also hold in the discrete case.

determine a person's ability to take advantage of the opportunities for growth in attainment given by q .

The individual characteristics relevant for a person's attainment will be said to determine a person's resources. These resources are assumed formed by the time a person enters the labor market, and not subject to further change. This is the exact opposite of the assumption made in human capital theory where it is assumed that a person's level of resources (as expressed by his productivity) is changing over time due to on-the-job training, experience and the like. Such additions to a person's resources are measured in empirical investigations of human capital theory by time spent in the labor force. Here, time spent in a labor force will be a measure of how long persons have been exposed to the mobility regime formulated in the preceding section. No claim for the universal validity of the assumption of no change in resources over time can be made, but neither can such a universal claim be made for the validity of the assumption that all changes in attainment are due to changes in resources. Empirical analysis does not necessarily confirm the latter assumption when time is used as a proxy for growth in resources.

The higher the attainment level of a job, the higher the level of resources needed to gain access to a job. It will be assumed further that for a given level of personal resources, there is an attainment level that is the best a person can hope to obtain. This is the case because the distribution of jobs according to attainment levels is fixed; hence, everyone entering at a certain level has to exit in such a way that the distribution is preserved. A job at the highest attainment level possible for given resources should not be left voluntarily by a person, for there is then no gain to be made. Not all people occupy this level, as voluntary moves are assumed possible in the system as defined above because of the creation of new promotion opportunities at each level of attainment. Some people, therefore, are in jobs that provide them with lower attainments than they may hope to obtain. Since every move voluntarily undertaken by a person will produce a gain in attainment, those who

have just entered the labor force will have the lowest attainment relative to their resources. The longer the time a person has spent in the labor force, the more likely it is that the person has the best job (s)he can hope to obtain. Hence a person's ability to take advantage of a vacancy at a higher attainment level will depend on the amount of time spent in the labor force. Time in the labor force, in turn, summarizes all information on the relation between a person's resources and his/her current attainment.

Denote by $r(y,t)dt$ the probability that a person at attainment level y and having spent t years in the labor force will change jobs; i.e., take advantage of a vacancy arriving in dt at his/her current level of attainment. It must be the case that for all people at y , the individual $r(y,t)$'s must sum to the overall rate at which vacancies arrive at y or $q(y)$. The $r(y,t)$'s of persons at y differ only with respect to t ; hence,

$$q(y) = \int_0^{\infty} r(y,t)dt \quad (12)$$

where the integration runs over values of t so that $t \rightarrow \infty$ as the rate of leaving the current attainment level approaches zero for people with attainments commensurate with their resources. It was established in the preceding section that $q(y)$ is a constant and equal to $-1/b$. Hence, (12) implies that

$$\int_0^{\infty} r(y,t)dt = -\frac{1}{b}. \quad (13)$$

The index y may be dropped from $r(y,t)$ since the rate of shift is the same at all attainment levels. The specification of $r(t)$ that is a monotonic function and satisfies (13) is

$$r(t) = e^{bt}. \quad (14)$$

Since $b = \beta/h$, it has been shown that $r(t)$, the rate at which an individual with t years of experience moves to a higher level of job rewards, depends on: (1) h —the rate at which new vacancies are created, (2) β —the parameter that governs the shape of the distribution of attainments and (3) t —the amount of time spent in the labor force.

A person moves to a higher level of attainment through a voluntary job shift. The rate, $r(t)$, of voluntary shifts integrated over t will give the number of shifts a person has undertaken by time t . Denote this quantity $v(t)$, and define it as

$$\begin{aligned} v(t) &= \int_0^t r(u) du \\ &= \frac{1}{b} (e^{bt} - 1) \end{aligned} \quad (15)$$

with a maximum value $v(\infty) = -\frac{1}{b}$, that is, the total number of shifts a person will undertake in his/her lifetime. This quantity can be used to derive the gains in attainments obtained by a person. Denote by $y(0)$ the level of attainment for a person at entry into the labor force, by $y(t)$ the level obtained by time t , and by $y(m)$ the maximum level of attainment possible. The total growth possible is then $y(m) - y(0)$. At each job shift, a person will realize a fraction Δy of this gain. On the average, the gain will be

$$\Delta y = \frac{y(m) - y(0)}{v(\infty)} \quad (16)$$

It will be the case that the level of attainment by time t will be equal to the level at entry plus the gain realized up to this point, or

$$y(t) = y(0) + v(t)\Delta y. \quad (17)$$

Substituting equations (15) and (16) in (17) will give

$$y(t) = y(0) + \left[\frac{1}{b} (e^{bt} - 1) \right] [-b(y(m) - y(0))]. \quad (18)$$

Differentiating gives

$$\begin{aligned} \frac{dy(t)}{dt} &= [-be^{bt}][y(m) - y(0)] \\ &= -b[y(m) - y(t)]. \end{aligned} \quad (19)$$

This is finally the model for change in attainment that obtains in a structure of inequality where mobility takes place in the manner described here.

A person's resources will determine the level $y(m)$ that (s)he eventually will obtain. However, the value of $y(m)$ for the same level of resources will be different in

different opportunity structures, i.e., for different values of b . To reflect this, a slight reformulation of (19) is useful. Define a quantity z through the relation,

$$\frac{dz}{dy(m)} = -b. \quad (20)$$

Let z be defined as a person's resources. It will vary across people but, for each person, be constant over time. From (20) by integration,

$$y(m) = -\frac{z}{b} \quad (21)$$

incorporating the constant of integration into z . Substituting into (19) gives

$$\frac{dy(t)}{dt} = z + by(t). \quad (22)$$

This is the simplest linear differential equation with negative feedback of the dependent variable on itself. The negative feedback has been shown here to be determined by the rate at which new vacancies are created and the shape of the distribution of jobs according to attainment levels.

Equation (22) describes a career line that is concave to the time axis; that is, there will be rapid growth in attainment in the beginning of the career and slower growth later until the attainment reaches the stable level $y(m) = -z/b$. This pattern is found on observed career curves. For an example, see Sørensen (1975:463)⁷

The career line predicted from the model also corresponds to the one predicted from human capital theory. In this theory, the curve is predicted from a pattern of growth in resources where resources grow at a lower rate as people get older, primarily because there is less time left in the labor force in which to recapture costs incurred in acquiring more resources. More specifically, the neo-

⁷ It is shown in Sørensen (1975:463) that the career lines of black males are flatter than those for white males. This presumably reflects the more unfavorable opportunity structure for blacks; that is, $b = \beta/h$ is larger in absolute magnitude for blacks.

classical theory assumes that, at any point in time, the level of attainment is $y(m)$, but the resources, z , change over time in a manner that results in the observed concave career profiles.

Both human capital theory and the theory formulated here predict the same career line. The observed career lines thus do not validate either theory. The objective here was not to prove human capital theory wrong, but to formulate an alternative theory using assumptions that are the opposite assumptions of those used in the economic theory. It would be a poorer theory if it could not account for the same observed career patterns as the human capital theory.

The model developed in this section is of importance both for the interpretation of status attainment research in the tradition created by Blau and Duncan (1967) and for research on intragenerational mobility.

Implications for Status Attainment Research

Research on status attainment usually employs linear equations where the level of attainment, as measured by SEI or prestige scores, is the dependent variable. Characteristics of the individual are employed as independent variables. Typically, they are measures of the respondent's education, father's status, parents' education and other measures of family background. All explanatory variables are then measures of individual characteristics, and no attempt is made to introduce characteristics of the structure of inequality. The model formulated here is derived from consideration of the impact of structural characteristics on growth in attainment, and its parameters are well defined in terms of the various forces that govern attainment processes. The attainment model, therefore, can be used to reinterpret status attainment models and evaluate the appropriateness of the research designs typically employed.

A global measure of resources, z , was used in the derivation of the model above. A formulation of this model that makes it similar to the models employed in status

attainment research is obtained by letting z be a linear function of relevant individual characteristics, or

$$z = c_0 + c_1x_1 + c_2x_2 + \dots + c_nx_n \quad (23)$$

where the x_i variables stand for education, father's status, parents' education, etc. The coefficients to the x_i variables represent the contribution of these variables to the overall level of resources. In status attainment research, as here, these resources are assumed constant over time, although status attainment research has never been explicit about such assumptions. With this expression for z , the model for the process of attainment becomes

$$\frac{dy(t)}{dt} = c_0 + by(t) + c_1x_1 + c_2x_2 + \dots + c_nx_n \quad (24)$$

This model has the solution

$$y(t) = \frac{c_0}{b} (e^{bt} - 1) + e^{bt}y(0) + \frac{c_1}{b} (e^{bt} - 1)x_1 + \frac{c_2}{b} (e^{bt} - 1)x_2 + \dots + \frac{c_n}{b} (e^{bt} - 1)x_n \quad (25)$$

This is one of the most important equations estimated, in status attainment research, as it relates current status of a respondent to the status of first job and individual resources. Typically, this equation is estimated by pooling all respondents on cross-sectional data.

The use of cross-sectional data assumes that the process is in equilibrium. In terms of the model, this means that $dy(t)/dt = 0$, and the level of achievement is

$$y(m) = -\frac{c_0}{b} - \frac{c_1}{b}x_1 - \frac{c_2}{b}x_2 - \dots - \frac{c_n}{b}x_n \quad (26)$$

where $y(0)$ does not appear. Thus, if the assumption of equilibrium is valid, the typical model that includes the status of first job is misspecified. However, the assumption of the process being in equilibrium is generally not valid for representative samples of a population. Hence, observed coefficients to the x_i variables will be some function of

$$d_1 = \frac{c_1}{b} (e^{bt} - 1). \quad (27)$$

This means that the observed coefficients will be a function of (1) the amount of time respondents have spent in the labor force, (2) the quantity b that measures the opportunities for growth in attainment as determined by both the rate at which vacancies are created and by the shape of the distribution of jobs by attainment level and (3) the contribution c_1 of the variable in question to a person's overall level of resources.

Equation (24) can be used to estimate the various parameters if applied to over-time data (see Coleman, 1968; Sørensen, 1977 for details); but when all respondents are pooled in a cross-sectional design, such identification is not possible.

It should be noted that the dependency of d_1 on both time and b is such that the older the respondent and the more favorable the opportunity structure, the larger the magnitude of the effects of x_1 variables. Therefore, one should expect that the effect of a major determinant of resources such as education should have an observed effect on status that increases with time in the labor force. Such a pattern can indeed be found on life-history data (Sørensen, 1977). Further, it is expected that if blacks are assumed to be exposed to a more unfavorable opportunity structure than whites, observed status returns to education should be lower for blacks than for whites. This pattern has been found repeatedly.

Research on the process of stratification and status attainment originated in intergenerational mobility research where the objective of comparing equality of opportunity in different societies and over time has always been a dominant one. Such comparisons could, in the framework of linear models, be carried out by comparing the effect of father's status on son's status observed in different societies or at different time periods. This would amount to estimating the equation

$$y = d_0 + d_1 x_1 \quad (28)$$

where y is the observed status of sons and x_1 is the status of fathers, and comparing d_1 over time or across societies. In the framework of the model proposed here,

this means estimating the equilibrium equation (26) omitting other x_1 variables, so that $d_1 = -c_1/b$.

The coefficient d_1 as a measure of equality of opportunity will confound variation in the contribution of father's status to a son's overall level of resources (c_1) and variation in the opportunity structure (b). Different implications for our understanding of societies depend on whether the contribution of father's status to resources or the opportunity structure are responsible for the variation. In particular, it can be noted that in two societies where parental status is equally important for a person's resources, the society with the most favorable opportunity structure will show the most inequality of opportunity because b will be closer to zero and, hence, d_1 will be larger in absolute magnitude.

Implications for Models of Intragenerational Mobility

Social mobility always has attracted mathematical sociologists as a phenomenon that should lend itself to modeling using stochastic process models. The inherently stochastic nature of the process and the use of discrete occupational categories seem to call for a stochastic process model. Furthermore, mobility tables—showing the number of persons moving among occupations—are readily converted into estimates of transition probabilities of a Markov chain by dividing the row totals into the cell frequencies.

All attempts at testing the simple Markov chain on mobility data, however, has shown that this model does not account for observed movements (for an early example, see Blumen et al., 1955). Numerous reasons have been given for the failure of the model—heterogeneity in the parameters (McFarland, 1970; Spilerman, 1972), duration-specific transitions or cumulative inertia (McGinnis, 1968; Tuma, 1976) and age dependency in the parameters (Mayer, 1972; Sørensen, 1972). The resulting modifications of the Markov Model usually improve the fit of the model. However, the improved fit does not necessarily indicate the validity

of the proposal. Heterogeneity will result in apparent nonstationarity, and vice versa, so that attempts to remedy either problem will improve the fit but not necessarily indicate the true source of failure in the model. Similarly, duration-specific rates and age dependency are difficult to tell apart since age and durations in jobs are highly correlated. Most of the proposals for improving the Markov Model are *ad hoc* proposals that are not based on an explicit theory of the mobility process. Hence, it is not possible to choose among the proposals on theoretical grounds either.

The model for the attainment process proposed here indicates a specific modification of the simple Markov Model. This modification has been described in another paper (Sørensen, 1975), where an empirical analysis using the model is also carried out. The main result shall be summarized here briefly.

The simple Markov Model can be written (cf. Singer and Spilerman, 1976)

$$P(t) = P(0)e^{\lambda(M-I)t} \quad (29)$$

where $P(t)$ is a vector giving the distribution of people according to job categories (say occupations) by time t . The matrix M has elements m_{ij} that give the probabilities of moving from category i to category j , given that a person is in state i ; and I is the identity matrix.

The parameter λ , a scalar, is the rate of job shifts that is assumed constant over time in the simple model. In a system governed by the mobility regime described in this paper, λ will be dependent on time in the labor force, as λ corresponds to the quantity $r(t)$ defined in equation (14). This suggests that a reformulation of equation (29) where λ is dependent on time will be a more adequate representation of the intragenerational mobility process. A particularly simple representation is obtained by redefining time to take into account the decline in $\lambda(t)$ with time.

The desired redefinition of time should be such that, in the new time scale, the rate of job shift is constant over time; that is, job shifts follow a Poisson process. It still may be the case that the rate of shift will show variation among people; that is,

heterogeneity will be present. However, removing the nonstationarity also will remove much of the apparent heterogeneity. In addition, the decline in the rate of job shift by time in the labor force was shown above to be generated by a reduction of the discrepancy between current attainment and the maximum attainment to be obtained. The latter quantity is determined by a person's resources. Hence, the time dependency in the rate indirectly captures important sources of variation among people.

The redefinition of time is obtained easily by defining a new time scale as the number of opportunities for shifts a person has encountered after t years in the labor force. The number of opportunities is captured by the quantity $v(t)$ defined in equation (15).

Assuming the validity of the model, the rate of shift in time scale $v(t)$ will be time independent. Denote this rate of shift λ^* . This quantity, in fact, will be 1, if it is assumed that people only shift to obtain gains in attainment. If voluntary shifts for other purposes are allowed, a value of λ^* different from 1 will be observed.

With this time transformation, the Markov Model can be written

$$P(v) = e^{\lambda^*(M-I)v} \quad (30)$$

assuming $P(0) = I$; and if the time transformation indeed removes time dependency in the rate of shift, a more realistic model is obtained.

A test of the proposed model for the dependency of the rate of job shifts on time in labor force can be obtained using life-history data that give information on the completed durations of each job. The completed durations are the waiting times between events and, if events follow a Poisson process, in $v(t)$ waiting times will be exponentially distributed with a mean that will estimate the inverse of the rate. Transforming the completed observed duration into time scale $v(t)$, therefore, should produce exponentially distributed durations with means independent of time in the labor force. A test of the time transformation using this property was found to be quite satisfactory. A slight departure from the expected pattern could be ex-

plained as resulting from a change in the opportunity structure in the period where these job shifts took place. This change in opportunity structure is reflected in a decrease in the parameter b that governs the time transformation. It was shown further that the change in opportunity structure favored whites more than blacks (Sørensen, 1975:458).

The proposed attainment model not only leads to a more empirically adequate stochastic model of mobility but also points to substantively meaningful analysis. The results summarized here, particularly the successful removal of time dependency in rates of shift using the model, in turn lend support to the model proposed in this paper.

Conclusion

This paper has proposed a model for the process of attaining income, status and other job rewards. The structure of inequality—that is, the distribution of jobs according to attainments—is assumed fixed and not subject to change due to variation in the distribution of personal resources (family background, education, ability) relevant for getting access to jobs. A simple exponential model is assumed for the attainment distribution. In this structure, new vacancies are created in each period of time, and these vacancies represent opportunities for growth in individual attainment. The mobility regime that prevails in such a structure—where persons are entering and leaving the labor force at all attainment levels—was shown to be particularly simple. It is further assumed that an individual's ability to take advantage of the opportunities for attainment gains is dependent on his/her current attainment relative to the maximum level of attainment (s)he will be able to obtain given his/her resources. These resources are assumed to remain unchanged after entry into the labor force. From these assumptions, a simple linear differential equation model is derived for change in achievement over time.

The theory proposed here is derived explicitly on assumptions that are contrary to those used in human capital theory. There, change in attainments after

entry into the labor market are assumed to reflect changes in personal resources due to on-the-job training, experience and the like. In human capital theory, a competitive market for skills is assumed to exist with no imperfections producing individual attainment increases without increases in resources (productivity). It is a consequence of this theory that the structure of inequality will reflect the distribution of individual resources as the supply of people at various levels of resources will affect the returns (attainments) obtained, assuming a given demand schedule. Changes in the distribution of resources thus will change the structure of inequality.

Assuming attainment changes are produced by the creation of vacancies in a predetermined structure of inequality is consistent with the observed stability of the income distribution since World War II despite a marked change in the distribution of education—a stability that is contrary to the implications of human capital theory. In the framework proposed here, changes in the distribution of resources are assumed not to affect the structure of inequality, though resources are crucial for individual attainment. Changes in the distribution of education presumably would change the relative importance of education among the various attributes relevant for attainment, but not the distribution of rewards provided by jobs.

The purpose of the paper has, however, not been to prove human capital theory wrong. Both processes may operate simultaneously, and labor markets may be segmented according to whether one or the other process is dominant. The empirical identification of which mechanism prevails where is a major research task for which the theory proposed here only represents an alternative point of departure to the economic theory.

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COMMENTS

CITY NONDIFFERENCES REVISITED*

(COMMENT ON STOLZENBERG AND
D'AMICO, ASR DECEMBER, 1977)

Stolzenberg and D'Amico's paper in this issue of the *Review* contains errors of conceptualization and methodology. In our opinion, the errors are sufficient to undermine the validity of their conclusions.

I.

Stolzenberg and D'Amico (hereafter SD) review the principal studies regarding the relationship between black occupational status and city characteristics. They note Turner's (1951) conclusion about the relevance of industry structure, Glenn's (1963; 1964) thesis concerning the "overflow" of blacks into upper status occupations where they constitute a large portion in the population, and Cutright's (1965) contention that whites also benefit in occupational standing from residing in a large proportion black community. Finally, they comment on our (Spilerman and Miller, 1976) attempt to assess the importance of these and related community factors for black and white status.

The thrust of SD's argument is to dismiss the foregoing studies as being concerned with a "trivial" amount of variation: "[F]or about three decades, the sociological literature has indicated that there is substantial intercity variation in the race-occupation relationship. . . . In a word, we find that variation in the race-occupation relationship between large metropolitan areas is trivial" (p. 938).¹ Central to their argument is a demonstration that the three-way interaction between race, occupation and city in a log-linear formulation is very small. In particular, SD report that in the non-South² the Index of Dissimilarity between the

observed data and a model based on all two-way interactions is 1.1 percent; also, the amount of variation (X^2) explained by the three-way interaction is only 2.1 percent (7.0 percent in a second formulation). From such results, they conclude that the "intercity variation in occupational differences between the races is not sufficiently important to justify sustained empirical analysis or theoretical explanation" (p. 939).

The error in SD's argument results from equating the three-way interaction in a log-linear model with the propositions in the literature concerning city effects on the occupational standings of the races. The log-linear model, as employed by SD, generates a particular decomposition of the variation into main effects and interactions of different orders. However, the theoretical explanations which SD address—and dismiss—cannot be simply associated with an interaction term of a given order.

To convey the import of this remark, we present hypothetical data in Table 1 such as would result from an "overflow" process as described by Blalock (1957), Glenn (1963; 1964), Cutright (1965) and Spilerman and Miller (1976:2).³ The four cities in the table differ by percentage black, ranging from 28 percent to 75 percent. In city 1, with the smallest black population, all blacks are confined to the lowest occupational category.⁴ In larger percentage black communities, their range of occupa-

analysis of southern SMSAs as well. Similarly, we do not discuss SD's results concerning sex effects because our own research has been limited to males. Our criticisms of methodology, however, apply to this portion of their analysis as well.

³ Glenn (1964:43) writes, "[W]here a large percentage of all workers are Negro . . . [Negroes may] 'overflow' into jobs of intermediate desirability. How 'overflow' could produce a direct relationship between Negro occupational status and percentage Negro can more easily be visualized if one imagines discrimination so complete that all Negro workers are kept below white workers." In summarizing this literature, we noted that white status would be raised because the upper-level positions relinquished to blacks would come, to a disproportionate extent, from the lower strata of this occupational category (Spilerman and Miller 1976:2).

⁴ To facilitate convergence of the ECTA program, each cell in Table 1 contains at least the numerical value 5.

*We are indebted to Robert Hauser and Hal H. Winsborough for comments on an earlier draft.

¹ Unless indicated otherwise, quotations are from Stolzenberg and D'Amico's paper in this issue of the *Review*. Our own study (Spilerman and Miller, 1976), upon which SD base much of their analysis, is unpublished at this writing. Manuscript page numbers are therefore reported when that study is cited.

² Because our study (1976) uses data from the non-South, we discuss figures from only that region. Our comments, however, are applicable to SD's

Table 1. Data Illustrative of the "Overflow" Thesis,^a Four Cities

Occupation	City							
	(1)		(2)		(3)		(4)	
	W	B	W	B	W	B	W	B
1	300	5	500	5	800	5	1000	5
2	400	5	600	5	1400	5	500	700
3	400	5	1100	5	500	1000	5	1200
4	700	5	200	1200	5	2500	5	1400
5	300	800	5	1300	5	2800	5	1300

^a Occupations ordered from (1) high status to (5) low status. Entries are numbers of whites and blacks in each occupational category. The value 5 was added to each cell to facilitate convergence of the ECTA program.

tional options is improved; in city 4, 72 percent of blacks are employed above the lowest occupational rung. This situation, then, is descriptive of the outcome one would expect from a process in which blacks overflow into upper status positions where they constitute a large proportion in the population and relieve whites for even higher status employment.

The validity of the "overflow" thesis is not at issue in this comment; indeed, in our study (1976) we concluded that the effect on black occupational standing is quite small. What is of concern is the ability of SD's formulation to capture the overflow process *when it clearly operates*. To investigate this matter, we performed a log-linear analysis on the data in Table 1, identical to SD's analysis in their Table 1. Our results are similar to SD's findings: the Index of Dissimilarity between the original data and a model containing all two-way interactions equals 1.1 percent. Further, as reported in Table 2, the proportion of X^2 explained by the three-way interaction equals 2.4 percent according to SD's first formulation (CS_1/CS_1),⁵ and 2.8 percent according to their second formulation (CS_2/CS_2). Following SD's reasoning, the variation to be explained is "trivial," hardly worthy of "sustained empirical analysis or theoretical explanation." *Yet, our data were constructed to incorporate a massive "overflow" process.* The reason for this anomaly is that the thesis at issue operates principally through the two-way interactions. SD are incorrect in believing that its import can be judged by an examination of the three-way interaction alone.

⁵ Despite a claim to the contrary (SD, fn 2), the only substantively meaningful baseline model is their second, (Race, SMSA), (SMSA, Occupation). The complete independence model faults the three-way interaction for not explaining the distribution of blacks among cities and the distribution of occupations among cities. For a correct formulation of an identical type of problem, see Hauser et al. (1975:281-4).

It is the case more generally that the hypotheses of city effects on the occupational standings of the races, which SD associate solely with the three-way interaction (and dismiss because this interaction is small), cut across interactions of a particular order in the log-linear formulation. What this literature is about is not narrowly the size of the three-way interaction, but whether it matters in occupational standing for a black man—or a white man—to reside in a community with a particular-sized percentage black population, type of industry structure, or occupational distribution. A large three-way interaction is one way that these factors can be material, but not the only way.

Rather, low-order interactions and main effects tend to be implicated, and this fact is recognized in the literature cited by SD.⁶ Nor are low-order interactions uninteresting for the purpose of theory. Indeed, even were the log-linear analysis to involve primarily main effects, the operative mechanism may be different for each racial group. (An example concerns the impact of proportion in manufacturing on black and white occupational standing. Both groups benefit from a manufacturing concentration, though the causal process involved is different for each [Spilerman and Miller, 1976: Table 2].) Such matters are pertinent to

⁶ In our study (1976), we constructed *separate* models of the determination of black status and white status in order to ascertain the extent to which particular community characteristics affect the standing of each racial group. By this formulation, we were able to identify variables which advantage blacks and whites for identical reasons, variables which advantage both but through different mechanisms, and variables which advantage one racial group but not the other. Also, see Glenn (1963:444) on the reasonableness of assuming an absence of intercity variation in discrimination by whites (i.e., a small three-way interaction) when assessing the impact of city characteristics on the occupational standings of the races.

Table 2. Log-Linear Analysis of Data Illustrative of the "Overflow" Thesis *

Model	Likelihood Ratio Chi-Square Statistic
1. (Race), (Occup.), (SMSA)	24,200
2. (Race, Occup.), (Race, SMSA), (SMSA, Occup.)	586
3. (Race, SMSA), (SMSA, Occup.)	21,191
Ratio of Chi-Squares	
CS ₂ /CS ₁	2.4%
CS ₃ /CS ₁	2.8%

* Data from Table 1.

policy formulation and, in our opinion, a proper concern of theory.

We wish to emphasize that it is not our intention to denigrate the value of a log-linear formulation of these issues. Different methodologies provide alternative perspectives and can lead to greater balance in the understanding of a social process. For this reason, in our study (1976), we employed two conceptual formulations and three index breakpoints to assess the sensitivity of our conclusions to method. With respect to a log-linear formulation, it would be useful to know the extent to which the various explanations in the literature operate through particular main effects, two-way interactions, and the three-way term. SD, however, do not address this important issue and, as we have documented, they have incorrectly associated propositions about city differences in the occupational standings of the races with the three-way interaction.⁷

II.

Having dismissed past research findings, SD ask, "But how do we reconcile the gross differences between our conclusions and theirs" (p. 945). In answering this question, SD commit a second error, this time of a more narrowly methodological kind. SD report that the correlations among the four components of the log of Turner's index all exceed .92. They continue, "Thus, once one knows the number of white men and the number of black men in the ECLF of an SMSA, all the components of the

Turner index are also known. In plain English, this means that city differences in the race-occupation relationship are so small that one can accurately calculate the Turner index for an SMSA merely by knowing the numbers of white and black men in the SMSA's labor force" (p. 945).

Not quite. In Table 3, we present three columns of figures. Think of the first column as containing the number of black men (N_T) in the ECLF of each of ten cities; think of the second column as containing the number of black men in upper status occupations (N_U) in the cities. These data were constructed to illustrate a statistical point, but they are not unreasonable as a description of our cities, although the N s are scaled down for convenience. In particular, the ECLFs in our 1960 Census sample of SMSAs vary in size from 26,000 to 2.96 million, a range of 1 to 113; the corresponding range in black ECLF is even larger, 1 to 285. Thus, the illustrative data do not even exploit the full range of population sizes in the real data. Note, now, that the correlation between $\log N_T$ and $\log N_U$ is very high, .998. In this circumstance, according to SD, "the number of black men in 'good' occupations in an SMSA can be predicted almost exactly from the number of black males in the ECLF of the SMSA" (p. 945).

Now consider a second set of cities with the same total black population figures (column 1), but in which the number of blacks in upper status positions in each city differs from before by exactly 20 percent. Further, we assume that the 20 percent differences are distributed among cities in a manner which greatly reduces the correlation between $\log N_T$ and $\log N_U$ (where N_U is the new number of blacks in upper status occupations), namely the 20-percent changes alternate between an increase and a decrease running over the values in column 2.⁸

⁷ SD's conceptual error is most apparent from the following quote: "To assert that there is intercity variation in racial differences in occupational distribution is equivalent to asserting that . . . if one cross-tabulated workers' race by their occupation by their city of residence, one would observe a three-way interaction" (p. 939). Yet in Table 1, there is considerable intercity variation in racial differences in occupational distribution but only a modest three-way interaction.

⁸ We simulated 20 assignments of plus and minus 20-percent changes in values of N_U among the cities. In five instances, the resulting correlation with column (1) was less than .990, the lowest being .984.

Table 3. Hypothetical Data on Number of Blacks in the ECLF and in Upper Status Occupations in Two Sets of Cities*

City	(1) N_T	(2) N_u	(3) N_{-u}
1	1000	400	480
2	1900	900	720
3	3900	2000	2400
4	6800	4000	3200
5	11000	6000	7200
6	19000	10000	8000
7	28000	13500	16200
8	41000	20000	16000
9	60000	28000	33600
10	89000	42000	33600

* $r(\log N_T, \log N_u) = .998$ (first set of cities). The entries in column (3) were obtained from the corresponding entries in column (2) by increasing or decreasing each value by 20 percent, with the changes alternating in sign. $r(\log N_T, \log N_{-u}) = .990$ (second set of cities).

The resulting figures for N_{-u} are reported in column 3, and the correlation between these entries and those in column 1 equals .990. Thus, we find that a very high correlation between total black population (column 1) and number of upper status blacks is compatible with two very different sets of figures for number of upper status blacks.⁹ Further, corresponding to each 20-percent change we introduced, there is a 20-percent change in each value of N_u/N_T (a principal component of Turner's index), and in Turner's index itself if we assume constancy of the white occupational distribution. In short, SD's claim about predictability of the components of Turner's index from a very high correlation with an ECLF term is in error.

The reason why the correlation between $\log N_T$ and $\log N_u$ is unresponsive to rather substantial changes in the values of the second term is because the ranges in the population figures are very large. In this circumstance, there is considerable room for variation in the individual entries without the correlation being significantly affected. Indeed, it is precisely in this circumstance that standardization is called for. We do not, for example, compare the number of crimes in Madison, Wisconsin with those in Chicago. Instead, we divide by the population at risk and compare crime rates in the two cities. The components of Turner's index used in our analysis (1976)—proportion

of blacks in upper status occupations¹⁰ and proportion of whites in upper status occupations—are rates of exactly this sort, designed to net out the effect of population differences in communities of very different sizes so that sociologically more interesting phenomena can be studied.¹¹

III.

We prefer to conclude on a substantive note. What are some of the city effects on the occupational standings of the races? Based on our causal model (1976: Table 6, 29–32), we find that if a black man were to change cities in a way such that there results a one-standard-deviation increase in the manufacturing proportion, he would find himself in a community in which the occupational standing of blacks is 9.7 percent higher than in the city he left

¹⁰ It is not the case that one can simply replace this ratio formulation by a regression of N_u on N_T (and other variables) as the error term in such a regression will be heteroscedastic. What is the remedy? Divide each term in the equation by N_T ; i.e., create the ratio that we use (Johnston 1963:207–11). Additional comments on the use of ratio variables for the problem we address may be found in our study.

¹¹ Turner's index is a useful measure, though not without its faults. In particular, it is insensitive to distributional differences except at the breakpoint between upper and lower status occupations. To overcome this drawback, we (1976) used multiple cutting points. The fact that Turner's index is responsive to low order interactions and main effects is not a disadvantage for our purposes, since such effects are part of the phenomenon under study. The Index of Dissimilarity—as well as the log-linear model formulated by SD—have a drawback of a different sort: they are insensitive to the hierarchical nature of status.

Because these cases involved irregular patterns of change and because the correlations are not very different, we report the regular pattern in the text.

⁹ Indeed, the correlation remains very high even under a greater manipulation. If a 40 percent change is made to each entry of column (2)—alternating an increase with a decrease—the resulting correlation with column (1) (logged values) is .959.

(Turner's index breakpoint). If he were to migrate to an SMSA larger by one standard deviation in population size, he would be in a community in which 29 percent more blacks are employed in white-collar occupations. In each case, the percentage improvement in white status is negligible.

These are not "trivial" differences with respect to individual opportunity, and the purpose of our analysis was to make clear the mechanisms by which city characteristics influence the occupational standing of blacks. That this is not simply a function of the size of the three-way interaction is evident from our example (Table 1). Despite a small three-way interaction, the occupational prospects of a black man would be considerably better in city 4 than in city 1. Moreover, the operative process is one that we consider to be theoretically interesting.

As a final remark, it is worth clarifying what has been the central research issue in the 30 years of literature referenced by Stolzenberg and D'Amico. The monolithic nature of prejudice and discrimination was well understood by Turner, Cutright, and Glenn. Consider, for example, Glenn's (1963:44) statement: "It is likely that the relative size of the Negro population varies considerably more by locality than does discrimination; therefore, to assume discrimination to be a constant . . . is probably a justifiable simplification of reality." The problem these researchers faced was how to study a phenomenon characterized by a constant! The strategy they adopted was to focus on the *consequences* of monolithic prejudice and discrimination under different structural conditions; i.e., in communities with different characteristics.¹²

We agree with Tukey (1977:72): "Models can—and will—get us in deep trouble if we expect them to tell us what the unique proper questions are."

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¹² We are indebted to Hal Winsborough for an enlightening conversation about theory construction on this topic.

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REPLY TO SPILERMAN AND MILLER

The objections that Spilerman and Miller (S&M) raise in their comment are partly the result of misreadings of the Stolzenberg-D'Amico (S&DA) paper and partly the result of disagreement over methodological and theoretical issues. I address their points in the order in which they raise them.

(1) In the third paragraph of their comment, S&M state that we base our argument on "the amount of variation (X^2) explained by the three-way interaction." This is a misreading of our argument which we took pains to prevent. In particular, we wrote (p. 942):

it is crucial to understand that we are *not* attacking the use of "city" as an independent variable on the ground that it does not explain much variance. Rather, we argue that intercity variation in the race-occupation relationship makes a poor *dependent* variable because it has so little variance to be explained. (*italics in original*).

(2) In their next paragraph, S&M object that:

The log-linear model, as employed by SD, generates a particular decomposition of the variation into main effects and interactions of different orders. However, the theoretical explanations which SD address—and dismiss—cannot be simply associated with an interaction term of a given order.

Here S&M make two errors. First, although they agree that the three-way interaction is small, they do not perceive the consequences of this finding for studies of racial occupational

differentiation. And second, they seem to think that there is something odd or unusual about the "particular" decomposition of covariation (not variation!) generated by the log-linear method. These are the most serious errors in the S&M comment, and I now deal with them.

The consequences of finding a negligible triple interaction term. Our finding of a negligible triple interaction between SMSA, race and occupations has two key implications:

(a) The race-occupation relationship does not vary substantially across SMSAs; the race-SMSA relationship does not vary substantially across occupations; and the occupation-SMSA relationship for whites does not differ materially from the occupation-SMSA relationship for blacks. (b) The occupational distributions of blacks and whites in different cities can be reproduced with tolerable accuracy from the three pair-wise relationships among city, race and occupation. Implication (a) means that stratifying an analysis of the race-occupation relationship according to city of residence will, at best, add *no* information to one's findings, except possibly in the case of anomalous SMSAs. The literature which we criticize is first and foremost a set of stratified analyses in which a summary measure of the race-occupation relationship is computed in each SMSA, and then this measure is related to other characteristics of the SMSA. (Spilerman and Miller's paper is a new variation on this methodology, but not terribly different—they stratify by SMSA separate analyses of white and black occupational achievement.) Implication (b) means that once one has understood the pair-wise relationship between occupation and race, the pair-wise relationship between race and SMSA and the pair-wise relationship between SMSA and occupation, one has understood *implicitly* the occupational distribution of blacks and whites *by SMSA*. Thus, *there is no need to clutter up analyses of the two-way race-occupation relationship by treating this relationship as being possibly different in every SMSA in the country*. To see the advantage of being able to leave "city" out of studies of race effects on occupational achievement, imagine the excruciating complexity involved if Duncan (1968) had stratified his occupational achievement equations by SMSA. Given these implications and lacking any reason to clutter up two-way relationships with extraneous third variables, we suggest that research will be easier, that theory will be more parsimonious and that no damage will be done if efforts are directed toward understanding the three pair-wise relationships among city, race and occupation rather than toward understanding city differences in racial occupa-

tional patterns. In practice, this means that analyses of the pair-wise relationships are sufficient and that one need not perform ecological studies using Turner's index, the index of dissimilarity or other, more complicated aggregate measures. This implication is the very heart of the Stolzenberg-D'Amico paper.

The log-linear decomposition. The decomposition of covariation into main effects and interactions in the log-linear hierarchical model is neither peculiar, nor unusual nor inconsistent with the simplicity-seeking aims of theory construction. Indeed, this decomposition is directly analogous to the decomposition obtained for continuous variables using the analysis of covariance. In most applications, both covariance analysis and log-linear analysis seek simplicity, starting with low-order effects and then adding higher-order interactions as necessary, just as theory-building strategies generally suggest starting with the simplest possible model and complicating it only as necessary. One *could* have theories that are more complicated than necessary, just as one could build models which are composed only of high-order interactions, but the purpose of science is to simplify rather than to complicate, and so simplicity is preferred to complexity, other things being equal. So while it can be distressing to see a complicated theory cut apart by Occam's Razor, the surgery is therapeutic.

At this point it seems relevant to mention a covariance-analytic study of city effects on occupational attainment, which is reported briefly in Mueller (1974). Following Hauser's (1973) methodology, and using Blau and Duncan's OCG data, Mueller found only negligible additive and interactive effects of "city" on occupational attainment. His dissertation also reports an absence of significant interactions between "city" and the effects of race on occupational achievement.

Thus, S&M's most important point—that we have used an inappropriate decomposition of the SMSA-race-occupation relationship—is neither correct in strictly statistical terms, nor consistent with the findings of previous research based on covariance analysis, nor consonant with the simplicity-seeking goals of theory construction.

(3) S&M complain that the log-linear method is insensitive to "overflow" effects. Their support for this complaint is a log-linear analysis of data which they have invented for the purpose of supporting their argument; but I think that they are quibbling here, since they themselves conclude that "overflow" effects in the real world are "quite small" (p. 980).

(4) S&M object that we have missed some-

thing essential in the literature which we criticize:

What this literature is about is not narrowly the size of the three-way interaction, *but whether it matters in occupational standing for a black man—or a white man—to reside in a community with a particular sized percentage black population, type of industry structure, or occupational distribution.* (p. 980, italics mine)

In inviting us to make this interpretation of these studies, S&M invite us to commit the ecological fallacy of making inferences about individual-level occupational achievement from aggregate-level analyses. Mueller's individual-level analysis discussed above avoids this ecological fallacy and is entirely consistent with the argument that racial differences in the occupational attainment process do not vary substantially across cities. More directly, our analysis is an individual level analysis *without problems of ecological correlation* (it cross-classifies men by SMSA, race and occupation). The log-linear triple interaction parameters are directly interpretable as effects of living in a particular city on the black-white difference in the (log) probability of being employed in a given occupation (see Goodman, 1970:228). The chi-square statistics and measures of cases misclassified presented by us indicate that these probability effects can be ignored without doing violence to the observed race-by-occupation-by-SMSA distribution of men.

(5) Next, S&M use hypothetical data to illustrate their argument that "In short, SD's claim about predictability of the components of Turner's index from a very high correlation with an ECLF term is in error." Here, S&M have provided a dramatic, if unnecessary illustration of two well-known properties of regression models. The first point they illustrate is that there are outliers (deviant cases) in regression analysis. We anticipated that there would be outliers, and we speculated about their causes:

We suspect that urban areas which are small enough to have economies dominated by a single firm or a single industry may, as a consequence, have an occupational structure which is marked by the peculiarities of that firm's or that industry's hiring, firing and promotional practices. If these peculiarities include unusually high or low amounts of discrimination, or if they merely create unusual racial differences in occupational distribution without being atypically discriminatory, then places dominated by a single industry or firm will also tend to manifest atypical race-occupation relationships.

The second well-known property of regression which S&M illustrate here is that focusing

only on the deviant cases is likely to blur one's view of the processes affecting the vast majority of cases in the distribution. Spilerman and Miller might have used the standard error of estimate to get a more representative measure of the extent to which the components of the Turner index are accurately estimated from the numbers of white and black men in the experienced civilian labor force (ECLF) of an SMSA.

Where Y is regressed on X , the standard error of estimate is the standard deviation of the difference between the observed values of Y and the values of Y predicted by the regression equation. Thus, where Y is the number of white men in "good" occupations in an SMSA and X is the number of white men in the ECLF of the SMSA, we regress $\log_e Y$ on $\log_e X$, obtaining the equation

$$\log_e Y = a + b (\log_e X) + u \quad (1)$$

where a is the constant term and u is the residual. Using data from non-Southern SMSAs, we found this regression to have an R^2 of .99954 (Stolzenberg and D'Amico, 1977:fn. 6). The standard error of estimate (which is the standard deviation of u) is .02027. Exponentiating both sides of the equation yields

$$Y = e^a X^b e^u, \quad (2)$$

where e is the base of the natural logarithm. Since u is unobservable and has a mean of zero, we would ignore it in making estimates of Y from values of X , using equation (3) below. The circumflex indicates an estimate:

$$\hat{Y} = e^a X^b. \quad (3)$$

Dividing Y by \hat{Y} provides a percentage measure of the inaccuracy of the estimate \hat{Y} . Substituting from (2) and (3), we get

$$\frac{Y}{\hat{Y}} = \frac{e^a X^b e^u}{e^a X^b}. \quad (4)$$

Cancelling terms $Y/\hat{Y} = e^u$. Thus, when u is within one standard deviation of its mean, Y and \hat{Y} differ by only two percent (since $1.02 = e^{0.02027}$). When u is within two standard deviations of its mean, Y and \hat{Y} differ by only four percent ($1.04 = e^{2 \cdot 0.02027}$). Two standard deviations is an unusually large distance from the mean, and four percent seems like a small error. Similar results obtain for estimates of the number of black men in "good" jobs from the size of the black male ECLF. So the components of the Turner index can be computed with acceptable accuracy from the numbers of white and black males in an SMSA's ECLF, contrary to S&M's assertion.

(6) Drawing an analogy to crime statistics, S&M argue for the sensibility of using percentages in analyses of city differences in the race-occupation relationship. I also like the intuitive appeal of percentages, but I recall the well-known difficulties of using percentages or logs of percentages as dependent variables in regression analysis (see Schuessler, 1973; Fuguitt and Lieberman, 1974). The log-linear specification avoids these difficulties with ease (see Goodman, 1970; 1972).

(7) S&M conclude by saying, "We agree with Tukey: 'Models can—and will—get us in deep trouble if we expect them to tell us what the unique proper questions are.' " The quotation is inspiring, but it is irrelevant to the issues at hand. The hypothesis of important city differences in the race-occupation relationship is implicit in some hypotheses which were invented (largely without statistics) about 30 years ago. D'Amico and I looked for the predicted differences and, generally we have found them to be unimportant. So we rejected the hypothesis.

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ITEMS (Continued)

grateful to Edmonston, Hannan and Rosenblum for the high quality of their contribution. Second, to the editorial board. Despite the increasing use of special editorial consultants by the ASR, its associate editors continue to play a central role in determining quality. They only contribute about a third of the evaluations used by the ASR, but the associate editors read an average of 36 manuscripts per editor per year while the remaining evaluations are distributed so widely that the typical editorial consultant reads five or fewer manuscripts in a year. Insofar as there is coherence to the ASR's evaluation process, therefore, it lies with the editorial board. I want to thank them for their contributions, particularly the retiring members of the board, Richard A. Berk, Edna Bonacich, Jonathan R. Cole, Robert M. Hauser, George McCall, Jeffery M. Paige and Andrea Tyree. Third, the editorial consultants of the ASR. Increasingly, the ASR has come to rely on more specialized consultants, chosen for their knowledge of specific methods or subjects. Some 300 editorial consultants contribute about two-thirds of the evaluations used by the ASR. No one of them, obviously, has the effect of a member of the editorial board, but in the aggregate they are of great importance to the quality of the *Review*. They are particularly difficult to thank in a genuine way, because essentially one is thanking a list (see page 991). Perhaps the right way to suggest the importance of their contribution is to note the hours spent building and maintaining the two file cabinets containing their names and specialties. Finally, the editorial staff. The life of an editor depends on the ability of the ASR's managing and copy editors. At the Chicago meetings, everyone asked me if I was glad to give it up. Virtually everyone expected me to say "yes" because of the sheer volume of work involved. But the truth is that I give it up with regret, and the reason is that I had a pretty easy time of it because Phyllis Cairns and Harriet Foster made it that way.

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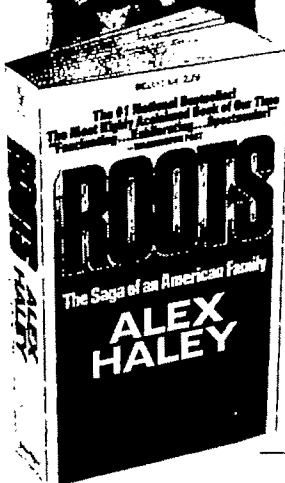
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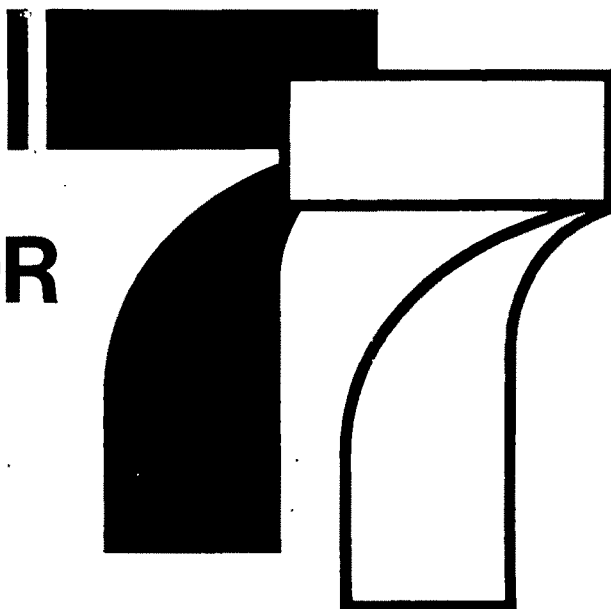
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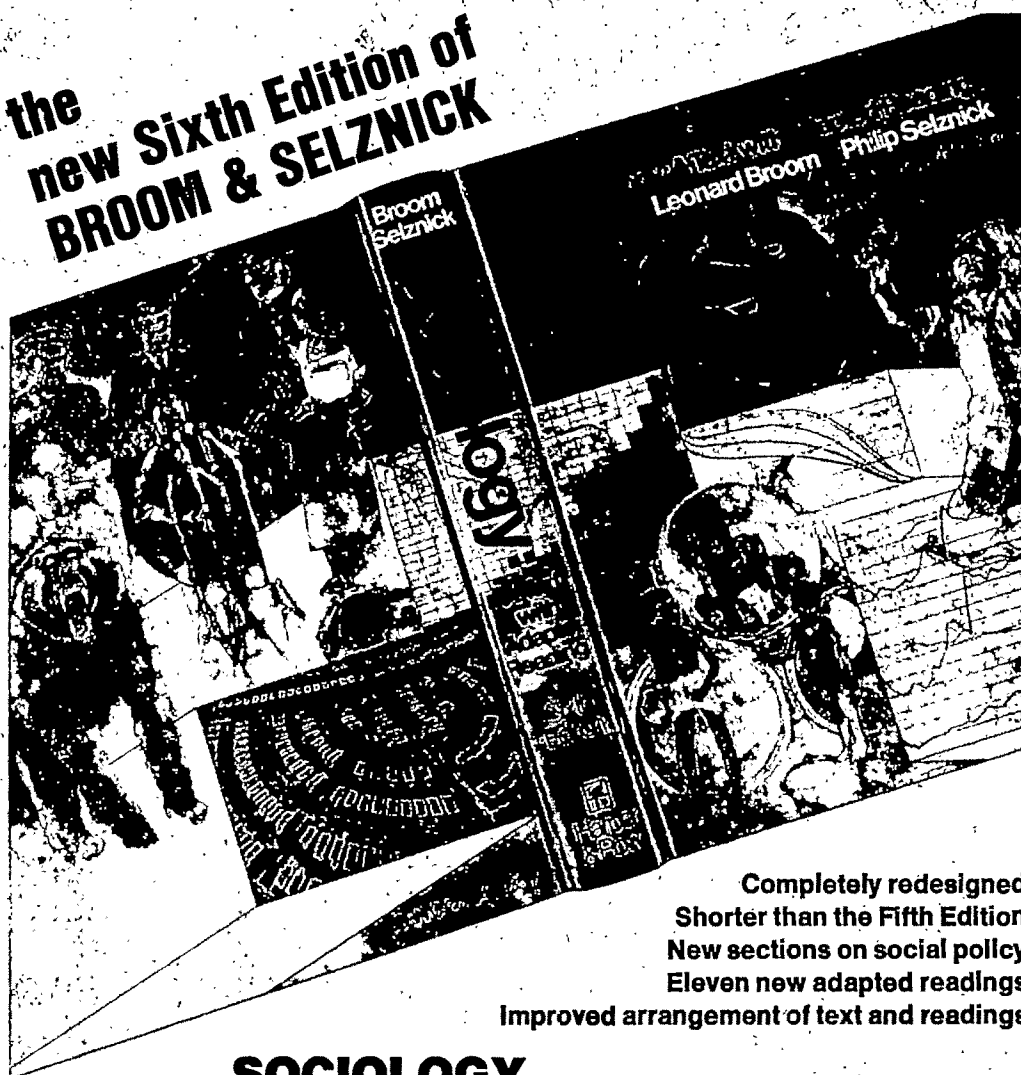
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